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ARTICLES

CHOOSING PREFERENCES BY CONSTRUCTING INSTITUTIONS: A CULTURAL THEORY OF PREFERENCE FORMATION

AARON WILDAVSKY
*University of California
Berkeley*

Preferences come from the most ubiquitous human activity: living with other people. Support for and opposition to different ways of life, the shared values legitimating social relations (here called cultures) are the generators of diverse preferences. After discussing why it is not helpful to conceive of interests as preferences or to dismiss preference formation as external to organized social life, I explain how people are able to develop many preferences from few clues by using their social relations to interrogate their environment. The social filter is the source of preferences. I then argue that culture is a more powerful construct than conceptual rivals: heuristics, schemas, ideologies. Two initial applications—to the ideology of the left-right distinctions and to perceptions of danger—test the claim that this theory of how individuals use political cultures to develop their preferences outperforms the alternatives.

The question of where political ideas come from is not only highly deserving of study, but also within the competence of our contemporary research techniques. I join Bill Riker in commanding it to you as one of the truly exciting and significant areas of investigation in our field.

—Herbert Simon

Agreement on political fundamentals cries for an explanation. Why, how, through which mechanisms do people come to think alike about political fundamentals?

—Charles E. Lindblom

The formation of political preferences ought to be one of the major subjects of political

science. Although it is eminently reasonable to study—as most of us, including myself, have throughout our professional lifetimes—how people try to get what they want through political activity, it is also *unreasonable* to neglect the study of why people want what they want. To omit or slight the most important reason all of us have for studying politics, namely, educating our preferences, is a particularly unfortunate lapse for scholars.

I am making a double argument: first, on behalf of the usefulness of a cultural approach in general (rooting explanation in social life) and, second, on behalf of a particular cultural theory (cultures characterized by boundedness and prescription). Readers might find the first more persuasive than the second. There may be

better formulations. My brief for the cultural theory that follows is based upon the usual criteria of parsimony and power, that is, getting the most explanatory and predictive capacity from the fewest variables. Challenges and improvements are welcome.

Interests As Preferences

Ask political scientists where preferences come from and if they don't just stop the conversation with "haven't a clue" or refer disparagingly to the muddle over ideology, you are likely to hear that ubiquitous catch-all term "interests." Preferences presumably come from the interests people have. Indeed, a sweeping review of the literature done by Michael Thompson and Michiel Schwarz (1985) tells us what we already suspect: politics of interests is the mainstay of political science.¹ Yet, if preferences come from interests, how do people figure out what their interests are (presumably, these do not come with a birth certificate or social security card) so they will know what they prefer? For if interests and preferences are synonymous, we still are no wiser about how people come to have them.

In the beginning, apparently, there were interests—lumpy, fully formed psychological facts, self-evident and self-explanatory. How any one of us would come to know what our interests are need not matter if they are derived from immediate sense perception. Individuals, presumably, size up the situation, distinguish opposing interests, separate the interests of others from self-interest, and choose (or choose not to choose) the self. Instead of this phenomenological understanding—interests are self-evident, chiseled in stone on objects that force themselves as they are upon human perception—I would rely on the convergence of certain strands of work in social science, according to which *meanings are shared*;

they are conferred on objects or events through social interaction.

If the interests that we consider ours are indeed the products of social relations, then the origins of our preferences may be found in the deepest desires of all: how we wish to live with other people and how we wish others to live with us. "The real moment of choosing," as Mary Douglas (1983) maintains, "is . . . choice of comrades and their way of life" (p. 45). But that fateful choice, while broad, is not unlimited.

The first choice—the available combinations of values and practices—is made for us. Human beings do not choose what they want, like ordering a la carte, any more than they (so far) select their body parts in any size or shape they want, regardless of the configuration into which these have to fit. Preference formation is much more like ordering *prix fixe* from a number of set dinners or voting a party ticket. Only those combinations that are socially viable, that can cohere because people are able to give them their allegiance, to share their meanings, may be lived. Some things—accepting authority while rejecting it—just can't be done. Only second-level choices (which of the available ways of life do I prefer?) and third-level choices (which policies do I believe are efficacious in supporting my preferred way?) are potentially available to choice. If preferences are formed through the organization of social relations, however, these preferences must come from inside, not from outside, our ways of life—from institutional arrangements.

Preferences Are Endogenous, Not Exogenous

Ask an economist where preferences come from and you will be told that they are exogenous, external to the system being considered.² The motive force for

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participation in markets, the desire to do better through living a life of bidding and bargaining by competing for resources, is ruled out as a noneconomic question! Worse still, preferences are referred to as "tastes," for which, as the saying goes, there is no accounting, thus rendering them not merely noneconomic but non-analyzable.

The difficulty for economics conceived as rational choice is stated cogently by R. T. Michael and G. S. Becker (1976):

For economists to rest a large part of their theory of choice on differences in tastes is disturbing since they admittedly have no useful theory of the formation of tastes, nor can they rely on a well-developed theory of tastes from any other discipline in the social sciences, since none exists. . . . The weakness in the received theory of choice, then, is the extent to which it relies on differences in tastes to "explain" behavior when it can neither explain how tastes are formed nor predict their effects. (in Burt 1982, 347-48)

Nevertheless, Becker goes on to state that "all human behavior can be viewed as involving participants who maximize their utility from a stable set of preferences" (p. 348). If preferences are fixed and outside the process of choice, then we cannot inquire into how preferences are formed. The least interesting behavior, instrumental actions, may be explained by preferences; but about the most interesting, preferences themselves, nothing at all can be said. Lindblom is right: "We have impoverished our thought by imprisoning it in an unsatisfactory model of preferences taken as given" (1982, 335).

Cultural theory, by contrast, is based on the premise that preferences are endogenous—internal to organizations—so that they emerge from social interaction in defending or opposing different ways of life. When individuals make important decisions, these choices are simultaneously choices of culture—shared values legitimating different patterns of social practices. Always, in cultural theory, shared values and social relations go together: there are no disembodied values apart

from the social relations they rationalize, and there are no social relations in which people do not give reasons for or otherwise attempt to justify their behavior. When choices are not completely controlled by conditions (cultural theory holds), people discover their preferences by evaluating how their past choices have strengthened or weakened (and their future choices might strengthen or weaken) their way of life. Put plainly, people decide for or against existing authority. They construct their culture in the process of decision making. Their continuing reinforcement, modification, and rejection of existing power relationships teaches them what to prefer.

"If political preferences are molded through political experiences, or by political institutions," James March and Johan Olsen (1984) state, "it is awkward to have a theory that presumes preferences are exogenous to the political process" (p. 739). Cultural theory, by contrast, gives preferences an endogenous political explanation: preferences are formed through opposing and supporting institutions.

Rejecting a social science that begins at the end by assuming interests, I wish to make *what people want*—their desires, preferences, values, ideals—into the central subject of our inquiry. By classifying people, their strategies, and their social contexts into the cultural biases that form their preferences, cultural theory attempts to explain and predict recurrent regularities and transitions in their behavior. Preferences in regard to political objects are not external to political life; on the contrary, they constitute the very internal essence, the quintessence of politics: the construction and reconstruction of our lives together.

Deriving Preferences from Cultures: Four Ways of Life

Cultural theory is based on the axiom

Figure 1. Models of Four Cultures

Number and Variety of Prescriptions	Strength of Group Boundaries	
	Weak	Strong
Numerous and varied	Apathy (Fatalism)	Hierarchy (Collectivism)
Few and similar	Competition (Individualism)	Equality (Egalitarianism)

Note: Adapted from Douglas 1970; 1982.

that what matters most to people is their relationships with other people and other people's relationships with them. It follows that the major choice made by people (or, if they are subject to coercion, made for them) is the form of culture—shared values legitimating social practices—they adopt. An act is culturally rational, therefore, if it supports one's way of life.

A basic proposition of this cultural theory (which cannot be demonstrated here) is an impossibility theorem: there are only a limited number of cultures that between them categorize most human relations.³ Though we can imagine an infinite number of potential cultures, only a relatively small number (here I shall work with four) are filled with human activity; the rest are deserted. What makes order possible is that only a few conjunctions of shared values and their corresponding social relations are viable in that they are socially livable.

The dimensions of cultural theory are based on answers to two questions: Who am I? and What shall I do? The question of identity may be answered by saying that individuals belong to a strong group, a collective, that makes decisions binding on all members or that their ties to others are weak in that their choices bind only themselves. The question of action is answered by responding that the individual is subject to many or few prescriptions, a free spirit or a spirit tightly

constrained. The strength or weakness of group boundaries and the numerous or few, varied or similar, prescriptions binding or freeing individuals are the components of their culture.

Strong groups with numerous prescriptions that vary with social roles combine to form hierarchical collectivism. Strong groups whose members follow few prescriptions form an egalitarian culture, a shared life of voluntary consent without coercion or inequality. Competitive individualism joins few prescriptions with weak group boundaries, thereby encouraging ever new combinations. When groups are weak and prescriptions strong—so that decisions are made for them by people on the outside—the controlled culture is fatalistic (See Figure 1).

The social ideal of individualistic cultures is self-regulation. They favor bidding and bargaining in order to reduce the need for authority. They support equal opportunity to compete in order to facilitate arrangements between consenting adults with a minimum of external interference. They seek opportunity to be different, not the chance to be the same, for diminishing social differences would require a central, redistributive authority.

Hierarchy is institutionalized authority. It justifies inequality on grounds that specialization and division of labor enable people to live together with greater harmony and effectiveness than do alternative arrangements. Hence, hierarchies are

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rationalized by a sacrificial ethic: the parts are supposed to sacrifice for the whole.

Committed to a life of purely voluntary association, those from egalitarian cultures reject authority. They can live a life without coercion or authority only by greater equality of condition. Thus egalitarians may be expected to prefer reduction of differences—between races, or income levels, or men and women, parents and children, teachers and students, authorities and citizens.

An apathetic culture arises when people cannot control what happens to them. Because their boundaries are porous but the prescriptions imposed on them are severe, they develop fatalistic feelings: what will be, will be.⁴ There is no point in their having preferences on public policy because what they prefer would not, in any event, matter.

But none of these modes of organizing social life is viable on its own. A competitive culture needs something—the laws of contract—to be above negotiating; hierarchies need something—anarchic individualists, authority-less egalitarians, apathetic fatalists—to sit on top of; egalitarians need something—unfair competition, inequitable hierarchy, non-participant fatalists—to criticize; fatalists require an external source of control to tell them what to do. “What a wonderful place the world would be,” say the adherents of each culture, “if only everyone were like us,” conveniently ignoring that it is only the presence in the world of people who are not like them that enables them to be the way they are. Hence, cultural theory may be distinguished by a necessity theorem: conflict among cultures is a precondition of cultural identity. It is the differences and distances from others that define one’s own cultural identity.

Alone, no one has power over anyone. Power is a social phenomenon; power, therefore, is constituted by culture. But

the form and extent of manipulation vary. Apathetic cultures are manipulated; fatalists live by rules others make and impose upon them. Manipulation is built into hierarchies; orders come down and obedience presumably flows up. The evocative language of New Guinea anthropology (the “big men” versus the “rubbish men”) expresses the growth of manipulation in market cultures as some people cease to possess the resources to regulate their own lives. Egalitarians try to manipulate the other cultures by incessant criticism; they coerce one another by attributing inequalities to corruption and duplicity.⁵

To identify with, to become part of a culture, signifies exactly that: the unviable void of formlessness—where everything and therefore nothing is possible—is replaced by social constraint. Even so, individuals keep testing the constraints, reinforcing them if they prove satisfactory in practice, modifying or rejecting them, when possible, if unsatisfactory. It is individuals as social creatures, not only being molded by but actively molding their social context—shaping the maze as well as running it—that are the focus of cultural theory.

Suppose a new development occurs. Without knowing much about it, those who identify with each particular way of life can guess whether its effect is to increase or decrease social distinctions, impose, avoid, or reject authority—guesses made more definitive by observing what like-minded individuals do.⁶ Of course, people may be, and often are, mistaken. To seek is not necessarily to find a culturally rational course of action. Gramsci’s would-be capitalists may try to establish hegemony over others, but they are often mistaken about which ideas and actions will in fact support their way of life. They may, for instance, use governmental regulation to institute a pattern of cumulative inequalities that convert market arrangements into state capitalism, leading to their ultimate subordina-

tion. To be culturally rational by bolstering one's way of life is the intention, not necessarily the accomplishment.

If social life is the midwife of political preferences, how do people get from culture to preferences? Perhaps politics is too complicated to allow many people to figure out what they prefer.

"Preferences Need No Inferences"

An obstacle to the development of a theory of political preference formation is the view, dominant in psychology until recently, that cognition must precede affect. For if "preferences are formed and expressed only after and only as a result of considerable cognitive activity" (Zajonc 1980, 154), then it would indeed be difficult to explain how most people, including many who engage only in minimal cognitive activity, at least in regard to politics, come to have so many preferences. If, however, one goes along with Zajonc and the considerable literature he cites "that to arouse affect, objects need to be cognized very little—in fact minimally" (p. 154), more promising theoretical avenues open up. Preferences, Zajonc continues, "must be constituted of interactions between some gross object features and internal states of the individual" (p. 159). But how, we may ask, do preferences get from object features to internal states?

The cultural hypothesis is that individuals exert control over each other by institutionalizing the moral judgments justifying their interpersonal relationships so they can be acted upon and accounted for. The prevailing view is that the interrelatedness among attitudes in the mass public is low, that is, people are inconsistent. Now, criteria of consistency expressing what ought to be related to what are not found in nature but, like the categories of culture I am expounding, are imposed in an effort to make sense out of

people's political behavior. If these criteria are incorrectly or insufficiently specified, they will make people's opinions unrelated where another set of criteria would make them more consistent. When there is a question as to whether it is the people who do not understand what they are doing or we social scientists who do not understand the people, I am inclined to think that we have fallen down. All of us in social science are looking for bedrock, for the most basic value and factual premises that we can hypothesize as lying behind specific political and policy preferences. My claim is that this foundation lies in social relationships, roughly as categorized by political cultures.

foundation lies in social relationships, roughly as categorized by political cultures.

How does the social filter enable people who possess only inches of facts to generate miles of preferences? What is it about cultures that makes them the kind of theories that ordinary folk can use to figure out their preferences? The ability of people to know what they prefer without knowing much else lies at the crux of understanding preference formation. Culture codes can be unlocked, I maintain, because its keys are social. By figuring out their master preferences, as it were—who they are and are not, to what groups they do and do not belong—they can readily figure out the rest. A basic reason people are able to develop so many preferences is that they actually do not have to work all that hard. A few positive and negative associations go a long way.

It is no more necessary for a person to verbalize about culture than it is necessary to know the rules of grammar in order to speak. The stock phrases "one of us" versus "one of them" goes a long way. Preferences might come from insight into general principles, but, because meanings have to be shared, ideologues and theorists often discover that their views are

rejected or modified by others. Preferences can and do come sideways, from identifications, experiences, and conversations. What matters is not how preferences are first proposed (many are called but few are chosen) but how they are ultimately disposed through the presence or absence of social validation. It is not the lone individual, after all, who creates what is called *ideological constraint* ("one thing entailing another") among preferences but social interaction among adherents of a particular culture in contrast to other cultures whose identifiers have different preferences.

Heuristics

Brady and Sniderman, in pursuing a closely related question, "How . . . can citizens make sense of groups—that is, know which is relevant to which issue and which stands for what—without having to know a great deal about them?" (1985, 1073),

focus on the operation of an affective calculus, or, as we call it, a *likability heuristic*. This calculus is organized around people's feelings toward groups such as liberals and conservatives. Clearly, many in the mass public lack a firm understanding of political abstractions. All the same, many know whom they like, and, equally important, they also know whom they dislike. If coherent, these likes and dislikes can supply people with an affective calculus to figure out the issue positions of strategic groups. We suggest that in this way many in the mass public can figure out who wants what politically without necessarily knowing a lot about politics. (pp. 1061–62)

The more people are able to choose sides—ours versus theirs—"the more they appreciate the differences between the issue positions of the two sides. What counts, then, is not how people feel toward groups, one by one; rather it is how they feel toward *pairs* of opposing groups" (p. 1075). It is precisely this pairing or, more accurately, this triangulation of rival cultures, I believe, that enables people to position themselves in political life.

Preferences may be rationalized from the top down, specific applications being deduced from general principles. But complexity of the causal chains invoked leaves people who lack a capacity for abstract thought unable to form preferences. Reasoning in steps is also slow. Without social validation at each step, moreover—which is difficult to achieve—the chain of reasoning may snap. Fortunately, faster methods are available. People can know what they believe or whom they trust without knowing how the belief is derived. Sniderman, Hagen, Tetlock, and Brady (1986) agree that such bottom-up processes operate on white attitudes toward blacks. In their view

It may be nearer the mark to say that citizens, so far as their reasoning about policy is affect-driven, start at the beginning of the chain, taking account of their feelings toward blacks. Then, rather than working their way along the chain hierarchically, from general to specific, they skip over the intermediate links of the chain and go straight to its end. Having reached the end of the chain, they work their way backwards and fill in the missing links. That is to say, not only do they reason forwards, from general to specific; they also reason backwards, from specific to general. And, because they can reason both forwards and backwards, with affect guiding them, they can indeed figure out what they think about questions, such as the reasons for racial inequality, they may not ordinarily think about. (p. 33)

Mediating their perceptions through their cultures, people can grab on to any social handle to choose their preferences. All they need are aids to calculation.

"How," Paul Sniderman and his colleagues ask, "do people figure out what they think about political issues, given how little they commonly know about them?" (Sniderman et al. 1986, 2). They state that "three heuristics are of particular importance: affect (likes and dislikes); ideology (liberalism/conservatism); and attributions of responsibility (the so-called desert heuristic)" (p. 2). The desert heuristic is a version of system versus individual blame through which adherents of political cultures seek to hold others

accountable for their behaviors. "Liberal" versus "conservative" stands as a surrogate for equality of condition versus equality of opportunity, that is, for the rivalry of egalitarian and market cultures. (When "liberal" meant "laissez-faire," its cultural associations were different.) The two heuristics—desert and ideology—are related: market forces blame individuals (they are undeserving); egalitarians blame the system (it is oppressive). Liberals dislike conservatives because they "blame the victims," while conservatives dislike liberals because they encourage irresponsible behavior. All these aids to calculation are ideological (or, to use the anthropological term, cosmological) in the sense of rationalizations for preferred social relationships.

I agree entirely that

it would be . . . a mistake merely to enumerate various heuristics; a mistake partly because they are likely to proliferate endlessly; a mistake more fundamentally because it is necessary to understand how these aids to judgment are themselves interrelated. It is, that is to say, necessary, to understand how people work their way, step by step, through a chain of reasoning. And to understand how they manage this, one must establish what they do first, then second, then third. (Sniderman et al. 1986, 47)

Cultural theory attempts to unify heuristics by suggesting that these chains have but one link: the internalization of external social relations.

Schemas

Another entry for understanding the formation of political preferences has now appeared—schema theory. According to Pamela Conover and Stanley Feldman (1984), this theory views "people as 'cognitive misers' who have a limited capacity for dealing with information, and thus must use cues and previously stored knowledge to reach judgments and decisions as accurately and efficiently as possible" (p. 96). Political cognition is about how different schemas, such as

party identification and economic class, organize perception. How are the schemas that form our preferences formed?

Although the logic of schemas may appear similar to that of cultural theory—a small number of premises generating a large number of premises—this appearance is misleading. Cultures are not disembodied ideas; they are not merely cognitive.⁷ The mental activity has a purpose: the justification of desired social practices. It is both together, shared values indissolubly connected to social practices, that make up cultural theory. Comparing cultures means just that—comparing cultures as totalities with values and practices joined, not isolated.

The concept of schemas, essentially a reinvention of our old friend "attitudes" under a new name (like "political behavior" for "political science"), falls prey to the same disability—the endless proliferation of explanatory constructs until there is an attitude or a schema for every act. I think that the notion of schemas lacks a crucial element that cultural theory offers: a systematic context from which preferences can flow. Let us try a couple of quick tests.

Two Tests of Cultural Theory: Ideology and Risk

Cultural theory is open to tests normally applied in social science: *retrodiction* (Can it explain historical puzzles? [Ellis and Wildavsky 1986]) and *prediction* (Does it account for future events better than do other theories?). The degree of incorporation into group life and the degree of prescription can be measured so as to arrive at (forgive the cumbersome expression) intersubjective coder reliability. Jonathan Gross and Steve Rayner's book, *Measuring Culture*, does just that.

One test of cultural theory is conceptual-historical; I contend that the cultural categories described here fit far better in accounting for political prefer-

ences than the usual left-right, liberal-conservative dimensions. A second test is both contemporary and future-oriented; I claim that perception of danger and disposition toward risk—from technology and from acquired immune deficiency syndrome—are better explained and predicted by cultural theory than by competing theories.

A Confusion of Cultures: Competitive Individualism versus Egalitarian Collectivism

The single worst misunderstanding about U.S. politics, in my opinion, is the joining together as a single entity, called "individualism," two separate and distinct political cultures with opposing preferences for policies and institutions—competitive individualism and egalitarian collectivism. Between equality of opportunity (enabling individuals to accentuate their differences) and equality of results (enabling them to diminish their differences), there is a vast gulf.⁸ To say that equal opportunity is empty without more equal results is to say that the latter is more important than the former.

Individualistic cultures prefer minimum authority; just enough to maintain rules for transactions, but they do not reject all authority; if it leaves them alone, they will leave it alone. While egalitarians also like to live a life of minimal prescription, they are part and parcel of collectives in which, so long as they remain members, individuals are bound by group decisions. This critical distinction in group-boundedness, the freedom to transact for yourself with any consenting adult vis-à-vis the requirement of agreement with group decisions, makes for a radical difference in the formation of political preferences.

The confusion to which I am objecting manifests itself more generally in the use

of dichotomous instead of triangular designations of political cultures. The most infamous of these is left versus right. Left, or liberal, presumably designates a tendency toward greater use of central government for policy purposes, including an inclination to welfare state measures designed to be at least somewhat redistributive. Presumably, right, or conservative, signifies a disposition against central governmental intervention in the economy but of greater respect for collective authority. As political shorthand, these terms have their uses. But for purposes of political analysis, they obfuscate more than they clarify. The preference for greater use of government may stem from a hierarchical culture in which the individual is subordinated to the group. Yet the very same preference for central governmental action may be rooted in a desire to reduce all social distinction, including those on which hierarchies are based. Hierarchies and egalitarian collectives may, in certain historical contexts, ally themselves in favor of redistributive measures, yet they may also, at the same time, be bitter opponents in regard to respect for authority. For equalization of statuses would destroy hierarchy. It is not easy, as the Catholic Church is learning, to say that all forms of inequality are bad but that popes and bishops are good (Wildavsky 1985a).

The left-right distinction is beset with contradictions. Hierarchical cultures favor social conservatism, giving government the right to intervene in matters of personal morality. Thus egalitarians may support intervention in the economy to reduce economic differences but not intervention in social life to maintain inequality. Libertarians, who are competitive individualists, oppose both social and economic intervention.

A division of the world into left and right that is equally inapplicable to the past and to the present deserves to be dis-

carded. Efforts to read back the left-right distinction into U.S. history, for instance, succeed only in making a hash of it. In the early days of the republic, egalitarians pursued their objectives through severe restrictions on central government because they then regarded the center as monarchical, that is, hierarchical. Nowadays, after decades of dispute and struggle, they regard the federal government as a potential source for increasing equality. Their egalitarian objectives remain constant, but their beliefs about what will be efficacious instruments of policy vary according to the conditions of the times (cf. Banning 1978).

Without knowledge of the historical context, and therefore, without being privy to the internal discussions through which shared meanings are worked out, it is impossible to explain why a given culture prefers certain institutional arrangements and instruments of policy at one time and different ones on other occasions. How, nowadays, make sense of the Republican alliance of economic free markets and social conservatism or the Democratic combination of statism with distrust of authority? Is it the "left" that supports the authority of central government and the "right" that opposes it, or is it the "right" that respects authority and the "left" that denigrates it?

The division of the political universe into liberals and conservatives, when based on innate tendencies toward change, is bound to be misleading because historical context alters whatever the various political cultures wish to preserve. Given the current extent to which most proposals for government action involve redistribution of income or regulation of business, it is not surprising that people who are opposed to these policies have learned to dislike change. So, when asked, they reply that most change is for the worse. People who prefer these programs respond that they like change. Were the tables turned, so that most legis-

lation was in favor of maintaining social and economic differences, say anti-abortion and anti-inheritance taxes, contemporary liberals would learn that most change is bad and their conservative opponents that change is by and large good.

In a rich analysis of differences and similarities among left- and right-wing activists, McClosky and Chong (1985) conclude that "thus, paradoxically, despite its patriotic fervour, spokesmen of the radical right are profoundly antagonistic to the status quo" (pp. 346-47). It is paradoxical if conservatism is identified with resistance to change but not if desire for change depends on perceived distance from desired behavior. Those who look at life from the conservative perspective "continually lash out against what they consider to be the government's conciliatory stance towards Communism, its support for welfare programmes, (which, in their view, rewards laziness and lack of initiative), its encouragement of moral depravity (sexual license, tolerance of abortion, homosexuality, etc.), and its lenient treatment of criminals" (pp. 346-47). If readers believed that, they might also want big changes. What kind of changes we want depends not nearly so much on our predispositions toward change per se, as if the destination did not matter, but on the gap between desired and actual power relationships.⁹

The further the distance between the real and the ideal, the greater the desire for rapid and radical change. If this proposition is correct, it should follow that "left" or "progressive" forces, when they consider existing power relationships more desirable than proposals for change, should cling to the status quo with as much passion as any reactionary who prefers the last century to the present. Wandering in the void between the Articles of Confederation (interpreted as minimal central authority) and the Constitutional Convention (which, by com-

parison, elevated central power), the anti-federalists preferred the past to the future. Worrying about the return of monarchy or, just as bad, monarchical principles, the individualist Jacksonians (who believed that equality of opportunity, rigorously enforced, would lead to relative equality of condition), fought a rearguard action against commercial capitalism.¹⁰ Similarly, the Federalist party (a hierarchy coalescing with market forces to form an establishment) fought to achieve and maintain the relative centralization of the Constitution—a radical change from the immediate past.

An advantage of cultural theory is that it handles both economic and social issues without strain. Conover and Feldman (1981) wrote that

traditionally, it was assumed that the meaning of ideological labels and self-identifications could be easily summarized in terms of a single dimension: the liberal/conservative continuum. In recent years, however, this viewpoint has undergone some modification. The decade of the 1970s ushered in a variety of "social" issues—abortion, marijuana use, the Equal Rights Amendment—which did not fit easily into the traditional liberal/conservative spectrum. Because of this, many researchers now posit that the meaning of ideological labels and self-identification must be interpreted within the context of two liberal/conservative dimensions: one economic and one social. (p. 168)

Using cultural concepts, however, makes such ad hoc category massage unnecessary. Individualists, being nonprescriptive and anticollectivist, prefer minimal economic and social regulation. Egalitarians, combining nonprescription with collective decision, prefer strong economic but weak social regulation. And adherents of hierarchy, joining hard group boundaries to heavy prescription, desire strong social and economic regulation. Presumably, students of cultural theory would not be surprised at a U.S. president who (combining market individualism with social hierarchy, like his party) urges compulsory urine tests to detect drug users.

Culture and Risk

Comparing perceptions of danger is especially useful as a test of cultural theory. The subject abounds with anomalies; it is fiercely contested; rival theories are already in place; and, best of all, readers can check out the performance of cultural theory vis-à-vis its competitors by reading their daily newspapers.

"Ideology," Samuel Barnes (1966) reminds us, "is one of the most frequently cited and inadequately understood subjects of empirical inquiry" (p. 513).¹¹ In an effort to improve the situation, a number of anthropologists (Claude Levi-Strauss, Clifford Geertz, David Schreider, Ward Goodenough) have brought up the concept of cultures as "ideational codes" (Elkins and Simeon 1979). For Geertz, "culture is best seen not as complexes of concrete behavior patterns—customs, usages, traditions, habit clusters . . . but as a set of control mechanisms—plans, recipes, rules, instructions (what computer engineers call 'programs')—for the governing of behavior" (Elkins and Simeon, 1979, 129). In order to give greater precision to this research program, Elkins and Simeon list a number of questions (regarding people's assumptions about causality, human nature, the "orderliness of the universe") that would get at who is controlling whom or what controlling which. Let us take one of their most explicit questions—"Should one try to maximize gains, or to minimize losses? In other words, what assumptions are made about the relative payoffs of optimistic or pessimistic strategies?" (p. 132)—and compare the kind of answers given by cultural theory with the kind given by other theories of risk-taking and risk-aversion.

In discussions of technological danger, one theory is that people are reacting to the actual dangers; they are risk-averse because the risks are rising. Another theory is psychological; there are risk-taking and risk-averse personalities. Still

another theory concerns an intuitive sense of justice: people are willing to accept dangers that are voluntarily undertaken, but they reject risks that are imposed on them. In *Risk and Culture* (1982), Mary Douglas and I argue that perception of danger is a function of political culture, risk acceptance going along with approval of individualistic and hierarchical cultures and risk aversion with egalitarian opposition to these other cultures on the grounds they are coercive and domineering. Put briefly, we contend that the debate over risk stemming from technology is a referendum on the acceptability of U.S. institutions. The more trust in them, the more risk acceptance; the less trust, the more risk rejection.¹²

Consider, in this context of competing explanations, a variety of survey findings. The first, a survey of the feelings of a variety of elites about the safety of nuclear power plants shows, among other things, an immense gap (far greater than survey research usually produces) between nuclear energy experts (98.7%) and the military (86.0%) saying "safe" compared to relatively tiny proportions of leaders of public interest groups (6.4%), movie producers and directors (14.3%) and elite journalists (29.4%). The difference between people expected to support and to oppose authority is very great (Rothman and Lichter 1985).

A second poll compares the general public to executives of small and large corporations and environmentalists on a variety of preferences related to politics and public policy. Whereas around two-thirds of the general public and executives favor a strong defense, only a quarter of environmentalists give it a high priority. Maintaining order in the nation gets around 80% or more from everyone else but just 47% from environmentalists. On an egalitarian issue, such as having more say at work, the situation is reversed. Two-thirds of environmentalists and the general public give it a high priority but

only a quarter of executives in large companies and two-fifths in small ones do the same (Bloomgarden 1983). Polarization of elites is evident.

A third study surveys business and ecology activists vis-à-vis the general public in West Germany in regard to their political positions on a left-right basis. It is obvious that ecologists and business elites are divided (twice as sharply as the general public) on ideological grounds. Similarly, in a study of voters in the United States Senate, Kalt and Zupan (1984) report, "It turns out that politicians consistently package liberalism and environmentalism together—the correlation between the LCV [League of Conservation Voters]'s and the ADA [Americans for Democratic Action]'s rating scales is 0.94." But why are the two found together?

Nuclear war may well be the greatest contemporary risk of all. Glenn Sussman (1986) has conducted a survey of U.S. and British anti-nuclear weapons activists in which he asked them to rate four goals: fighting rising prices, giving the people more say in important governmental decisions, maintaining order in the nation, and protecting freedom of speech. *A priori* there is no reason to believe that these activists have anything else on their minds except opposition to nuclear war. Yet approximately two-thirds valued more say in government as their first priority while maintaining order got less than 5%.¹³ If one posits a cultural connection between this "anti" activism and opposition to existing authority as egalitarian, the low ranking of "order" makes more sense. Viewing environmentalists as protestors against unequalitarian institutions (recall their concern about "endangered species" and corporations that cause cancer) helps us understand their political alliances. Because Berkeley constitutes a kind of political-medical museum for this purpose, we can observe a member of the city council, accused of

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**Table 1. The Double Polarization in West Germany, 1980:
Left Versus Right**

Respondents	(n)	How would you describe your political attitude?"					
		Strong Left	Middle Left	Center	Middle Right	Strong Right	No Position
Ecology activists	(98)	9%	27%	29%	15%	3%	14%
General public	(1,088)	3	16	39	23	6	12
Business leaders	(130)	1	8	30	49	8	3

Source: Milbrath 1981a, 1981b; Lauber 1983.

spending too much time on foreign affairs instead of local concerns, respond, "You can't explain one without the other. If the money was not going to Central America, we would have the money to fix the sewers."¹⁴

Why, if we are dealing with a reasonable adaptation to emerging knowledge, do attitudes to political authority distinguish so well positions on nuclear power? Why, if there are major personality differences, do ecologists and environmentalists and businessmen divide so neatly on general ideological grounds? Why, if it is the voluntary/involuntary distinction that matters, are there such strong and similar differences on public order and defense? Rooting explanation in adherence to several different ways of life rather than the usual left-right dichotomy, I think, makes more sense out of the data.

A striking contemporary example connecting culture and risk comes from perceptions of acquired immune deficiency syndrome. The more hierarchical the group, I hypothesize, following cultural theory, the more it minimizes technological danger as the price of progress while maximizing fear of casual contact with people who have AIDS. For, in its view, when people violate divine commandments, the Lord brings plague. Conversely, egalitarians tend to grossly overestimate the dangers from technology (on grounds that the social and economic relationships they dislike are bad for your

health) while minimizing the dangers from casual contact with carriers of AIDS. Gays are good in the egalitarian view because they are antiestablishment and because they reduce differences among people. Only cultural theory explains why, when we know a group's general ideology, we can tell how much danger they will impute to technology versus AIDS.

Now this conclusion, which is sure to be contested, depends on a substantial scholarly apparatus. How can laymen, that is, most of us most of the time, figure out what our preferences ought to be?

The Calculation of Preferences

How do people make so much, derive so many preferences, from so few clues? We know that most people are not interested in or knowledgeable about most issues most of the time. Consequently, the clues must be exceedingly simple. Even the highly educated and interested cannot know much about most matters of politics and policy, yet they are able to generate and express preferences when necessary. Indeed, the educated may well be getting more than their due from social scientists. Though they do know more about a few major issues than the less educated, people with high levels of formal education have many more preferences than they can know much, if anything, about. It is

likely, therefore, that the highly educated have many more unfounded preferences than do those who have far fewer preferences about subject matter of which they know little.

Rational people, I have argued, support their way of life. By answering two questions, they are able to discover their cultural identity: Who am I? (free to negotiate or bound by a group?) and What should I do? (follow detailed prescriptions that vary with role or decide for myself?). Knowing who they are and are not—the cultures to which they do and do not belong—helps them to begin sorting their preferences. Cultural identity enables individuals to answer for themselves the crucial quantitative and qualitative questions about preferences: How many are they expected to have? What kind should these be? Fatalists know that they do not need to know anything (it won't matter) except what others tell them to do. They are prescribed to, not prescribing. Members of hierarchies can rule in whatever goes with their station and rule out whatever does not. By relying on others whose duty it is to take care of whatever they neglect and by positive reinforcement of this nonparticipation—it is normative not to act above (or, for that matter, below) your station—both groups come to learn how much of what kind of preferences they are expected to have and how much they can leave to the authorities. Individualists are expected to figure out for themselves whether and to what extent participation is worthwhile. There is no onus on nonparticipation.¹³

Overall, it cannot be too difficult to arrive at preferences on most matters, because everyone does it. Just as we consider our connections with those who advocate petitions as a quick way of determining whether we would feel comfortable in signing, so do people in general learn how to know what they ought to prefer without knowing much about it. People who do not pay much attention to

politics or public policy can nevertheless develop preferences by getting them from Berelson, Lazarsfeld, and McPhee's (1954) well-known ("But," as James Stimson [1986] reminds us, "we keep forgetting it") two-step flow of communication from activists to less attentive citizens. Stimson (1986) shows that "mass perceptions track activist positions." His thesis is that this social connection "accounts for the riddle of inattentive electorates who seem to know much of what they need to know to make policy informed choices." Wholly in the spirit of cultural analysis, Stimson concludes "that many of the things that matter in political life . . . have very little to do with individual psychological processes. They are macro behaviors, such as mediated cognitions, that require for understanding a focus on 'between' rather than 'within' individual effects" (pp. 4, 19, 20). All I would add is that "between . . . individual effects" become "within individual effects."

"System" or "person" blame are dead giveaways. The slightest clue as to whether the authorities and the institutions vis-à-vis individuals are at fault helps people know whether they want to go along with egalitarian or hierarchical or market policies. Anyone who thinks that attribution of blame to "the system" or to individuals is not diagnostic should consult Table 2 from Verba and Orren, in which they address that very question to a variety of elites. The differences could hardly be greater (Wildavsky 1985b).

If it were necessary to go back to the cultural source each time a new preference is involved, building back up to the actual preference through some sort of chain of inference, many people could not manage the complexity; hence there would be far fewer preferences. Consequently, concerted political action would be a rarity. Near universal preference formation requires that preferences be inferred from all possible directions. Culture is the India rubber man of politics, for it permits pref-

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Table 2. Poverty in the United States

Group	Fault of Poor	Fault of System
Business	57%	9%
Labor	15	56
Farm	52	19
Intellectuals	23	44
Media	21	50
Republicans	55	13
Democrats	5	68
Blacks	5	86
Feminists	9	76
Youth	16	61

Source: Verba and Orren 1985, p. 74.

erences to be formed from the slimmest clue. By knowing who or what is involved, the arena or institution of involvement, the subject or object of involvement, people know whether they are supposed to have preferences and what these preferences ought to be.

What is it that enables everyone to come up with reliable solutions to the problem of preference formation whenever it arises? The one source all human beings know something about is their social relations.

Cultures Constitute Our Political Selves

Even when I carry out *scientific* work—an activity which I seldom conduct in association with other men—I perform a *social*, because *human* act. It is not only the material of my activity—like language itself which the thinker uses—which is given to me as a social product. *My own existence* is a social activity.

—Karl Marx

The view of human life as suffused in social relations makes the study of institutions central to political science. To use Elkin's apt expression, "Values are thus 'in' politics, not above or outside it. Hannah Arendt makes the point when she comments that in political activity 'the

end (telos) is not pursued but lies in the activity itself" (1985, 17-18). Michael Oakeshott's (1962) insistence on good form and better manners, his "idioms of conduct," is based on the understanding that the purposes institutions create are expressed in their practices (Elkin 1985, 17-18). Elkin goes on to say that "the institutions are a way in which citizens experience each other and for different institutions the form of experience is different. . . . Political institutions constitute the citizenry in the sense of . . . giving it an organized existence" (pp. 16-17). Wolin (1986) defines democracy as I would a political culture: "Democracy involves more than participation in a political process. It is a way of constituting power" (p. 2). Similarly, Connell and Goot explain that "politics must be invoked not merely as the outcome of political socialization but a cause thereof as well" (Cook 1985).

On the level of ideas, a research program on political culture would seek to increase our understanding of how opposed visions of the good life are selected, sustained, altered, and rejected. As social scientists following Robert Merton and knowing, therefore, that unanticipated consequences are a staple of social life, we want to understand what else we choose when we choose our political cultures.

The Great Depression was a market phenomenon. The great holocaust was perpetrated by a hierarchy (the Nazi party) that tolerated no rivals. The second greatest holocaust was perpetrated by egalitarians (the Cambodian Khmer Rouge) (Jackson n.d.). Deadly visions as well as virtues are also rooted in our public lives. Appraising the consequences of living lives of hierarchical subordination or of the purely voluntary association of egalitarian liberation or of the self-regulation of individualistic cultures, at different times, on different continents, with different technologies, languages, and customs would be a remarkably productive research program. So would comparing cultures rather than countries or, put precisely, comparing countries by contrasting their combinations of cultures. Such a research program would enable us to test the general hypothesis that how people organize their institutions has a more powerful effect on their preferences than any rival explanation—wealth, technology, class, self-interest, tradition, you name it. The field of preference formation is open to all comers.

Notes

This essay is the presidential address presented on August 28, 1986 at the 82nd annual meeting of the American Political Science Association.

1. "Interest explanations are reason explanations. That is, when we explain an action by pointing to the interest that prompted, produced, or motivated it, we allude not to a human cause but to a *reason or ground for acting*" (Ball 1979, 199). Reasons justify our behavior to others.

2. See a perceptive paper by James March for a list of "the properties of tastes as they appear in standard prescriptive theories of choice." His list includes two properties of special interest: "Tastes are *relevant*. Normative theories of choice require that action be taken in terms of tastes." Yet, "tastes are *exogenous*. Normative theories of choice presume that tastes, by whatever process they may be created, are not themselves affected by the choices they control." As March observes, "each of these features of tastes seem inconsistent with observations of choice behavior among individuals and social institutions" (1978). Keith Hartley's paper

"Exogenous Factors in Economic Theory" explains the general perspective, "Utility or preference functions are central to neoclassical economics and are assumed to be given" (1985, 470).

3. Michael Thompson and I are working on a book, *The Foundations of Cultural Theory*, that will attempt to demonstrate this proposition.

4. "Fatalistic attitudes are discernible in many Romanian literary creations, indeed even in folklore. The most famous Romanian folk ballad is 'Miorita,' or 'The Lamb.' It is the moving, beautiful story of a Moldavian shepherd whose fellow shepherds plot to kill him and steal his flock. Learning of the plan from his 'wonder lamb,' the young shepherd makes no move to keep it from being carried out. He serenely accepts his fate, comforted by the thought that he will be reunited with nature" (Shafir 1983, 405).

5. Michael Thompson argues in favor of the viability of his hermit category, a marketlike people who, however, seek subsistence rather than domination, to escape (a) from manipulating others and (b) from being manipulated themselves. I wish them luck (see Thompson 1982).

6. A test of cultural consistency is provided by what March and Harrison call "Postdecision Surprise." When things go badly, the excuses should fit the culture. The market-oriented should accept more personal responsibility than the members of a hierarchy; egalitarians should blame "the system" (see Harrison and March 1984).

7. Many more cultures can be conceived than can be lived in. As Robert Lane says, "Although for every act there is an implicit or explicit belief to justify the act, the reverse is not true; not every thought, fantasy, image, or argument is reflected in behavior, especially since thoughts often rehearse alternative lines of behavior. The world of behavior, therefore, is smaller than the world of thought; the two worlds are not isomorphic" (Lane 1973, 97).

8. Another common confusion is mixing up egalitarianism with exclusive hierarchies because they are both passionate and moralistic in defending their strong group boundaries. The fervor of the exclusive hierarchy comes from its simplicity: only a small number of prescriptions are applicable to the vast diversity of life forms. Deviance (and, hence, deviants) are excoriated. The difference is that whereas egalitarians find society at fault because the distances between people are too large, members of hierarchies believe that the moral spaces are too small. The lack of complexity in small hierarchies means that they are left without sufficient variety for the objects they wish to control. Therefore, they get rid of people who do not fit either by labeling them as deviants or treating them as moral trash, thus removing them from those who deserve to be taken into account in making decisions. The alternative is to co-opt more people by creating moral, and hence social, compartments for them. Using variety to

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cope with complexity (as in Ashby's "Law of Requisite Variety"), however, greatly increases the size (the number and diversity of subunits) of the hierarchy. Once hierarchy is complex, as almost any but the most rudimentary government must be, its fervor declines because of the necessity of accommodating a broad range of values and adjusting relationships among more diverse life-styles.

9. For the reasons given, I disagree with the view that liberals are pro- and conservatives anti-change (see McCloskey and Cheng 1985; and Robinson 1980).

10. The widespread belief among those who theorized about Jacksonian democracy in his time, a belief apparently shared by their supporters in the citizenry as well, was that equality of opportunity, meticulously followed, would lead to an approximation of equality of result. The operation of economic markets, unimpeded by the federal government, would eventually approximate real equality of condition as closely as innate differences in human ability permitted. At the very least, central government would not add artificial to natural inequality, thereby preserving representative government. Individuals would be allowed, indeed encouraged, to keep all gain that resulted from the unfettered use of their own talents. But everything artificial and unnatural, everything government imposed on man in his free state, such as charters, franchises, banks, and other monopolies, became anathema. It is this belief—not in equality undefined nor in just one kind of equality but in the *mutual reinforcement of opportunity and result*—that I think made the United States truly exceptional. Another way to describe U.S. exceptionalism is to say that liberty (i.e., individualism) is held to be compatible with equality (egalitarianism). Just as supporters of hierarchy understand that their organizations are likely to be rigid and, egalitarians recognize that perfect equality is unattainable, so adherents of U.S. individualism understand that liberty can conflict with equality and vice versa. What they deny is that this conflict is immutable, and what they affirm is that their two cherished passions, liberty and equality, can reinforce one another (see Wildavsky 1986).

11. The impasse comes through in these two comments: 1) "There is mounting evidence that mass publics do not react in ideological terms. It seems equally true that much contemporary political conflict has an ideological dimension" (Barnes 1966, 513). 2) "Although the conceptual and methodological problems with ideological belief systems are serious enough for Bennett to have called for a moratorium on empirical research pending the development of better concepts and measures, a 'cottage industry' of comments and rebuttals continues to fill the journals with challenges to each and every piece of research" (Hamill, Lodge, and Blake 1985, 850).

12. Often distinctions are made between active

and passive risk (active risk being more voluntary and controllable by the individual and passive risk less voluntary and perhaps uncontrollable) in order to justify why certain risks are more and other risks less acceptable. But the distinction is misleading. One might imagine a static social system whose values, including its rules of accountability, were petrified. The people who conferred meaning on objects must have lived long ago, no one having come along since with any changes to make. Classifications are clearly labeled and immobile. Then, and only then, might one allocate dangers according to those that are active and voluntary and, therefore, properly subject to governmental regulation or prohibition. Once social change enters the picture, however, the active-passive distinction is constantly redrawn. We now see that egalitarians consider the dangers stemming from technology (nuclear power or chemical carcinogens) as a passive risk, while they perceive the dangers stemming from casual contact with sufferers from acquired immune deficiency syndrome as an active risk. At the very same time, adherents of hierarchy view the dangers of technology as actively chosen. To say a danger is voluntary is tantamount to saying it is acceptable; involuntary dangers imposed on passive people, by contrast, are unacceptable. Classification and decision are one and the same. If the anger against institutions were comprehensive enough, suicides would be owed redress by the implacable institutions that drive them to their undeserved and involuntary end. Just as "we the people" are the ones who confer meaning on these distinctions, so we are also the ones who change these meanings.

13. The actual figures are 1) more say: U.S. citizens 64.5%, British 68.6%; 2) order: U.S. citizens 4.3%, British 2.1%; 3) freedom of speech scored a little over 25%; 4) rising prices were 4% or below.

14. *Oakland Tribune*, 10 August 1986.

15. This discussion of the conception of apathy as a part of cultural bias is congruent with Carole Pateman's view that "there is more than one way to interpret the norm of political efficacy and the other norms and values traditionally associated with democracy; there is more than one view on what 'really' constitutes responsiveness of leaders and so on, and these differences in interpretation also encompass divergent notions of what form[s] of democratic institutions actually embody, or give practical expression to those norms and values" (1971, p. 304).

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WHAT MOVES PUBLIC OPINION?

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Democratic theory must pay attention to what influences public opinion. In this study the content of network television news is shown to account for a high proportion of aggregate changes (from one survey to another) in U.S. citizens' policy preferences. Different news sources have different effects. News commentators (perhaps reflecting elite or national consensus or media biases) have a very strong positive impact, as do experts. Popular presidents tend to have positive effects, while unpopular presidents do not. In contrast, special interest groups tend to have a negative impact.

Public opinion is supposed by some to be the great engine of democracy, determining what governments do. Recent evidence has indicated that public opinion does in fact have substantial proximate effects upon policymaking in the United States (Erikson 1976; Monroe 1979; Page and Shapiro 1983a; Shapiro 1982; Weissberg 1976). The next question, however, is, What moves public opinion? What affects citizens' policy preferences?

The answer makes a great deal of difference. It would be premature to celebrate the triumph of democracy before knowing how and by whom the public is itself influenced. Does the public react directly to objective events, so that opinion is effectively autonomous? Do experts or enlightened political leaders educate the public with helpful new information? Or do demagogues or self-serving elites manipulate opinion with false or misleading propaganda? Which influences are most important: events,

experts, politicians, interest groups? Do the mass media report relevant information accurately or inaccurately?

In this paper we make a start at answering such questions by investigating the impact upon public opinion of the statements and actions of certain actors as reported in the media.

Rational Citizens and the Mass Media

We consider citizens' preferences among alternative public policies to be primarily instrumental. That is, policies are judged in terms of expected costs and benefits for the individual and for his or her family, friends, favored groups, and the nation or world as a whole. But because there is great uncertainty about the effects of policies, the expected utility of a particular policy alternative depends critically upon beliefs about the state of nature, that is, beliefs about present and

future facts and causal relationships (see McCubbins and Page 1984).

Thus new information that modifies relevant beliefs can change the expected utility of policies for citizens. This should occur if five conditions are met: if the information is (1) actually received, (2) understood, (3) clearly relevant to evaluating policies, (4) discrepant with past beliefs, and (5) credible. (For related views of attitude change, see Jaccard 1981; Zaller 1985.)

When these conditions are met to a sufficient extent, new information should alter an individual's preferences and choices among policies. Further, if the conditions are met in the same way for many individuals, there may be a change in collective public opinion that shows up in opinion polls. For example, if many citizens' policy preferences depend critically on the same belief (e.g., "We must spend more on national defense because the Russians are overtaking us") and if highly credible, well publicized new information challenges that belief (e.g., U.S. military spending is reported to rise sharply and a CIA study concludes that Soviet spending has changed little since 1976), then enthusiasm for increased military spending may drop.

Since most people have little reason to invest time or effort learning the ins and outs of alternative policies (Downs 1957), we would not expect new information ordinarily to produce large or quick changes in public opinion. Indeed the evidence indicates that aggregate public opinion about policy is usually quite stable (Page and Shapiro 1982).

By the same token, however, for whatever they do learn about politics, most people must rely heavily upon the cheapest and most accessible sources: newspapers, radio, and television, especially network TV news. When news in the media reaches large audiences and meets our five conditions for many individuals, we would expect public opinion to

change.

Television news often meets the exposure condition. Most U.S. families own television sets, and most tune in to network news broadcasts from time to time. Viewers may wander in and out; they may eat or talk or be distracted by children; but every day millions of U.S. citizens catch at least a glimpse of the major stories on TV news. Others see the same stories in newspaper headlines or get the gist of the news from family and friends. Over a period of weeks and months many bits and pieces of information accumulate.

The conditions of comprehension and relevance, too, are often met. The media work hard to ensure that their audiences can understand. They shorten, sharpen, and simplify stories, and present pictures with strong visual impact so that a reasonably alert grade-schooler can get the point. Often stories bear directly upon beliefs central to the evaluation of public policies.

Credibility is a more complicated matter. Rational citizens must sometimes delegate the analysis or evaluation of information to like-minded, trusted agents (Downs 1957, 203-34). The media report the policy-relevant statements and actions of a wide variety of actors, from popular presidents and respected commentators, to discredited politicians or self-serving interest groups. News from such different sources is likely to have quite a range of salience and credibility, and therefore quite a range of impact on the public (see Hovland and Weiss 1951-52). The analysis of effects on opinion should allow for such variation.

News may also vary greatly in the extent to which it is or is not discrepant with past beliefs. If it closely resembles what has been communicated for many months or years, if it simply reinforces prevalent beliefs and opinions, we would not expect it to produce change. If, on the other hand, credible new information

calls into question key beliefs and opinions held by many people, we would expect changes in public opinion. The extent of discrepancy with past news and past opinions must be taken into account.

We are, of course, aware of the curious notion that the contents of the mass media have only minimal effects (Chaffee 1975; Klapper 1960; Kraus and Davis 1976; McGuire 1985; but cf. Graber 1984; Noelle-Neumann 1973, 1980, 1984; Wagner 1983). This notion seems to have persisted despite findings of agenda-setting effects upon perceptions of what are important problems (Cook, Tyler, Goetz, Gordon, Protess, Leff, and Molotch 1983; Erbring, Goldenberg, and Miller 1980; Funkhauser 1973; Iyengar, Peters, and Kinder 1982; McCombs and Shaw 1972; MacKuen 1981, 1984).

We believe that the minimal effects idea is not correct with respect to policy preferences, either. It has probably escaped refutation because of the failure of researchers to examine collective opinion over substantial periods of time in natural settings and to distinguish among news sources. One-shot quasi-experimental studies (e.g., of presidential debates) understandably fail to find large, quick effects. Cross-sectional studies seek contrasts between media attenders and media "nonattenders" that hardly exist: nearly everyone is exposed either directly or indirectly to what the media broadcast (see Page, Shapiro, and Dempsey 1985a, 2-4). A more appropriate research design yields different results.

Data and Methods

Taking advantage of a unique data set in our possession, we have carried out a quasi-experimental study that overcomes several of the limitations of previous research. The design involved collecting data from many pairs of identically repeated policy preference questions that were asked of national survey samples of

U.S. citizens; coding TV news content from broadcasts aired in between (and just before) each pair of surveys; and predicting or explaining variations in the extent and direction of opinion change by variations in media content.

Our design facilitated causal inferences and permitted comparison across types of issues and historical periods. The use of natural settings meant that all real world processes could come into play, including major events and actions, the interpretation of news by commentators and others, and the dissemination of information through two-step or multiple-step flows and social networks (cf. Katz and Lazarsfeld 1965). The examination of moderately long time periods (several weeks or months) allowed enough time for these natural processes to work and for us to observe even slow cumulative opinion changes. In addition, our measurement scheme permitted us to distinguish among different sources of news and to take into account the extent of news story relevance to policy questions, the degree of discrepancy between current and previous media content, and the credibility of news sources.

As part of our ongoing research project on public opinion and democracy, we have assembled a comprehensive collection of survey data on U.S. citizens' policy preferences. It includes the marginal frequencies of responses to thousands of different policy questions asked by various survey organizations since 1935. Among these data we have identified several hundred questions that were asked two or more times with identical (verbatim) wordings, by the same survey organization. (For a partial description, see Page and Shapiro 1982, 1983a.)

For the present research we selected 80 pairs of policy questions from the last 15 years (for which TV news data are readily available) that were repeated within moderate time intervals averaging about three months.

These 80 cases are not, strictly speaking, a sample from the universe of policy issues or poll questions but (with a small number of exceptions) constitute either a random sample of the available eligible survey questions and time points for a given survey organization or *all* the available cases from an organization. They are very diverse, covering many different kinds of foreign and defense ($n = 32$) and domestic ($n = 48$) policies. In nearly half the cases public opinion changed significantly ($p < .05$; 6 percentage points or more), and in a little more than half, it did not—nearly the same proportion as in our full data set of several hundred repeated items. A list of cases and a more detailed methodological discussion is available in Page, Shapiro, and Dempsey (1985a, b).

The dependent variable for each case is simply the level of public opinion at the time of the second survey (T2), that is, the percentage of the survey sample, excluding "don't know" and "no opinion" responses, that endorsed the most prominent (generally the first) policy alternative mentioned in the survey question. As will be seen, our method of using T2 level of opinion as the dependent variable and including first survey (T1) opinion as a predictor yields nearly identical estimates of media effects as does using a difference score—the magnitude and direction of opinion *change*—as the dependent variable.

For each of the 80 cases, we and our research assistants coded the daily television network news from one randomly selected network (in a few low-salience cases, *all* networks) each day, using the summaries found in the *Television News Index and Abstracts* of the Vanderbilt Television News Archive. These summaries, while rather brief and not intended for such purposes, were generally satisfactory in providing the fairly straightforward information we sought, especially since they were aggregated over

several weeks or months. We coded all news stories that were at least minimally relevant to the wording of each opinion item, beginning two months before the T1 survey—in order to allow for lagged effects and for discrepancies or changes in media content—and continuing with every day up to T1 and through to the date of the T2 survey.

Being interested in the effects of particular actors or *sources*—particular providers of information, or Downsian "agents" of analysis and evaluation—whose rhetoric and actions are reported in the media, we distinguished among the original sources found in each news story. We used 10 exhaustive and mutually exclusive categories: the president; fellow partisans and members of his administration; members of the opposing party; interest groups and individuals not fitting clearly into any of the other categories; experts; network commentators or reporters themselves; friendly (or neutral) foreign nations or individuals; unfriendly foreign states or individuals; courts and judges; and objective conditions or events without clearly identifiable human actors (e.g., unemployment statistics, natural disasters, unattributed terrorist acts).

Our independent variables characterize *reported statements or actions by a specified source*. Each such *source story*, or "message," constitutes a unit of analysis in measuring aggregate media content over the time interval of a particular case. For each reported statement or action by a particular source—each *source story*—we coded the following: 1) its degree of *relevance* to the policy question (indirectly relevant, relevant, or highly relevant); 2) its *salience* in the broadcast (its inclusion in the first story or not, its proximity to the beginning of the broadcast, its duration in seconds); 3) the pro-con *direction* of intended impact of the reported statement or action in relation to the most prominent policy alternative mentioned in the opinion item; 4) the president's pop-

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ularity (measured by the standard Gallup question) as an indication of his *credibility* as news source at the time of his statement or action; and 5) some judgments—not used in this paper—concerning the quality of the information conveyed, including its logic, factuality, and degree of truth or falsehood.

The most important part of the coding effort concerned the directional thrust of reported statements and actions in relation to each opinion question. Proceeding a little differently from the method of our earlier work on newspapers (Page and Shapiro 1983b, 1984), we measured directional thrust in terms of the intentions or advocated positions of the speakers or actors themselves. We took considerable care in training and supervising coders and in checking the reliability of their work. We prepared detailed written instructions and held frequent group discussions of coding rules and the treatment of problematic cases. All pro-con coding decisions, and those on other variables central to our analysis, were validated by a second coder and also by one of the present authors, who made the final coding decisions.¹ We masked the public opinion data so that coders would not be affected in any way by knowledge of whether or how policy preferences changed; we gave them only the exact wording of each opinion item and the time periods to be examined, not the responses to the questions.

As a result of these efforts we are confident that very high quality data were produced. It proved rather easy to code reported statements and actions on a five-point directional scale with categories "clearly pro," "probably pro," "uncertain or neutral," "probably con," and "clearly con" in relation to the main policy alternative outlined in each opinion question.

For each type of news source in each opinion case, we summed and averaged all the numerical values of pro-con codes (ranging from +2 to -2, with 0 for

neutral) in order to compute measures of total and average directional thrust of the news from each source. The sums and averages of directional codes for television news content prior to T1 and between T1 and T2—for all messages coming from all sources combined and for messages coming separately from each distinct source—constitute our main independent variables. Most of our analysis is based on measures restricted to "relevant" or "highly relevant" source stories because we found that inclusion of less relevant source stories weakened the observed relationships.

Our principal mode of analysis was ordinary least squares regression analysis, in which we estimated the impact of each news source (or all sources taken together) along with opinion levels at T1, upon the level of public opinion at T2. We analyzed all cases together and also each of our two independently selected subsets of 40 cases, as well as subsets of cases involving different kinds of issues (e.g., foreign versus domestic policies), different time periods, and different levels of source credibility (popular versus unpopular presidents).

After testing hypotheses and exploring the aggregate data, we closely examined individual cases of public opinion change, scrutinizing media-reported statements and actions and the precise sequence of events. This served two purposes. First, it helped us with causal inference, shedding light on possibilities of spuriousness or reciprocal influence. Second, it enabled us to generate some new hypotheses about effects on opinion by certain sets of actors not clearly differentiated in our aggregate data.

Findings

We have argued that it is not appropriate to lump all media content together as if it came from a single source with a single level of credibility. It will be useful,

Table 1. Total TV News Content and Opinion Change

Variables	Coefficient
Opinion at T1	0.95 ^a (23.25)
TV news content for two months pre-T1	-0.30 ^a (-3.82)
TV news content between T1 and T2	0.11 (1.96)
Constant	0.34 (0.14)
<i>R</i> ² = .88	
adjusted <i>R</i> ² = .88	
<i>n</i> = 80	

Note: Entries are unstandardized (*b*) coefficients from a regression of the percentage level of opinion at the time of the second (T2) survey on the level of opinion at T1 and the total media content variables (sums of pro-con scores from relevant stories) for all 80 cases. *T* values for *b*'s are given in parentheses.

*Significant at the .05 level or better by a two-tailed test.

however, to disregard our own advice for a moment and consider the effects upon public opinion of all TV news messages from all sources added together. In this way we can make clear the form of the relationship and especially the roles of pre-T1 news and of opinion at T1 in affecting the level of opinion at T2.

We regressed the level of opinion at T2 (that is, the percentage of respondents at T2 supporting the most prominent alternative offered in the survey question) upon (1) the level of opinion at T1, (2) the total sum of pro-con scores based on all relevant or highly relevant news stories from all sources combined in the two months before T1, and (3) the total pro-con sum in the T1-T2 period.² The results are displayed in Table 1.

The level of opinion at T1 is a very strong predictor of the level at T2; in fact by itself, it accounted for more than 85% of the variance in T2 opinion. That is to say, on the whole public opinion is quite stable over these periods of up to a few months. The average magnitude of opinion change is about 5 percentage points. There is a simple first-order autoregressive structure in levels of public

opinion (*b* = .95). Thus regressions using the extent of opinion *change* rather than the level of T2 opinion as the dependent variable produce virtually the same coefficients for all the media content independent variables. Our results based on the level of opinion can equally well be interpreted as effects on opinion change.

Of more interest in Table 1 is the substantial negative effect that pre-T1 news had upon opinion at T2. A net sum of one "probably pro" story before T1 is associated with a drop of nearly one third (.30) of a percentage point in opinion at T2. This might seem puzzling at first, but it follows directly from our point that opinion change should depend upon a discrepancy or change in media content, given that opinion change is partly temporary.

If, for example, the TV news for several months before T1 was full of stories favorable toward a particular policy, so that opinion moved strongly in a pro direction before T1, and if the media were then utterly silent about the policy between T1 and T2, we would expect support for the policy to drop off as people forgot about or discounted the past news.

Thus opinion at T2 would be negatively related to media content before T1. If the discrepancy process worked in a particularly simple fashion (e.g., if all opinion changes were temporary and lasted exactly one period), we would find identical coefficients of opposite sign on corresponding pre-T1 and T1-T2 media variables, and we could use media content change scores to predict opinion change.

But things are not so simple. Part of the effect of media content is no doubt temporary, but part may last a long time, and some effects may be lagged or delayed. With our two-point time series we cannot precisely estimate lags or decay rates.³ The problem is further complicated by the need to distinguish among news sources, some of which (e.g., commentaries, reports by experts) may have delayed effects and/or unusually slow decays. And the necessity of using T1-T2 periods of varying lengths, not always corresponding to the two-month pre-T1 period, unavoidably reduces the precision of estimating T1-T2 effects. Under these circumstances, our method of entering both pre-T1 and T1-T2 variables separately into regressions allows the maximum information to be extracted from the data.

We have elsewhere noted an interesting "falling off" effect in the polls that is closely related to the negative coefficient for pre-T1 news (Page and Shapiro 1983b; Page, Shapiro, and Dempsey 1984). It appears that pollsters frequently decide to ask survey questions about a particular policy alternative (often phrased as the first or "pro" alternative in the question) when that alternative is a lively topic in the media and public discussion. Thus an initial poll at T1 may reveal high public support for a newly publicized policy idea. Then those initial effects fade, and news coverage may tend to become more mixed, with doubts and opposition beginning to be heard. By the time of a second survey at T2, public support tends to drop

a bit. We find a small negative opinion change (2.7 percentage points) on the average in our data set.

Another finding in Table 1 is the weak effect of T1-T2 news content. The estimated coefficient is positive but very small and not quite significant at the .05 level.⁴ The logic of our analysis would seem to indicate that T1-T2 variables should have effects of opposite sign and roughly the same magnitude as corresponding pre-T1 variables. But we would not take this nonfinding very seriously. The effects of pre-T1 and T1-T2 media content variables are both estimated to be very small in Table 1 because of the failure to distinguish among different sources of news. If some sources have negative effects and some have positive effects and some have no effects at all, it is not surprising that a measure combining all of them together has little relation to opinion change.⁵

The importance of distinguishing among news sources becomes clearly apparent when we regress opinion at T2 on pre-T1 and T1-T2 news variables from the 10 distinct types of sources (separate sums of pro-con scores for relevant stories from each source). The results are reported in the first column of Table 2.

Taken as a whole, this regression accounts for the great preponderance—more than 90%—of the variance in opinion at T2. Of course much of this is attributable to the effects of opinion at T1, but a comparable analysis with opinion change as the dependent variable still accounts for a very substantial portion of the variance, about half of it ($R^2 = .57$; adjusted $R^2 = .41$). This is quite striking, given the inevitable presence of sampling error in the original surveys and the presumably imperfect media summaries and coding procedures.

Again pre-T1 news tends to have negative effects, that is, opposite to those of corresponding T1-T2 variables (see the top half of the first column of Table 2).



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Table 2. Effects of TV News from Different Sources

Variables	Full Regression	Some Variables Deleted
Opinion at T1	0.97 ^a (23.82)	0.97 ^a (25.95)
Pre-T1 news		
President	-0.47 ^a (-2.43)	-0.42 ^a (-2.48)
Members of president's party	-0.07 (-0.32)	
Opposition party	-0.51 ^a (-2.51)	-0.53 ^a (-2.75)
Interest groups	-0.29 (-1.34)	-0.23 (-1.17)
Events	-0.53 (-0.99)	-0.44 (-0.90)
Commentary	2.16 (1.79)	1.87 (1.66)
Experts	-0.16 (-0.11)	0.00 (0.00)
Foreign—friendly, neutral	0.22 (0.34)	
Foreign—unfriendly	-0.19 (-0.37)	
Courts	1.37 (0.72)	1.77 (1.01)
News between T1 and T2		
President	0.30 (1.34)	0.23 (1.61)
Members of president's party	-0.09 (-0.73)	
Opposition party	0.44 (2.00)	0.46 ^a (2.39)
Interest groups	-0.38 (-1.93)	-0.33 ^a (-2.00)
Events	0.54 (1.27)	0.55 (1.52)
Commentary	4.34 ^a (4.25)	4.17 ^a (4.57)
Experts	3.37 ^a (2.32)	2.85 ^a (2.64)
Foreign—friendly, neutral	0.08 (0.14)	
Foreign—unfriendly	0.48 (0.99)	
Courts	-2.02 ^a (-2.22)	-2.08 ^a (-2.40)

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Table 2 (continued)

Variables	Full Regression	Some Variables Deleted
Constant	-1.34 (-0.56)	-1.11 (-0.49)
$R^2 =$.94	.94
adjusted $R^2 =$.91	.92
$N =$	80	80

Note: Entries are unstandardized (*b*) coefficients from regressions of opinion at T2 on opinion at T1 and the sums of the relevant pro-con news story scores from various sources, for all 80 cases. T values for *b*'s are given in parentheses.

*Significant at the .05 level or better by a two-tailed test.

Most important, however, news from different sources tends to have effects of different magnitudes and sometimes different directions. Source differences are apparent both among the pre-T1 variables and among the more readily interpretable T1-T2 variables, displayed in the bottom half of Table 2. We will focus on the latter,⁶ treating pre-T1 variables as controls.

News commentary (from the anchor-person, reporters in the field, or special commentators) between the T1 and T2 surveys is estimated to have the most dramatic impact. A single "probably pro" commentary is associated with more than four percentage points of opinion change! This is a startling finding, one that we would hesitate to believe except that something similar has now appeared in three separate sets of cases we have analyzed. It was true of editorial columns in our earlier analysis of 56 two-point opinion series using the *New York Times* as our media source (Page and Shapiro 1983b), in the first 40 TV news cases we collected (Page, Shapiro, and Dempsey 1984), and in the 40 new TV cases, which we analyzed separately before doing all 80 cases together.

We are not convinced that commentators' remarks in and of themselves have such great potency, however. They may serve as indicators of elite or public consensus (Hallin 1984; McClosky and Zaller

1984; Noelle-Neumann 1973, 1980). Or the commentaries may—if in basic agreement with official network sentiment or the attitudes of reporters (perhaps providing cues for reporters)—indicate slants or biases in media coverage that are transmitted to citizens in ways that supplement the statements of the commentators. These could include the selection of news sources and quotes, the choice of visual footage, the questions asked in interviews, camera angles, and so forth.

Certain other estimated effects on opinion are probably important even though some do not reach the .05 level of statistical significance according to a conservative two-tailed test.⁸ Most notably—and clearly significantly—a single "probably pro" story about experts or research studies is estimated to produce about three percentage points of opinion change, a very substantial amount. Presidents are estimated to have a more modest impact of about three-tenths of a percentage point per "probably pro" story, and stories about opposition party statements and actions may also have a positive effect.

There are indications, on the other hand, that interest groups and perhaps the courts (in recent years) actually have negative effects. That is, when their statements and actions push in one direction (e.g., when corporations demand sub-

sides or a federal court orders school integration through busing) public opinion tends to move in the opposite direction. We are not certain about the negative effect of courts, however, because of the instability of coefficients across data sets.

Certain kinds of news appear on the average to have no direct effect at all upon opinion, or less impact than might be expected. The president's fellow partisans, when acting independently of the president himself, do not appreciably affect opinion. Events may move public opinion indirectly, but they do not speak strongly for themselves. They presumably have their effects mainly through the interpretations and reactions of other news sources. The same applies to statements and actions from foreign countries or individuals, whether friends or foes. U.S. citizens apparently do not listen to foreigners directly but only through interpretations by U.S. opinion leaders.

The marked distinctions among types of news fits well with our idea that information from different sources has different degrees of credibility. It is quite plausible, for example, that the public tends to place considerable trust in the positions taken by network commentators and (ostensibly) nonpartisan experts. Some other sources may be considered irrelevant. Still others, like certain interest groups that presumably pursue narrowly selfish aims, may serve as negative reference points on public issues (see Schattschneider 1960, 52–53). Similarly, the federal courts may have served as negative referents in the 1970s and the early 1980s because of their unpopular actions on such issues as busing and capital punishment. In any case, it is clearly important to distinguish among sources of news.⁹

In the second column of Table 2, we report the results of a modified regression analysis in which we dropped some variables (party of the president, foreign friends and foes) that had small and

unreliably estimated coefficients in the previous regression. The results are much the same except that most of the coefficients are more stable and the effects of interest groups and opposition party appear statistically significant even by the conservative two-tailed test. News commentary and experts remain the most powerful sources of opinion change.

An interesting finding is that while most of the news variables have pre-T1 coefficients opposite in sign to those for T1-T2—consistently with the discrepancy and temporary effect hypothesis—commentary does not. Commentaries may in fact have lagged positive effects that take time to operate as the commentators' views (or the consensuses or biases they reflect) diffuse through the political system. By the same token, part of the negative effect of interest groups may be a lagged one as well.

For one news source, namely presidents of the United States, we were able to explore the credibility issue directly. We consider a president's popularity—that is, the percentage of U.S. citizens who approve his "handling of his job" according to the Gallup poll—to be a good indicator of the general level of trust and confidence in a particular president. When a president is popular we would expect people to put more faith in what he says and does and to be more prone to change their opinions accordingly. In order to test this hypothesis we partitioned our data into two subsets of cases: one in which, at the time of the T1 survey, the president had an approval rating of 50% or higher ($n = 35$); and the other in which approval was less than 50% (a larger $n = 45$ in the unhappy period studied). We performed the analysis of TV news impact separately for each of these subsets of cases, with the results displayed in Table 3.

When presidents are popular, they tend (though the estimate falls short of statistical significance) to have a small positive effect on public opinion. Each "probably

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Table 3. Presidential Popularity and TV News Effects on Opinion

Variables	When Presidents Were Popular	When Presidents Were Unpopular
Opinion at T1	0.89*	1.00*
	(10.78)	(16.77)
Pre-T1 news		
President	-0.64 (-0.97)	-0.66 (-1.50)
Members of president's party	-0.19 (-0.32)	-0.50 (-0.77)
Opposition party	-0.69 (-1.14)	-0.71 (-1.22)
Interest groups	-1.19 (-1.54)	-0.52 (-1.28)
Events	-3.07 (-1.24)	0.63 (0.70)
Commentary	1.00 (0.51)	1.85 (0.57)
Experts	-3.64 (-1.35)	-4.44 (-1.09)
Foreign—friendly, neutral	0.91 (0.75)	1.86 (1.10)
Foreign—unfriendly	-0.61 (-0.71)	-15.55 (-0.96)
Courts	-2.52 (-0.37)	2.19 (0.76)
News between T1 and T2		
President	0.58 (1.55)	0.05 (0.05)
Members of president's party	-0.41 (-1.79)	0.40 (0.82)
Opposition party	0.84*	0.23 (0.50)
Interest groups	-0.15 (-0.44)	-0.46 (-0.41)
Events	0.53 (0.53)	1.15 (0.60)
Commentary	2.51 (1.56)	6.16 (1.74)
Experts	7.86 (1.46)	6.89* (2.39)
Foreign—friendly, neutral	-2.57 (-1.94)	-0.51 (-0.46)
Foreign—unfriendly	-1.04 (-1.07)	3.78 (0.73)
Courts	0.52 (0.26)	-0.91 (-0.56)

Table 3 (continued)

Variables	When Presidents Were Popular	When Presidents Were Unpopular
Intercept	4.82 (1.02)	-3.72 (-1.01)
R ² =	.97	.95
adjusted R ² =	.93	.91
N =	35	45

Note: Entries are unstandardized (*b*) coefficients from regressions of opinion at T2 on opinion at T1 and the sums of relevant news story pro-con scores from various sources. "Popular" presidents had Gallup poll approval ratings of 50% or more at T1; unpopular presidents had ratings under 50%. T values for *b*'s are given in parentheses.

*Significant at the .05 level or better by a two-tailed test.

"pro" statement or action is estimated to produce more than half a percentage point of opinion change. Part of the effect is undoubtedly temporary and part reciprocal. The impact presumably could not be multiplied indefinitely by talkative presidents because of potential saturation and overexposure and the reporters' and editors' desires for fresh topics to cover. Still, this constitutes some evidence that a popular president does indeed stand at a "bully pulpit." On an issue of great importance to him he can hammer away with repeated speeches and statements and can reasonably expect to achieve a 5 or 10 percentage point change in public opinion over the course of several months (see Page and Shapiro 1984).

Unpopular presidents, in contrast, apparently have no positive effect on opinion at all. They may try—like Glendower in *Henry IV*—to call spirits from the vasty deep, but none will come.¹⁰

There are some indications that the effects of other news sources interact with presidential popularity. While the full set of possible first-order interactions is too complicated to model with confidence given the number of cases we have, these separate popular and unpopular president regressions indicate that commentaries may have their strongest effects when

presidents are unpopular. Perhaps news commentators substitute for a respected leader, challenging the one that is out of favor. In addition, administration officials and the president's fellow partisans in Congress and elsewhere, when acting independently of a popular president, appear to have a slightly negative impact on opinion, whereas they may have positive effects when presidents are unpopular. The opposition party, rather strangely, seems especially potent when presidents are popular. In short, there may be some substantial differences in the dynamics of opinion change depending upon whether the president in office at a particular time is popular or not.

Discussion

Our examination of a number of specific cases of opinion change has bolstered our general confidence in the aggregate findings. It has also illuminated certain issues of causal inference and has generated some new hypotheses about further differentiations among different actors or sources of news. Because we have reported on the cases in detail elsewhere (Page, Shapiro, and Dempsey 1985a, b), we will mention only a few important points about particular news sources.

News Commentary

The most dramatic finding in Table 2 is the strong estimated impact of news commentary. Our examination of specific cases provides a number of instances in which the statements of news commentators and reporters clearly parallel opinion change. Examples include Howard K. Smith's praise for Nixon's policies and his criticism of calls for unilateral withdrawal from Vietnam in 1969; various newsmen's support for continued slow withdrawal from Vietnam during 1969-70; commentary favoring conservation and increased production rather than stopping military aid to Israel in order to get cheap oil during 1974-75; Smith's and others' support for more attention to the Arabs during 1974-75 and during 1977-78; Eric Severeid's, David Brinkley's, and Smith's advocacy of campaign contribution limits in 1973; Brinkley's and Smith's backing of stricter wage and price controls during 1972-73; John Chancellor's editorializing on the importance of fighting unemployment (versus inflation) in 1976; Smith's support for federal work projects in 1976; and commentaries in the spring of 1981 that Reagan's proposed tax cuts would benefit the wealthy.

Our regression estimate of very large commentary effects, controlling for all other news sources, indicates that something substantial was going on. We cannot entirely rule out reciprocal effects of public opinion upon audience-seeking reporters and media, but in many cases, the timing of news commentary shortly after T1 polls indicates some kind of genuine influence upon opinion at T2.

The exact nature of that influence is harder to judge, however. We would not claim that individual news commentators like Howard K. Smith—for all the esteem in which they are held—are, in themselves, the biggest sources of opinion change (but cf. Freeman, Weeks, and Wertheimer 1955). We do not believe that

Walter Cronkite single-handedly ended the Vietnam War with his famous soul-searching broadcast in 1968.

Instead, the commentary we have examined may reflect the positions of many journalists or other elites who communicate through additional channels besides TV news or even a widespread elite consensus in the country (see McClosky and Zaller 1984). Or commentators' positions may be indicators of network biases, including subtle influences of reporters and editors upon the selection of news sources and upon the ways in which stories are filmed and reported. Or, again, commentators and other sources with whom they agree may (correctly or not) be perceived by the public as reflecting a climate of opinion or an emerging national consensus on an issue, which may weigh heavily with citizens as they form their own opinions (see Lippmann 1922; Noelle-Neumann 1973). With our present data, we cannot distinguish among these possibilities. But news commentators either constitute or stand for major influences on public opinion.

Experts

According to our estimates in Table 2, those we have categorized as "experts" have quite a substantial impact on public opinion. Their credibility may be high because of their actual or portrayed experience and expertise and nonpartisan status. It is not unreasonable for members of the public to give great weight to experts' statements and positions, particularly when complex technical questions affect the merits of policy alternatives.

The existence of a reciprocal process, influence by public opinion upon experts, cannot be ruled out (particularly to the extent that the audience-seeking media decide who is an expert based on the popularity of his or her policy views), but it is probably limited in the short run because experts do not face immediate electoral

pressures—that is, public attitudes may ultimately influence who are considered experts and what their basic values are; but once established, experts are less likely than presidents or other elected officials to bend quickly with the winds of opinion.

One striking example of the influence of expert opinion as reported in the media concerns the Senate vote on the SALT II arms limitation treaty. Public support for the treaty dropped 5.5% from February to March 1979 and 19% from June to November. During both periods many retired generals and arms experts spoke out or testified against the treaty, citing difficulties of verification and an allegedly unequal balance of forces favoring the Soviets.

Experts seem also to have been important in building support for the 1981 AWACs sale to Saudi Arabia; in increasing skepticism about Reagan's tax cuts between May and June 1981; in cooling off enthusiasm for a tax on large cars in 1974; and in encouraging support for public financing of political campaigns (1973) and for banning handguns (1981).

We cannot tell from the present data how accurate or inaccurate expert testimony tends to be. There is reason to believe, for example, that the Russian scare of the late 1970s and early 1980s was greatly overblown (Halloran and Gelb 1983; Sanders 1983). Nor can we say much about the possibility that interest groups are important in funding and publicizing favorable expert studies (Saloma 1984) or that the media may be biased in choosing which experts to feature (perhaps favoring, for example, the political tides of the day). Such matters are quite important for any conclusive assessment of the role of the public in democracy, and we plan to pursue them further.

Presidents

As we have seen, public opinion tends to shift somewhat in the direction sup-

ported by a popular president. Our single equation regression analysis, however, cannot by itself exclude the possibility of reverse or reciprocal influence. Rather than leading the public, presidents may sometimes take positions (or make policy) in response to public preferences or in anticipation of future changes in public opinion (see Page and Shapiro 1983a). Popular presidents may be more apt than unpopular ones to try this and to succeed at it, hence, perhaps, their popularity in the first place (which is presumably augmented by taking popular stands).¹¹ Lack- ing continuous survey data between T1 and T2, we cannot be sure that unmeasured opinion did not change before some of the T1-T2 news reports that we have taken as causally prior. Nor, of course, can we be sure that presidents did not anticipate opinion changes.

In this situation our scrutiny of specific cases has been helpful. It has not ruled out causal complexities. On the contrary, we are convinced that the relationship between presidents and public opinion is reciprocal, with each influencing the other (Page and Petracca 1983). But numerous cases support the inference that popular presidents' actions and statements reported in the media do affect public opinion. These include President Nixon's persistent opposition to accelerating U.S. troop withdrawals from Vietnam during 1969, 1970, and 1971; Reagan's 1981 argument for AWACS airplane sales to Saudi Arabia; Carter's 1977-78 increased attention to Arab countries; Carter's early 1980 movement (during a temporary peak in popularity) toward toughness in the Iranian hostage crisis; Reagan's 1982 belligerent posturing toward the Soviet Union; Ford's 1974-75 defense of military spending; Ford's 1976 and Carter's 1980 advocates of cuts in domestic spending; and, perhaps, Nixon's 1972-73 support for wage and price controls.

On the other hand, as our regression results showed, unpopular presidents do not have much success at opinion leader-

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ship. In a number of cases unpopular presidents made serious efforts to advocate policies but failed to persuade the public. This was true of Ford's attempts to increase military spending in 1976 and his resistance to jobs programs and health and education spending in the same year. Jimmy Carter in early 1979, with his popularity at 43% approval and falling, failed to rally support for SALT II. Carter was also unsuccessful at gaining significant ground on gasoline rationing, the military draft, or the Equal Rights Amendment in 1979 and 1980. Even Ronald Reagan, when near a low point of popularity (44%) in mid-1982, failed to move opinion toward more approval of a school prayer amendment to the Constitution. Because this distinction between popular and unpopular presidents emerged clearly in our previous analysis of newspaper data (Page and Shapiro 1984), we are inclined to believe that it is real (though modest in magnitude) even though the popular president effect does not quite reach statistical significance in Table 3.

Interest Groups

Our regression analysis indicated that groups and individuals representing various special interests, taken together, tend to have a negative effect on public opinion. Our examination of the cases supports this point but also suggests that certain kinds of groups may have positive effects while others have negative impact.

We found many cases (more than 20) in which public opinion unequivocally moved *away* from positions advocated by groups and individuals representing special interests. In some cases the groups may have belatedly spoken up after public opinion had already started moving against their positions, producing a spurious negative relationship. But in many instances they seem actually to have antagonized the public and created a genuine adverse effect.

Such cases include Vietnam War protestors from 1969 to 1970, protestors against draft registration in 1980, and perhaps the nuclear freeze movement in 1982. U.S. citizens have a long history of opposition to demonstrators and protestors, even peaceful ones, and apparently tend not to accept them as credible or legitimate sources of opinion leadership.¹²

In general, the public apparently tends to be uninfluenced (or negatively influenced) by the positions of groups whose interests are perceived to be selfish or narrow, while it responds more favorably to groups and individuals thought to be concerned with broadly defined public interests. The best examples of the latter in our data are environmental groups and perhaps also general "public interest" groups like Common Cause.

From 1973 to 1974, for example, support for leasing federal land to oil companies declined as TV news reported conservationists challenging the positions of the profit-seeking and presumably less credible oil companies. During the same period, support for a freeze on gasoline, heating, and power prices increased a bit despite opposition by gas station owners and oil companies.

Not only business corporations, but also some mass membership groups representing blacks, women, the poor, Jews, and organized labor seem to have been held in disrepute¹³ and to have had null or negative effects on opinion about issues of direct concern to them, including social welfare policies and some Middle East issues.

Events, Foreign Countries, and Other Sources

The fact that our regression analysis showed some types of news sources to have, on the average, no clearly positive or negative effects upon public opinion, does not mean that such sources never have effects. As the example of interest

groups suggests, a negligible net effect might conceal offsetting impacts by particular subclasses of sources under particular conditions. If it were feasible to subdivide our 10 source categories further, such effects would presumably be revealed by the statistical analysis.

Among the presidents' fellow partisans, for example, it might be useful to distinguish administration officials from congressional leaders, who may have more independence and a different impact upon public opinion. In the opposition party, too, key congressional leaders and media stars may be more influential than the rank and file.

For foreign actors it would perhaps be useful to distinguish statements (e.g., verbal threats) from actions or policies (e.g., military attacks). It is not easy to code either one so as to correspond to likely opinion change. Certain prominent foreign actors (e.g., Winston Churchill or the Pope) may merit special treatment, and our classification of foreign sources as friendly or unfriendly should perhaps be expanded to allow for finer distinctions.

We initially expected that objective events not attributable to particular actors—anonymous terrorist actions, changes in the rates of unemployment and inflation, natural disasters, and the like—might have substantial effects on public opinion. Contrary to this expectation, our aggregate analysis revealed little or no net direct effect of events. Examination of specific cases indicated that certain kinds of events probably do have appreciable impacts, especially changes in the consumer price index or unemployment rates, which can be felt directly by workers and consumers in their daily lives. Finer distinctions among types of events and allowance for variations in magnitude would permit quantitative estimates of such effects. We have come to believe, however, that much of the impact of objective events is indirect, mediated by U.S. political leaders and commentators

and experts in ways that we have not yet fully untangled.¹⁴ Events—like statements and actions from foreign countries—seldom speak directly and unambiguously to the public; rather they affect public opinion mostly through the interpretations and reactions of U.S. elites.

Conclusion

We believe we have identified the main influences on short-term and medium-term opinion change.

Our analysis does not offer a full account of certain glacial, long-term shifts in public opinion that reflect major social, technological, and demographic changes such as rising educational levels, cohort replacement, racial migration, or alterations in the family or the workplace. The decades-long transformations in public attitudes about civil liberties, civil rights, abortion, and other matters surely rest (at least in an ultimate causal sense) upon such social changes.¹⁵ If news reports play a part in such major opinion shifts, they may do so mainly as transmitters of more fundamental forces.

Within the realm of short- and medium-term effects, however, we have had striking success at finding out what moves public opinion. Our TV news variables, together with opinion at the time of an initial survey, account for well over 90% of the variance in public opinion at the time of a second survey. The news variables alone account for nearly half the variance in opinion change.

This success is especially remarkable because of the many possible sources of error that might be expected to reduce our explanatory power: sampling and measurement error in the original opinion surveys; imprecision in the published news summaries and in our coding scheme; the varying lengths of T₁-T₂ periods and our inability precisely to model lagged effects or decay rates; and the lack of provision for differential audi-

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ence receptivity or different population subgroup effects, not to mention our ignoring of possible opinion influences not reflected in TV news. Improvements in methodology would presumably reduce random error and strengthen the findings still further. But the present analysis already accounts for the bulk of observed change in public opinion concerning policy choices.

The processes of opinion change are not simple. In order to account for changes between two opinion surveys, for example, it is essential to examine media content before the first survey. *Discrepancies* between current news and prior news (or prior opinion) are important. Part of the media impact is temporary so that there is a tendency for opinion in the T1-T2 period to drift back, to move in a direction opposite to the thrust of the media content prior to T1.

Moreover, it is important to distinguish among news sources rather than aggregating all media content together. The effects of news from different sources vary widely.

Among the sources we examined, the estimated impact of news commentary is strongest of all, on a per-story basis, though such messages are aired less frequently than those from other sources. The causal status of this finding, however, is uncertain. Commentary may be an indicator of broader influences, such as media bias in the selection and presentation of other news, of consensus among the U.S. media or elites generally, or of a perceived public consensus.

Experts, those perceived as having experience and technical knowledge and nonpartisan credibility, also have very sizable effects. A policy alternative that experts testify is ineffective or unworkable tends to lose public favor; an alternative hailed as efficient or necessary tends to gain favor.

We found that messages communicated through the media from or about popular

presidents tend to have positive effects on opinion. Presidents respond to public desires, but they can also lead public opinion (see Page and Shapiro 1984). Active presidential effort can be expected to yield a 5- or 10-percentage point change in opinion over the course of a few months.

News commentators, experts, and popular presidents have in common a high level of credibility, which we believe is crucial to their influence on the public. Rational citizens accept information and analysis only from those they trust. In contrast, news sources with low credibility, such as unpopular presidents or groups perceived to represent narrow interests, generally have no effect, or even a negative impact, on public opinion.

Some of these findings might be thought to be limited to the recent period we studied, in which the public has relied heavily on TV and is better educated and more attentive to politics than U.S. citizens in the past. Our confidence in the generality of the findings, however, is bolstered by their consistency with our previous analysis (using newspaper stories) of opinion change from 1935 onward (see Page and Shapiro 1983b, 1984). This similarity also reinforces the observation that the national news media in the U.S. are very much of a piece. They all tend to report the same kinds of messages concerning public policy, from the same sources. This can be attributed to the norms and incentives—and the organizational and market structure—of the news industry and especially to the pervasiveness of the wire services (see Epstein 1973; Gans 1980; Roschco 1975). In this respect the contents of one medium is a good indicator of the content of many media.

In terms of our concerns about democratic theory, it is interesting to observe that relatively neutral information providers like experts and news commentators apparently have more positive

effects (at least direct effects) than do self-serving interest groups. It is also interesting that popular presidents, who presumably tend to embody the values and goals of the public, are more able than unpopular ones to influence opinions about policy. These findings suggest that objective information may play a significant part in opinion formation and change and that certain of the more blatant efforts to manipulate opinion are not successful.

On the other hand, unobtrusive indirect effects by special interests—through influences on experts and commentators, for example—may be more dangerous than would be a direct clash of interests in full public view. Clearly there is much more to be learned before we can be confident about the fundamental sources of influence on public opinion. The same is true of judging the quality of information received by the public.

In order to judge to what extent the public benefits from constructive political leadership and education and to what extent it suffers from deception and manipulation, we need to examine the truth or falsehood, the logic or illogic, of the statements and actions of those who succeed at gaining the public's trust (see Bennett 1983; Edelman 1964; Miliband 1969; Wise 1973; contrast Braestrup 1983; Robinson 1976; Rothman 1979). This applies to the sources whose messages are conveyed through the media and to the media themselves. There is much to learn about whether various sources lie or mislead or tell the truth; about how accurately or inaccurately the media report what the sources say and do; and about the causes of any systematic distortions or biases in the selection and reporting of policy-related news.

Notes

All three authors are associated with NORC (formerly National Opinion Research Center), Chicago. A longer version of this paper was presented at the

1985 annual meeting of the American Political Science Association, New Orleans. We have benefited greatly from the suggestions and comments of Tom Ferguson, Alex Hicks, Henry Brady, Michael MacKuen, Robert Erikson, David Fan, John Zaller, Eleanor Singer, Herbert Gans, Phil Davison, Mathew McCubbins, John Ferejohn, Roger Noll, Barbara Geddes, Gavan Duffy, and especially Garth Taylor. Harpreet Mahajan assisted in preparing the manuscript. We thank the National Science Foundation for research support under Grant No. SES83-08767; the responsibility for analysis and interpretation is our own.

1. This was done by Dempsey for all cases and by Shapiro for some early cases. Shapiro also checked the coding and analyzed the written summaries for several detailed case studies. Any disagreements about coding were resolved through meetings and discussion. Some reliability analysis was done, with Dempsey and Shapiro coding cases independently. Their intercoder reliability coefficients for the variables coded were in the .7 and .8 range. For the all-important pro-con codes, the two authors never disagreed by more than one unit on the 5-point scale.

Coding, verifying, and keypunching the data for the 80 cases took an immense effort. More than 10,000 hours were spent preparing the case level, aggregated data file based on 10,950 source stories (messages or data lines). We are grateful to the following research assistants for their diligent work: Amy Richmond, Karl Mueller, Mandy Kwock, Sasha Held, Joe Torres, Peter McCarthy, Marianne Eismann, Chris Hill, Dan Sakura, Susan Rosenbaum, Kathy Szydagis, Francis Kwakwa, John Kendzior, Mennette Masser, Jim Martin, Lance Selfa, Bill Sullivan, Wayne Arney, Ian Motkin, and Ellen Seebacher.

2. We performed the analysis for *average* media content (mean pro-con codes) as well as for sums. Averages do not account nearly as well for opinion change, however; the volume as well as the directional thrust of news is important. Hence we report the results for sums only.

3. For sophisticated efforts to do so using longer time series see Erbring (1975) and MacKuen (1981, 1983) on agenda setting and Fan (1984, 1985a-c) on a few policy preference items.

4. Given our unusual sampling scheme, the reported significance levels may be taken as referring to a hypothetical universe of similar cases (weighted by survey organization) or as informal indicators of substantive significance. They also provide some protection against erroneous inferences due to random measurement errors in the survey data and media coding. As we will note below, however, the two-tailed *t* test is quite conservative—probably too conservative—given our past work with different data sets.

5. The T1-T2 coefficient is probably reduced

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more than the pre-T1 because of measurement error due to the variation in T1-T2 period length.

6. The interpretation of pre-T1 coefficients is not entirely straightforward. By the logic of a partial adjustment model, they could be considered estimates (with reversed signs) of temporary effects: that is, effects that appear in the T1 opinion measurement but disappear by T2. This interpretation is complicated, however, by the apparent presence of some lagged positive effects of pre-T1 media content on opinion at T2. Because we cannot identify lags or decay rates with our short time series, we cannot sort out these complexities. We have, therefore, chosen to focus our interpretation on the T1-T2 variables (not attempting to distinguish what part of their effects lasts how long), treating pre-T1 factors as controls. We should once again note, however, that the uneven T1-T2 periods introduce measurement error that may depress T1-T2 estimates relative to pre-T1 estimates.

7. We are indebted to Tom Ferguson for this point.

8. Our previous data analyses gave us a priori expectations that would justify a more liberal one-tailed test. By the same token, a macrotest involving all three data sets would indicate that repeated estimates of small coefficients with the same sign were highly unlikely to arise by chance.

9. Fan (1985c) reports substantial media effects upon opinion about defense spending despite all news sources being combined. It is not clear whether special characteristics of the defense issue produce this result or whether it is applicable to other issues.

10. Our thanks to Jim Davis for improving our recollections of Shakespeare.

11. Mark Petracca and Jeffrey Tulis, among others, have emphasized this point.

12. This is not to deny that protestors may indirectly play a role in moving the public and policy makers, for example, against the Vietnam War (see Burstein 1979; Burstein and Freudenberg 1978). Protestors raised the domestic costs of the war and expanded its visibility, and, in the long run, many of their positions met the test of reality and were accepted as correct.

13. Ideally one would gather extensive survey data bearing directly upon the credibility and popularity of such groups at various times and use it explicitly in the analysis. The limited data available support our assessment of the relatively low level of public esteem in recent years for organized labor, special economic interests, and certain liberal groups, as well as the greater credibility of environmental and public interest groups (see Lipset and Schneider 1983).

14. Before we controlled for other sources, events (the T1-T2 pro-con sum) were significantly related to opinion change: a *b* coefficient of 1.11 (*t* = 2.56, *p* < .05), while controlling for pre-T1 events (*b* = -0.80, *t* = -1.44, n.s.). This suggests that exoge-

nous events may have large indirect effects through other sources, effects that vanish when those sources are included in the regression as in Table 2.

15. See, for example, Davis (1975). Page and Shapiro are currently completing a monograph on changes in U.S. public opinion since 1935, which considers long-term social changes as well as media-reported influences.

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RESIDENTIAL MOBILITY AND VOTER TURNOUT

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We examine the characteristics of a largely ignored low-turnout group—people who have recently moved. We find that neither demographic nor attitudinal attributes explain their lower turnout. Instead, the requirement that citizens must register anew after each change in residence constitutes the key stumbling block in the trip to the polls. Since nearly one-third of the nation moves every two years, moving has a large impact on national turnout rates. We offer a proposal to reduce the effect of residential mobility on turnout and estimate that turnout would increase by nine percentage points if the impact of moving could be removed. The partisan consequences of such a change would be marginal.

Fifty-three percent of the voting age population went to the polls in the 1980 general election. In the previous two years, one-third of all adults moved. We investigate the connection between these two facts and show that the turnout rate in the United States reflects in part the low turnout of those who have moved. The relationship of mobility to turnout is particularly interesting because movers are nearly as interested in politics and as fully convinced that they have a say in what the government does as people who have stayed put. Drawing on our research on mobility, we offer a proposal we believe would bring more people to the polls. We find that turnout would increase by nine percentage points if the effect of moving were removed. Finally, we assess the political consequences of reducing the deleterious impact of moving on turnout.

For most U.S. citizens the trip to the polls is a two-step process. First one must

register; then and only then can one vote. Election day is not likely to slip by unnoticed. Lavishly publicized, it is the climax of a long, hard-fought competition, the result of which is widely awaited. Everyone except an absentee votes on the same day; the act itself is a shared experience. None of this is true of registration. Deadlines, requirements, and procedures are shrouded in obscurity. In most states registration is closed in the month before election day, when the excitement and publicity of the campaign reach a peak. It should not be surprising that 31% of all U.S. citizens of voting age failed to register for the 1980 general election nor that this country's dismal showing in international comparisons of turnout is due in large measure to our registration system, in which the individual, not the government, bears the responsibility for establishing one's eligibility to vote.¹

The registration barrier does not impede all sections of society equally, a fact

only partially recognized in the numerous voter registration campaigns aimed at various groups believed to need help in dealing with the barrier. One obvious category is newly eligible young people facing their first opportunity to vote and not familiar with registration procedures. A second and larger group is people not sufficiently interested or skilled to surmount the bureaucratic hurdle of registration. The identifying characteristic of these people is low education; they are usually described as "the poor and minorities."

A third group whose voting is impeded by the registration requirement consists of those who have moved recently. Whenever one moves, whether it is down the street, to a nearby town, or across the country, it is necessary to register anew. As a result, movers (those who moved within two years of election day) had a turnout rate in 1980 just as low as young people or the uneducated. Moreover, the movers greatly outnumber these other groups. In 1980, movers represented 29% of voting age citizens, while people with incomes below the poverty line, blacks, and those just old enough to vote in a presidential election were respectively 22%, 11%, and 10% of the electorate. Although the residentially mobile are the largest light-voting group, they have received little attention from scholars. Before analyzing movers, we will examine more closely our basic assertion—that registration is indeed a major impediment to voting.

The Registration Obstacle

The decentralization of election administration, combined with the primitive level of record keeping in many jurisdictions, creates something of a mine field for researchers attempting to establish how many people are registered in order to compute how many of those who are registered go to the polls. Each side of the

percentage computation is questionable; each deficiency contributes to underestimating turnout. The numerator of this computation—the number of votes cast—is somewhat less than the number of all voters because it excludes spoiled ballots, people who go to the polls but do not vote for a presidential candidate, and those whose write-in votes are not counted. The Census Bureau estimated that counting all such ballots would have increased the official turnout figure by about two percentage points in 1976 and one point in 1980 (U.S. Bureau of the Census 1982, 7).

The greater difficulty is the denominator, the number who are registered to vote. It is important to understand that state registration records are not a satisfactory source because there are no widely employed methods of removing in a timely fashion the names of people who have died or moved. Most states do purge the names of those who have not voted for a prescribed length of time, most commonly four years. But some people will have died and a great many will have moved before their time is up, thus any state's records will always be out of date. Moreover, at least 20% of the population lives in states with no purging requirement at all.

The inadequacy of state records may be judged by a recent episode in Rhode Island, a state that purges the names of those who have not voted for five years. In the fall of 1983, informational pamphlets on the impending statewide election were routinely mailed to the state's 530,000 registrants. Because these are mailed at the bulk rate, in the past all undelivered ones had been destroyed by the post office. But in 1983 the Secretary of State, Susan L. Farmer, arranged for the post office to return undelivered pamphlets to her office. More than 100,000 came back because the addresses were not at their official voting addresses. In California, the Secretary of State's office estimated in 1986 that 9% of all regis-

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trations are "deadwood"—people who have died or moved. Because all state registration rolls are inflated in this way, voter turnout cannot be accurately measured by dividing the number of votes cast by the number of persons registered.

Surveys do not present this problem and are a more satisfactory source of data for our purposes. Of those available, we have most confidence in the Vote Validation Study conducted as part of the University of Michigan Center for Political Studies 1980 National Election Study (NES), in which interviewers visited county offices to establish through direct inspection of the records whether each respondent had registered and voted (see Appendix). The major drawback of this source is the somewhat "upscale" character of the Michigan sample, which underrepresents poor people, particularly uneducated young men. (For a comparison of data sources used in the study of turnout, see the Appendix.) Doubtless this is why the vote-validated Michigan NES sample shows a 1980 turnout rate of 60%. While this is surely something of an exaggeration, it should be noted that the 52.6% official turnout figure reported by the Census Bureau understates turnout.²

The question addressed here is whether registration requirements are a significant deterrent to voting. The answer is clearly affirmative. Eighty-seven percent of the registered respondents in the Michigan NES sample voted in 1980. The 1980 Voter Supplement of the Census Bureau's Current Population Survey (CPS), based on unverified responses from a more representative sample of 114,944, yields an estimate that 88.6% of those registered voted. In short, those who are registered vote. This finding leads to a critical shift in perspective. Traditionally scholars have asked why people don't vote.³ The fact that registration is virtually equivalent to voting changes the question. The problem instead is to understand why people do not register. We believe that

many U.S. citizens are not registered because they have recently moved.

The bureaucratic tasks required to vote are a greater impediment for some groups than for others. This observation applies to the impediments to registration far more than those to voting itself. In Table 1 we provide convincing evidence that registration is the critical hurdle. Only 51% of citizens with a grammar school education voted in 1980, compared with 84% of those with more than four years of college, a gap of 33 percentage points. The gap, however, narrows to 17 percentage points when the comparison shifts to registered respondents. The less educated act much more like the better educated, once they have crossed the crucial barrier of registration.

This pattern is even more pronounced for different age groups. The differences in turnout among age groups almost disappear when the comparison is with people who have registered. Only 42% of those aged 18 to 24 made it to the polls in 1980, far behind the 69% turnout rate of people 35 and older. Yet among young people who managed to register, 86% voted, compared with 90% of those aged 35 to 64 and 85% of those over 64.

Most interesting of all is the relationship of political interest to turnout among those registered. People who say they hardly ever follow politics are half as likely to vote as are those who say they pay attention most of the time; half of the former group are registered, compared with 83% of the latter. Yet once people who profess no political interest do register, they are very good bets to vote. Fully 74% of them voted in 1980, compared with 91% of the avid political fans who were registered.

Who Moves?

In order to understand how many people are affected by the obstacle of registration, it is important to emphasize that

Table 1. Turnout and Registration by Age, Education, and Political Interest, 1980

Participation Variables	Percent of All Respondents Who Voted	Percent of Respondents Registered to Vote	Percent of Registered Respondents Who Voted
Education			
0-8 years	51	65	79
9-12 years	53	64	84
1-4 years college	68	75	90
5 or more years college	84	87	96
Age			
18-24	42	49	86
25-34	57	65	88
35-64	69	77	90
65 and over	69	82	85
Political interest			
Hardly at all	38	51	74
Only now and then	54	64	84
Some of the time	70	76	92
Most of the time	75	83	91
All U.S.	60	69	87

Source: 1980 National Election Study.

very large numbers of U.S. citizens have lived in their present residences for fairly short periods of time. Between March 1980 and March 1981, 16.6% of the voting age population, nearly 27 million people, moved. Most of them (62%) moved within the same county, another 22% stayed in the same state, and the remainder moved to another state (U.S. Bureau of the Census 1983a). Mobility was even higher 20 years earlier. The one-year mobility rate was 20% in 1960/61 and 17.9% in 1970/71 (U.S. Bureau of the Census 1984). Of course, some people are more likely than others to move and, once moved, to move again; at any time the population of movers includes both those who are good bets to stay put and others who will move again and again. Even so, we were surprised to find that 32.8% of the 1980 Michigan NES sample reported that they had lived in their current residences for two years or less. It is these people—a third of the sample—whose

characteristics we will examine. Staying put for more than two years seems to be something of a dividing line in the relationship between mobility and turnout (Wolfinger and Rosenstone 1980, 52).

Table 2 summarizes the more relevant demographic and attitudinal characteristics of movers and stayers (people who had lived in the same place for more than two years). The most striking thing about the movers is their youth. Sixty-eight percent of the movers, while only 23% of the stayers, were under age 35. Perhaps equally predictable is the somewhat higher educational level of the movers. Forty-four percent—compared with 33% of the stayers—had attended college. The difference in income between the two groups is almost marginal. Twenty-six percent of the movers and a third of the stayers enjoyed family incomes of at least \$25,000 in 1979. The racial character of the two groups is identical. Finally, the movers are overwhelmingly renters, while

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Table 2. Movers' Demographic and Attitudinal Characteristics

Demographic and Attitudinal Attributes	Percent of Those Living 2 Years or Less at Current Address	Percent of Those Living More Than 2 Years at Current Address
Turnout	48	65
Age		
18-24	30	7
25-34	38	16
35-64	27	54
Over 64	6	23
Education		
0-8 years	6	15
9-12 years	50	51
1-4 years college	33	26
5 or more years college	11	7
Family income		
\$0-9,999	27	25
\$10,000-14,999	18	15
\$15,000-24,999	29	27
\$25,000-34,999	14	18
\$35,000 or more	12	15
Race		
White	86	88
Black	11	12
Other	2	1
Housing		
Rent	54	19
Own	46	81
Political activities*		
None	88	87
One or more	12	13
Political interest		
Hardly at all	16	15
Only now and then	27	22
Some of the time	35	35
Most of the time	22	28

*The four political activities used in this index were (1) attendance at political meetings, (2) work for a party or candidate, (3) display of a campaign button or sticker, (4) membership in a political organization or club.

Source: 1980 National Election Study.

most stayers own their own homes.

This thumbnail sketch offers reasons for differing expectations about movers' turnout. Their youth argues for light voting, their higher educational attainment hints at a contrary effect. That so many movers rent suggests lower voting rates since people who own their own homes turn out at a higher rate than do renters

(Kingston, Thompson, and Eichar 1984, 131-50). With so many variables, multivariate analysis is called for to identify the relationship of any particular variable to turnout.

At least three published studies fill the bill; all agree that mobility strongly depresses turnout (Cassel and Hill 1981; Verba and Nie 1972, 139-46; Wolfinger

and Rosenstone 1980, 50-54). The most thoroughgoing of these studies demonstrated that, with demographic variables and registration laws held constant, people who had lived at their current addresses for less than a year before the 1974 election had a turnout rate 22 percentage points lower than those who had not moved for at least 10 years. People who had stayed put for 1 to 2 years were 19% lower, and even those who had lived in their current homes for 3 to 5 years were nine points lighter voters (Wolfinger and Rosenstone 1980, 52).

There was a time when movers faced residency requirements of up to two years, which were quite an obstacle to quick resumption of the voting habit. These onerous laws were struck down by the Supreme Court in 1972 (*Dunn v. Blumstein* 1972). In almost every state the residency requirement is now 30 days or less. Movers are still light voters despite the removal of significant legal barriers because of the need to reregister and the low priority that this action has for them. People in a new home have all sorts of arranging and adjusting to do, from redecorating to mastering innumerable details of domestic administration: where to shop, how to get to work, how to spend recreational time. Registering to vote is drab and boring, a weak claimant for attention. What is more, half of all moves occur from June through September (Heiden 1981, 48-51). Such summer movers have little time to register before the deadline, which is most commonly 30 days before the election, that is, the first week in October. In short, movers do not face legal obstacles to registering, but this step takes a back seat to the many things that must be done when trying to settle down in a new home.

Although movers are the biggest light-voting group in the country, virtually no attention has been given to their characteristics, other than to note that they are mostly young. We begin our analysis with

a point of great importance: while movers are much less likely than stayers to vote, they share with them most of the interests, attitudes, and forms of behavior that are usually associated with voting. Movers are just as likely as stayers to talk about the campaign, pay attention to political items in the news, think they have a say in politics, deny that politics is too complicated to understand, and assert that public officials care what people like themselves think. They are almost as likely to care which party wins the presidential election, to be interested in the campaign, and to follow politics most or some of the time. They are equally likely to engage in political activities other than voting. These findings would lead us to expect movers to vote as much as stayers, but only 48% of them voted in 1980, compared with 65% of the stayers. Even those movers who participate in politics are much lighter voters than activist stayers.

A Methodological Interlude

The data source on which we have relied up to this point is the 1980 NES conducted by the University of Michigan Center for Political Studies. The standard source of most academic research on voting behavior and public opinion for 30 years, the NES includes dozens of questions about respondents' attitudes toward candidates, issues, parties, and the political process itself; as well as many items on aspects of political interest and behavior. Moreover, we do not have to rely on respondents' claims about registration and voting; these points were checked at the relevant county records office. One disadvantage of the NES is its sample size: with 1,437 cases in the vote validation sample, few analyses are possible in which individual states are the unit of analysis. By the same token, some important demographic categories are so sparsely populated as to engender anxiety.

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Table 3. Mobility by Vote and Registration in 1980

Years in Current Residence	Percent Who Voted	Percent Registered	Percent of Registered Who Voted
0-2 years	48	56	87
3-5 years	58	66	88
6-10 years	60	73	82
11 or more years	72	82	88
Total	60	69	87

Source: 1980 National Election Study.

Sample size is the least of our worries with our second data source: the Voter Supplement of the Census Bureau's CPS. The CPS includes all the relevant demographic questions found in the NES, but its political content is limited to questions on registration and turnout, and these are not validated. Its advantages are a somewhat more representative sample (see Appendix) of enormous size: 114,944 cases even after we deleted noncitizens and cases where the interviewer did not ascertain if a vote had been cast. This great number of cases enables us to analyze the behavior of respondents in states with election day registration and other unusual state-level contextual variables. We can, moreover, achieve unusually precise population estimates, even for small groups. We will present analyses using both surveys when this allows us to profit from the advantages of each. Should the different samples give us similar findings, we would have increased confidence in our conclusions.

In our analysis we have chosen to use turnout as the dependent variable. As discussed earlier registration and turnout are almost the same thing. We have performed multivariate analyses using both registration and turnout as dependent variables. Residential mobility as an independent variable has a large and statistically significant impact on both turnout and registration. Overall, there is little difference between explaining regis-

tration and explaining turnout. We report our analysis on turnout, rather than registration, because turnout is the variable of interest.

The dependent variable in our analysis—turnout—is dichotomous; therefore the most appropriate statistical technique to measure the effect of any independent variable—mobility, education, age—would be one that specifies the nonlinear form of the relationship and makes correct assumptions about error terms. Consequently we have employed probit analysis in this paper.

The Effect of Mobility on Turnout

As a preliminary step, we examine the simple bivariate relationships between mobility and registration, and mobility and turnout. In Table 3 we display these relationships. We show that 48% of people who had lived 2 years or less in their residences voted in 1980, compared with 58% of those who had stayed put for 3 to 5 years and 60% for those in the 6-to-10 year category. Turnout leveled off only among people who had lived in the same place for more than 10 years, 72% of whom voted. The data are very similar for registration, as shown in the middle column of Table 3. In the right column we show that the percentage of the registered who voted did not vary appreciably by

Table 4. Probit Estimates of the Effect of Variables on Turnout in 1980

Variable ^a	National Election Study Coefficient	Current Population Study Coefficient
Closing date	-.032 (.007)	-.010 (.0004)
Age	.030 (.016)	.048 (.001)
Age squared	-.00016 (.00017)	-.0003 (.00001)
Income	.018 (.010)	.0007 (.00004)
Rent/own	.137 (.114)	.194 (.011)
Education squared	.005 (.0008)	.006 (.00007)
Mobility	.057 (.013)	.084 (.002)
Single/married	.223 (.104)	.189 (.010)
Other/full absentee registration		.069 (.011)
Political interest	.210 (.046)	
Local elections important/not important	-.572 (.136)	
Public officials care/don't care what respondent thinks	-.226 (.092)	
Constant	-1.700	-2.494
N	1,005	103,575
Percentage correctly predicted	72.3	72.5

^aThe coding for each of the variables is given in the Appendix.

Note: We deleted from the probit equation any respondents for whom information was not available on all variables used in the equation. The numbers in parentheses are the standard errors.

mobility. Even among the movers, 87% of the registered went to the polls.

Our first major task is to examine the relationship between mobility and turnout, with other independent variables controlled. The probit coefficients for both the NES and CPS samples are presented in Table 4. We show that mobility has a substantial and statistically significant impact on turnout when all other variables are controlled. This finding is consistent with other research and thus is not particularly surprising.

More noteworthy are the strong posi-

tive influences of home ownership and marriage.⁴ These effects are independent of age, income, and mobility (and attitudinal variables in the NES sample). We think that homeowners are more likely to feel a stake in the society and feel connected to their community, and that these ties account for their higher turnout. We distinguish between this sense of being rooted and the more plausible explanation of the stayers' higher turnout: more time to take care of the administrative chore of registering. Most movers, after all, were already living in the same county before

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they shifted residences, and thus are not likely to need an extended period of time to become familiar with local issues and candidates. Moreover, we are talking about participation in a presidential election, where knowledge of the relevant issues and candidates and identification with the appropriate symbols is not lost by moving from one town or state to another.

By the same token, married people face the same registration obstacles as the unmarried, yet they are more likely to vote. They have an inherent support and motivational system that single people lack (Campbell, Converse, Miller, and Stokes 1960, 109; Wolfinger and Rosenstone 1980, 44-46). Information costs may also come into play. Only one spouse need invest time and energy in learning how and where to register after a move, but both partners benefit. A single person does not enjoy this advantage.

The equation using NES data is more fully specified because of the attitudinal variables not available in the census data. Political interest, "external" political efficacy ("I don't think public officials care much what people like me think"), and belief in the importance of voting in local elections are all substantively and statistically significant in their relation to turnout. (Although a belief that voting in most local elections is unimportant decreases the probability of voting by a whopping 18%, it should be noted that only 12% of the sample holds this view.)

Variations in the Effect of Mobility

Having established that mobility depresses turnout, we next investigate variations in that effect. Is everyone's probability of voting equally depressed by moving, or do some kinds of people respond differently to the experience? There are at least three dimensions on which movers vary and that seem likely to affect their probability of voting. First, they differ in their demographic and attitudinal attri-

butes. Second, their life situations are not the same: some movers are single, some married; some are homeowners, some are renters. Finally, the legal environment into which they move will vary from state to state with regard to registration requirements.

We begin by examining the relationship of mobility to turnout at different levels of education. In Tables 5a and 5b we show that the impact of moving varies inversely with education. College graduates are decidedly less hindered by moving than people with less education. With all other variables in the NES equation held constant (see Table 5a), people with college degrees who had moved within six months of the interview were 13 percentage points less likely to vote than if they had lived in the same place for at least 11 years. Those with less than a high school education in the same situation were 21 percentage points less likely to vote.⁵ In other words, moving has a larger impact upon those with a lower probability of voting in the first place. Among people with six months or less residential stability, those without a high school diploma have a 37% probability of voting, compared with a 77% probability for college graduates.

In Table 5b we show much the same pattern. Using the CPS sample, we have enough cases to break down the categories more finely. People with some graduate education are less affected by mobility than are those with just a college degree. Indeed, they so quickly begin voting that there is scant room for improvement as the months in their new homes lengthen into years. Their turnout rate after six months is only seven percentage points below their rate at maximum residential stability. In both tables the probability of the best-educated people voting increases modestly with greater residential stability. Well-educated movers seem to hit the ground running; even just after changing their address they still turn out at 88% in the CPS study.

**Table 5a. Effect of Mobility on Turnout, by Education, in 1980
(National Election Study Data)**

Years of Education	Differences between Probability of Voting after 11 or More Years Stay and after the Following:					Probability of Voting after 11 or More Years Stay*
	Less than Six Months	6-36 Months	4-6 Years	7-9 Years	10 Years	
11 or less (221)	-21	-18	-12	-6	-2	58
12 (387)	-21	-17	-11	-5	-2	71
13-15 (208)	-19	-15	-10	-5	-2	78
16 or more (189)	-13	-10	-6	-3	-1	90
Total (1,005)	-19	-15	-10	-5	-2	73

Two other aspects of the effect of education emerge in Table 5. First, while each increment in education tends to raise the likelihood of voting, the sharpest break is between those who have obtained a college degree and those who have not. People who did not attend high school, high school graduates, and college drop-outs increase their probability of voting by almost identical amounts as they remain longer in their residences. Turnout of college graduates increases more slowly. Second, the familiar relationship between education and turnout is strongest for recently moved citizens and progres-

sively declines among those who have stayed put for longer periods. That relationship never comes close to vanishing, but it is weaker among long-term residents than among the more mobile respondents.

The relationship of mobility to turnout by political interest is almost identical to that by education: the lower the interest, the less turnout among recent movers and the more residential instability reduces the probability of voting. The relationship of interest to turnout declines progressively as stability increases, although it remains strong even among the most stable.

**Table 5b. Effect of Mobility on Turnout, by Education, in 1980
(Current Population Survey Data)**

Years of Education	Difference between Probability of Voting after 6 or More Years Stay and after the Following:					Probability of Voting after 6 or More Years Stay*
	Less than One Month	1-6 Months	7-11 Months	1-2 Years	3-5 Years	
0-8 (12,624)	-19	-18	-17	-6	59	
9-11 (13,815)	-18	-17	-16	-14	-6	64
12 (39,125)	-17	-16	-15	-13	-6	71
13-15 (20,370)	-16	-15	-14	-12	-5	77
16 (10,024)	-11	-11	-10	-8	-3	89
17 or more (7,617)	-7	-7	-6	-5	-2	95
Total (103,575)	-16	-15	-14	-12	-5	73

Note for 5a and 5b: The entry in each cell is the probit estimate of the effect on turnout of living at one's current residence for the indicated length of time compared to the turnout of those in the same educational group at 11 years of residential stability for the NES sample or 6 years for the CPS sample.

*The probability of voting of an individual with the indicated amount of education who had lived in the same place for 11 (or 6 years, with all the other variables in the equation controlled).

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While mobility accentuates the impact of education and political interest on turnout, this is not the case with age. Moving has a strong and fairly similar negative effect on the turnout of *all* age groups. A very recent change of address decreases the likelihood of voting by 18 percentage points for people aged 18 to 21 and has only slightly less effect on older people. Across all age groups the probability of voting increases with stability to about the same extent.⁶ In summary, mobility has a strong and differentiating effect on people with different levels of education and political interest and, somewhat surprisingly, a strong but uniform effect on those at different ages.

Movers differ not only in their demographic and attitudinal attributes but also in their life-styles. As we saw, with all other demographic variables controlled, renters are considerably less likely to vote than are homeowners. Is this effect accentuated by how recently these individuals have moved? Apparently not. Renters and homeowners are both strongly affected by moving but to about the same extent. This is not to say that their probabilities of voting are the same. At 11 years of stability, renters have a 64% probability of voting compared with 77% for owners. Both mobility and renting have a substantial and independent depressive effect on turnout. Even the most recently moved homeowners vote as much as renters who have stayed in the same dwelling for 7 to 9 years. The relationship between ownership status and turnout seems to have different causes than that between mobility and turnout. The latter reflects the cost side of a cost-benefit analysis of turnout: the administrative hurdle of registering impedes immediate resumption of the voting habit. But once we have controlled not only for mobility but for all the other variables in our probit equation (including education, interest, and income), it is difficult to see how renters face any

impediments to registering and voting not experienced in equal measure by homeowners. The explanation would seem to lie on the benefit side: renters are less impelled to register, perhaps because of weaker feelings of societal membership.

The relationship between marital status and mobility and turnout is virtually the same as that with homeownership. Married people are heavier voters; the impact of moving on their likelihood of turning out is faintly weaker than for single individuals.⁷ In short, the relationship of these life situation factors and turnout is compounded when mobility is added to the picture. Since this is true with everything else controlled, it suggests that being either a homeowner or married gives one a larger personal stake in the community, or more motivation to register and vote.

Finally, the legal environment in which movers find themselves differs from state to state. We argue that movers are light voters because of the need to reregister and the low priority of doing so. This leads us to expect the impact of mobility to decline as the importance of registration declines. This diminution would be most marked in the four states that permit election day registration—Maine, Minnesota, Oregon, and Wisconsin—and in North Dakota, which does not require registration at all. Turnout in these five states is far above the national level. In 1980, when 52.6% of the U.S. voting age population went to the polls, the comparable figures in the easy registration states ranged from 61.5% in Oregon to 67.2% in Wisconsin and 70.1% in Minnesota (U.S. Bureau of the Census 1983b, 492).

Initial analyses confirmed our hunch about the weaker depressive effect of mobility in the 5 states. A simple cross-tabulation contrasting turnout at different stages of mobility in the two sets of states showed that people who had lived in their homes for no more than six months had a 61% turnout rate in the 5 states, com-

**Table 6a. Effect of Mobility on Turnout, by Ease of Registration, 1980
(National Election Study Data)**

States	Difference between Probability of Voting after 11 or More Years Stay and after the Following:					Probability of Voting after 11 or More Years Stay
	Less than Six Months	6-36 Months	4-6 Years	7-9 Years	10 Years	
Easy registration (47)	-10	-8	-5	-2	-1	93
Other (958)	-20	-16	-10	-5	-2	72
Total (1,005)	-19	-15	-10	-5	-2	73

pared with 43% in the other 45 states. The gain in turnout with greater stability was 20 percentage points in the 5 states and 31 points in the other 45. The same point was made by analysis of ordinary least squares equations of demographic variables and turnout for the two sets of states. The relationship of mobility to turnout is almost twice as strong in the 45 states as in the easy registration five, in which more permissive laws ameliorate much of the blighting effect of moving. By the same token, the effect of other demographic variables is weaker in the easy registration states because, with the registration barrier so much lower, the cost of turnout is drastically reduced.

The strongest test of the proposition would be made with probit analysis, and here the results are not so clear. The NES sample provided a gratifying confirmation, as we show in Table 6a. With only

47 respondents in the easy registration category, however, this data set is not ideal for state-level analysis. This is the strong point of the CPS, which has 8,341 respondents in the five easy registration states. Here we were disappointed. As we show in Table 6b, the findings are in the predicted direction, but the differences between the two groups of states are much smaller, at most two percentage points. This analysis suggests that mobility deters voting almost as much in states where registration is not so great a barrier.

We have seen strong evidence that mobility has a significant impact on registration and turnout. Among the most recent movers, differences in education and political interest significantly change the probabilities of registering and voting. These relationships weaken as residential stability increases. The longer people live

**Table 6b. Effect of Mobility on Turnout, by Ease of Registration, 1980
(Current Population Survey Data)**

States	Difference between Percentage Who Vote after 6 or More Years Stay and Percentage Who Vote after					Probability of Voting after 6 or More Years Stay
	Less Than One Month	1-6 Months	7-11 Months	1-2 Years	3-5 Years	
Easy registration (8,341)	-14	-14	-13	-11	-4	81
Other (95,234)	-16	-15	-14	-12	-5	73
Total (103,575)	-16	-15	-14	-12	-5	73

Note for 6a and 6b: The entry in each cell is the probit estimate of the effect on turnout of living at one's current residence for the indicated length of time compared with the turnout of those in the same state at 11 years of residential stability for the NES sample or 6 years for the CPS sample.

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at the same address, the more likely they are to vote, regardless of any other characteristic. Even less educated people with little interest in politics finally get around to registering if they live in the same place for several years. Differences in life-styles with regard to marital status and renting or owning one's residence also reveal a strong depressive effect on turnout as mobility increases, although the effect is fairly uniform. An early closing date for registration also depresses turnout among movers, although not as strongly as we expected.

How to Increase Turnout

If almost everyone who is registered votes, holding elections on Sunday, making election day a holiday, or extending the hours that the polls are open would do little to improve turnout. These measures are designed to facilitate people who are registered, who are already very likely to vote. Money, energy, and political capital should be invested in efforts that have more potential: getting people to register on the amply documented assumption that once people register, they vote.

We have established that movers are less likely to vote for no reason other than the need to reregister and the low priority that this action has. There is no question that election day registration would be particularly beneficial to such people. It is equally certain that this is a dead issue. President Carter's proposal for a national scheme did not even get out of a congressional committee in 1977, and there are no signs that the idea will soon be revived in Congress. In 1978 the voters of Ohio decisively rejected an election day registration plan enacted by the state legislature. This seems to be an idea whose time has come and gone.

A more feasible approach would be to concentrate on maintaining the registration of people who move. We propose a measure that would reduce the negative

effect of moving on turnout by linking the maintenance of registration to an action that is usually an intrinsic part of moving. Almost all movers file a change-of-address notice at a local post office before leaving their old home. According to the postal service, 40 million such forms were filed in 1983 and again in 1984. Some of these reported temporary moves, others were used by people who moved more often than once a year. If this form were modified to permit the mover to indicate that the move included a change of voting address and carbonized so a copy could be produced simply by filling out the form, the second copy could be sent by the post office to the chief election official in the state where the move originated. In 83% of all moves, this would also be the destination state. In the case of intrastate movers who were already registered, this copy could be accepted as adequate legal authority to reregister the individual at the new address. If the mover were not registered already, receipt of the notice might stimulate dispatch of a registration-by-mail postcard.

In any case, receipt of the notice would lead to cancellation of the old address registration. This would be an efficient and inexpensive way of removing deadwood from the registration rolls, thus eliminating perhaps the biggest opportunity for election fraud. Pruning the rolls would reduce the costs of election administration and of direct mail campaigning. Accurate registration records would also make jury selection more efficient and have a beneficial effect in those states where a proportion of the number of registered voters is used as a threshold for successful bond issue referendums or for signatures to qualify a proposition for the ballot.

We began to advocate this idea in September 1983, concentrating on attracting the attention of officials at the federal level. Action by the federal government is a necessary but not sufficient condition

for enacting this proposal, which could be implemented only in a state that made the necessary changes in its voter registration laws. People who heard the proposal liked it, but at first there was a very small audience. In March 1985 a bill (HR 1668) to accomplish the federal aspect of our proposal was introduced by Representative Mel Levine (D.-CA), with three Republican and four Democratic initial cosponsors. A similar bill, S 1439, was introduced later that year by Senators Christopher Dodd (D.-CT) and John Chafee (R.-RI).

The first legislative action was in the House. Over two dozen more Representatives had cosponsored HR 1668 by July 1985, when it was the subject of a hearing by the Subcommittee on Postal Operations and Services of the Committee on Post Office and Civil Service. The chairman, Mickey Leland of Texas, and the ranking minority member, Frank Horton of New York, were both strongly favorable, as were two testifying secretaries of state, March Fong Eu, a Democrat from California, and Susan L. Farmer, a Republican from Rhode Island. Leaders of three groups interested in expanding voter turnout supported the bill but expressed fear that some states might use the procedure to purge names of movers and pay little attention to reregistering them. The postal service witness, the only outspoken critic, was subjected to skeptical questioning by Leland and Horton.

After the hearing there were several rounds of negotiations with the three voter registration groups on the one hand and with several state and local election officials on the other. The former wanted to avoid purging without reregistration and were untroubled by deadwood and administrative feasibility, major concerns of even the most progressive election officials.

Several criteria are important as policy makers ponder this proposal: cost, administrative feasibility, and political

implications. The postal service, which is not motivated to minimize the difficulties, has produced several estimates of the cost of sorting change-of-address notices and transmitting them to the appropriate election official. In the fall of 1985, the most recent and lowest estimate was 30¢ per form. If every state participated in the program, the total cost of postal service processing would be \$12 million. No state could participate without enacting its own implementing legislation, however. This process would take years and some states doubtless would never choose to join in. Therefore the \$12 million figure is an upper limit that will probably never be attained. Also, this cost will be mitigated by substantial savings in printing and postage as deadwood is eliminated. Partly because the savings would be realized by the states and partly because of the mood of fiscal stringency in Washington in the mid-1980s, HR 1668 provided that these administrative costs would be borne by participating states.

The plan's administrative feasibility is attested by the endorsement of HR 1668 by 11 secretaries of state, a number of individual county clerks, the National Association of County Recorders and Clerks, and the legislation committee of the California County Clerks Association. On the other side of the ledger is a resolution of opposition passed by the Florida State Association of Supervisors of Elections.

HR 1668 was reported in June 1986 by the Subcommittee on Postal Operations and Services. In September the full Committee on Post Office and Civil Service, by a unanimous voice vote, ordered the bill reported. For a moment it appeared that HR 1668 might pass the House on suspension of the rules and—just possibly—be attached to a bill on the Senate floor. Instead, it became a casualty of the election-year rush to adjourn. At the time of writing, its prospects in the 100th Congress seem bright.

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Political Impact

And then there is the political question. How would the interests of the two parties be affected by easing the effects of moving on turnout? This question can be answered in large measure by further analysis of our data to see how a voting population enlarged by weakening the relationship of mobility to turnout would differ from the current voting population. Before considering the political implications of our proposal, we will see how effective this proposal would be in increasing turnout.

We begin by estimating the turnout rate that would result from eliminating the effect of mobility on turnout. How many people would vote if everyone participated at the same rate as the most stable segment of the population? This question cannot be answered simply by assuming that the mobile would vote as much as the stable because the two groups differ with respect to other variables affecting turnout. Among other things, movers are predominantly young renters, two characteristics associated with low turnout. We can use the equations in Table 4 to make the appropriate forecasts. After 10 years in the same residence, turnout no longer increases with additional stability. Therefore we estimated what the turnout rate of the sample would be if everyone voted as if they had lived at the same address for 11 years, as opposed to their current probability of voting. On the basis of the NES probit equation, we estimate that the national turnout rate would increase by nine percentage points under these conditions.⁹

This figure defines the outer limit of our proposal's potential consequences; indeed, it doubtless exaggerates them. Enactment and nationwide implementation of HR 1668 would not eliminate the relationship between residential mobility and turnout, if only because of interstate moves, which would be beyond the reach of our proposal. Even a nine-percentage

point gain in turnout would not alter this country's next-to-last position in international rankings (Wolfinger, Glass, and Squire 1985) because mobility is by no means the only explanation for low U.S. voting rates. Nevertheless, an increase of nine, or seven, or five percentage points would be a major accomplishment, comparable to the gains produced by election day registration and without any risk of election fraud.

Other than increasing the number of voters, what consequences would ensue from eliminating the effect of mobility on turnout?¹⁰ The proper comparison to answer this question is between actual voters and the hypothetical larger voting population.¹⁰ The characteristics of the actual and projected voting populations are summarized in Table 7. In most instances, the two groups are almost identical. The projected voters would be a bit younger, with people under the age of 34 accounting for 32%, compared with their 28% share of those who actually voted in 1980. This is the only difference in demographic characteristics that exceeds sampling error. Other differences are both statistically and substantively insignificant. People without college degrees would account for 77% of all voters, in contrast to the actual 75%. People with family incomes under ten thousand dollars would increase from 18% to 19.2%.

Some people concerned with voting—including some officials who administer elections—deplore efforts to "make voting too easy" because they believe that a certain amount of difficulty screens out those citizens whose commitment to electoral participation is unacceptably weak. Whatever the merits of this argument as prescriptive political theory, its relevance to our present concerns is limited because HR 1668 would affect only people who had registered before they moved. This is not, however, tantamount to saying that if the mobile voted at the same rate as the stable, the voting population would dis-

Table 7. Comparison of Actual Voters to Projected Voters at Eleven Years of Residential Stability, 1980

Characteristics	Percent of Actual Voters	Percent of Projected Voters
Age		
18-21	4	4
22-27	11	14
28-33	13	14
34-70	63	60
71 and older	8	8
Education		
11 years or less	17	17
12 years	37	38
13-15 years	21	22
16 or more years	25	23
Income		
\$0-\$9,999	18	19
\$10,000-\$19,999	28	28
\$20,000-\$29,999	28	28
\$30,000-\$49,999	19	18
\$50,000 or more	7	7
Party Identification		
Democrat	49	49
Independent	10	12
Republican	41	39
Other/apolitical	0	1
Self Identified Ideology		
Liberal	20	18
Middle-of-the-road	19	20
Conservative	34	34
Haven't thought much/don't know	27	28
Political Interest		
Hardly at all	10	10
Only now and then	20	22
Some of the time	38	37
Most of the time	33	32

Source: 1980 National Election Study.

play an equal level of civic virtue because those movers who most quickly return to voting are likely to be the most alert and concerned part of the mobile population. They have, after all, surmounted a barrier to participation not faced by the stayers. If the costs of moving were reduced, they might be joined at polling places by movers with somewhat less civic virtue, but still, perhaps, no less virtue than stayers.

The answer to these speculations is in

Table 7, in which we show that the least politically concerned citizens, those who say they follow politics "hardly at all," would account for the same 10% of the projected expanded voting population that they do of actual voters. People with intermittent interest in politics would be 22% of the expanded population, up two percentage points from their share of actual voters; and the proportion of people who follow politics some or all of the time would drop two points. None of

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these differences approaches sampling error, and we conclude that HR 1668 would not lower the political interest of the U.S. voter.

The most interesting question, of course, is the political consequences of expanding the electorate by reducing the effect of mobility on turnout. We can calculate the number of Democrats and Republicans in the projected larger voting population and compare that number to the partisan distribution among actual 1980 voters. This reveals a microscopic loss for the Democrats, from 49% of the actual voters to 48.6% of the projected voters. More noteworthy—although still below the level of statistical significance—is a loss of nearly two points for the Republicans, achieved almost entirely through a gain in the proportion of voters who are Independents. If we assume that this group splits its vote evenly, the net damage to the Republican party is more on the order of one percentage point.¹¹ It is true that elections have been won by a smaller margin and that no politician likes to give up any advantage, even a small one. Yet no responsible analyst of survey data would take seriously so tiny a gap between respondents in a sample reduced to barely a thousand cases.

Our conclusion about the evanescent character of the apparent loss in Republican identifiers is buttressed by the findings for self-identified liberals and conservatives, which are also in Table 7. In contrast to a two-percentage point loss for Republicans, it is liberals who suffer a similar decline from the actual to projected voters. The proportion of conservatives remains the same. The liberal loss comes from middle-of-the-road respondents and those who say that they have not thought enough about liberalism and conservatism to make a choice. None of these differences in partisan and ideological identification comes close to statistical significance. We cannot find much in these findings to alarm either party.

Conclusion

Movers resemble stayers on the motivational variables that are directly related to voting: interest in politics, attention to the campaign, concern about the outcome, and political efficacy. These similarities support our conclusion that movers' lower turnout reflects the administrative burden of registration rather than lower civic virtue. We would expect movers to vote more if the burden of reregistration were eased. We have proposed a method to accomplish this and estimate that if the effect of mobility on turnout were removed, turnout would increase by nine percentage points. Expanding the voting population in this way would produce no consequential advantage for either party.

Appendix

1980 Vote Validation Study

The Vote Validation Study covered respondents in the conventional American National Election Study (NES) time series sample ($N = 1,614$). This sample was to be interviewed before and after the 1980 election; those whose postelection interviews were not completed were, nevertheless, included in the Vote Validation Study. For a variety of reasons, information on registration status or voting could not be gathered on 177 of the 1,614 respondents. We have deleted these cases from our analysis (categories 0, 4, 5, 6, 7, 8, 9 on variable 1207), which is limited to the 1,437 respondents for whom validated data are available on both registration and voting. See the relevant codebook: University of Michigan Center for Political Studies (1982, 712–13).

Coding of Variables Used in Study

1980 National Election Study. Closing date: 0 = living in state with election day

Table A-1. Samples for the Study of Registration and Turnout
(in percentages)

Characteristics	1980 Census	1980 Current Population Survey	1980 National Election Study Vote Validation Study
Age			
18-24	18.4	17.8	14.2
25-34	22.9	22.7	23.0
35-64	42.9	44.1	46.0
65 and over	15.7	15.5	16.8
Education			
0-12 years	68.2	63.6	61.9
1-4 years college	25.3	29.1	28.6
5 or more years college	6.5	7.3	9.4
Income			
\$0-9,999	27.5	27.4	24.7
\$10,000-14,999	14.9	17.9	15.3
\$15,000-24,999	25.5	28.8	28.0
\$25,000 or more	32.1	25.9	32.0
Race			
White	83.4	88.2	88.2
Black	11.7	10.6	10.6
Other	4.9	1.2	1.2

Sources: The 1980 census figures for age and income are calculated from the U.S. Bureau of the Census 1983c. The numbers for education and race are calculated from Census of Population and Housing, 1980: Summary Tape File 3C (Machine-readable data file), prepared by the Bureau of the Census 1981. All of these figures are for the resident population *including noncitizens*. The figures for the CPS and the 1980 NES are for citizens 18 years old or older.

registration; coded value for other states was number of days from close of registration to election day (Council of State Governments 1980).

Age (variable 1179): respondent's age in single years, 18 to 97.

Income (variable 686): coded in 22 categories as done by NES with 1 = less than \$2,000 and 22 = \$50,000 and over.

Homeownership status (variable 719): 0 = rent or other financial arrangement; 1 = own.

Education squared: the square of the value coded for education (variable 429), which ranged in single years from 0 to 17.

Mobility (variable 718): less than 6 months = .25; 6 to 12 months = .75; 1 to 2 years = 1.5; 2 to 3 years = 2.5; 3 to 4 years = 3.5; 4 to 5 years = 4.5; 5 to 6

years = 5.5; 6 to 7 years = 6.5; 7 to 10 years = 7.5; 8 to 9 years = 8.5; 9 to 10 years = 9.5; more than 10 years = 11.

Marital status (variable 409): 0 = never married, divorced, separated, widowed, common law, other; 1 = married.

Political interest (variable 974): 1 = hardly at all; 2 = only now and then; 3 = some of the time; 4 = most of the time.

Importance of local elections (variable 146): 0 = disagree (respondent does think local elections are important); 1 = agree.

Efficacy (variable 1033): 0 = disagree (efficacious); 1 = agree (not efficacious).

1980 Current Population Survey. Marital status: 0 = not married; 1 = married.

Absentee registration: 0 = living in state with no absentee registration provi-

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sions; 1 = living in state with absentee registration provision.

Closing date: same as NES.

Age: respondent's age coded in single years.

Income: under \$1,000 = 5; \$1,000 to \$1,999 = 15; \$2,000 to \$2,999 = 25; \$3,000 to \$3,999 = 35; \$4,000 to \$4,999 = 45; \$5,000 to \$5,999 = 55; \$6,000 to \$7,499 = 67; \$7,500 to \$9,999 = 88; \$10,000 to \$11,999 = 110; \$12,000 to \$14,999 = 135; \$15,000 to \$19,999 = 175; \$20,000 to \$24,999 = 225; \$25,000 to \$49,999 = 375; \$50,000 and more = 600.

Homeownership status: 0 = do not own home; 1 = own home.

Mobility: less than 1 month = .041; 1 to 6 months = .292; 6 to 11 months = .75; 1 to 2 years = 1.5; 3 to 5 years = 4; 6 years = 6.

Education squared: The square of the respondent's coded value for education, ranging from 0 to 18 years.

Notes

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1. This figure is from the Vote Validation Project of the 1980 National Election Study (NES), which is described in the Appendix. For comparisons of U.S. and foreign performance on different measures of turnout, see Glass, Squire, and Wolfinger (1984) and Wolfinger, Glass, and Squire (1985). For a multi-

variate analysis demonstrating that the greatest part of the disparity in turnout between the United States and other countries reflects U.S. registration procedures, see Powell (1986).

2. Part of this underestimation is a result of the failure to count some ballots we have described. A more important source of underestimation is the denominator of the official turnout percentage: the entire population age 18 and above, *including non-citizens*. Deleting noncitizens from the denominator of the percentage calculation for the 1972 election raised the estimated turnout by 1.2 percentage points (Wolfinger and Rosenstone 1980, 116). The noncitizen proportion of the population has grown considerably since then. This trend alone may be responsible for the decline in official turnout from 55.2% in 1972 to 52.6% in 1980. We cannot duplicate the 1972 correction for 1980 because we have not succeeded in obtaining from the Census Bureau an estimate of the voting age citizen population in 1980. Deleting the noncitizens from the Current Population Survey (CPS) increases the estimated 1980 turnout by three percentage points (U.S. Bureau of the Census 1982, 5).

3. For an exception, see Erikson (1981). He, however, came to a very different conclusion from ours:

Thus, one reason that people vote may be to stay registered. . . . Remove the cost of registration, and some who otherwise would have voted stay home because it is no longer necessary to vote in order to stay registered. (p. 274)

4. Despite their marginal levels of statistical significance, variables for homeownership, age, and age squared were kept in the NES equation for two reasons. First, we had strong theoretical grounds for including them in our equations and the sign of each of the suspect coefficients is in the expected direction. Second, the CPS coefficients for the variables are statistically very significant, which strengthens our belief that they belong in our analysis.

5. Wolfinger and Rosenstone (1980, 54) found that moving within a year of the 1974 election had a slightly greater negative effect on the turnout of the college-educated, a difference that disappeared after two years of stability. We have no explanation of this disparity, other than noting that the newer finding is more compatible with the theoretical approach in both studies and that the 1974 finding is based on a subsample of 7,936 cases, while our probit analysis of the CPS sample employs 103,575 cases.

6. Wolfinger and Rosenstone (1980, 52-54) found that the effect of mobility on turnout increased with age.

7. We also examined the relationship between both homeownership and turnout, and marriage and turnout. Unmarried renters have the lowest probability of voting and are hardest hit by moving, while married homeowners have the highest turnout

rate and are a bit less affected by changing their residence. Single owners and married renters are in between, turning out 9 percentage points above single renters and 11 points less than married owners.

8. The most stable mobility category in the CPS data is six years or more, hence for this data set we were only able to estimate the increase in turnout if everyone voted as if they had lived in the same place for six years, as opposed to their actual probability of voting. Under this condition the national turnout rate would rise by 5.5 percentage points.

9. What groups of respondents are to be compared to each other to answer this question? Because the status quo is an obvious reference point, one group is actual voters. If we were interested in the characteristics of those people deterred by mobility from voting, the other comparison group would consist of the hypothetical 9%. But the question we are trying to answer is different: How would elections change if mobility no longer depressed turnout? We answer this question by comparing actual voters not to the potential 9% increment but to the hypothetical larger voting population created by adding the 9% to existing voters. Because a measure like HR 1668 would not eliminate the depressive effect of mobility, this method produces

somewhat inflated effects, but we have no alternative. We calculated the characteristics of projected voters by the method used by Wolfinger and Rosenstone (1980, 139).

10. One factor is beyond quantitative calculation. One or the other party may be able through its own efforts to reregister supporters who move. If this were the case, enactment of our proposal would, by governmental action, do for both parties something that one party could do for itself. This is not a wholly hypothetical observation. In some places Republicans are using their superior resources and organizational coherence to identify movers likely to be Republican and also movers likely to be Democrats, whose continuation on the registration rolls can then be protested. If these practices become an important part of Republican calculations, our proposal's bipartisan appeal will be limited to its effectiveness as a scourge of deadwood. This might not be negligible, particularly in those places where Republicans believe that large-scale exploitation of deadwood for fraudulent purposes is something of a Democratic monopoly.

11. In point of fact, Independents usually vote predominantly for the winning presidential candidate (Keith, Magleby, Nelson, Orr, Westlye, and Wolfinger 1986).

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VOTING OUTCOMES IN THE 1984 DEMOCRATIC PARTY PRIMARIES AND CAUCUSES

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Scholarly inquiry concerning influences on electoral outcomes in the presidential nomination process, though extensive, has been conducted almost exclusively with data collected at the individual level of analysis. The Michigan model of normal vote analysis suggests that long-term influences measured at the aggregate level, such as the sociodemographic, economic, and ideological characteristics of the states, are also important in determining electoral outcomes. We present an aggregate-level analysis of state characteristics that affected the Hart, Jackson, and Mondale vote proportions in the 1984 Democratic caucuses and primaries. Our primary election models explain between 65% and 83% of the variance in candidate vote shares, with socio-demographic and economic factors as the leading indicators. In the caucuses, we find that campaign spending and sociodemographic influences are dominant in models that explain between 38% and 81% of the variance. We conclude with a brief discussion of what our findings mean for future Democratic candidates.

Presidential elections have always been the centerpiece of American democracy. However, it is only recently that the public has been allowed both to determine the party nominees and to select between them in the general election. Presidential nominations, once brokered among a few party leaders, are now largely determined by the millions of party followers who participate in state primaries and caucuses. As the presidential nomination process has become more popular it has also become more complex. Several related research programs have attracted and organized scholarly efforts to understand the dynamic processes underlying selection of presidential nominees within the major parties. One of the principal issues dividing analysts has

been whether to study influences on electoral outcomes at the aggregate or the individual level of analysis.

Aggregate-level studies focusing on turnout and representation have become common. Various factors explaining differences in voter turnout in primaries have been identified (Norlander and Smith 1985; Ranney 1972, 1977; and Schier 1982), while studies of representation have examined whether presidential primary and caucus electorates are representative either of the total electorate or of party identifiers (Crotty and Jackson 1985; Lengle 1981; Marshall 1978, 1981; Ranney 1972, 1977). In addition, other aggregate-level studies have examined the possible impact of campaign expenditures upon primary and caucus outcomes

(Goldstein 1978; Orren 1985). Though aggregate-level studies have dealt extensively with aspects of the electoral process such as rates of participation among sociodemographic groups, turnout levels, and campaign spending levels, analysts have not taken the further step of asking what impact aggregate-level influences actually have on candidate vote shares.

A second line of scholarly inquiry, operating almost exclusively at the individual level of analysis, has concentrated on vote choice. Studies of individual vote choice have attempted to assess the relative importance of candidate strategies (Aldrich 1980a, 1980b; Brams 1978; West 1984), voter predispositions, candidate images, issues, and ideology (Bartels 1985a, 1985b; Gopoian 1982; Wattier 1983) in explaining voter decision making in presidential nomination campaigns.

While analyses of individual-level data concerning primary and caucus vote choice enhance our understanding, they provide incomplete explanations for outcomes in the presidential nominating process because they focus only on short-term influences such as voter predispositions, candidate images, and issues. The Michigan model of normal vote analysis suggests that long-term factors measured at the aggregate level, such as the sociodemographic and economic characteristics of the states, are also important in determining electoral outcomes. These are factors that the turnout, representation, and campaign expenditure studies have clearly indicated are relevant for explaining voting outcomes in presidential primaries and caucuses.

Like Kramer (1983, 94) we are persuaded that in some cases "an aggregate-level analysis is more likely to yield valid inferences about individual behavior than one based on individual-level data." Given that a cross-sectional study of primaries and caucuses in 51 settings (the 50 states and the District of Columbia) would be impossible to perform at the

individual level, this paper employs aggregate-level data to explain voting outcomes in the 28 caucuses and 27 primaries that made up the delegate selection process in the Democratic party in 1984. This approach will allow us to develop and test a model of candidate vote shares in primaries and caucuses that takes into account both long- and short-term impacts on aggregate voting outcomes.

We selected the 1984 Democratic party nomination process as the arena for our examination for several reasons: (1) most of the previous literature on the impact of reforms on the nomination process and on voter decision making has focused on the Democratic as opposed to the Republican party process, (2) the contest for the Democratic nomination in 1984 was in doubt down through the final events of early June, (3) there was no contest for the 1984 Republican party nomination, and hence Republicans were free to cross over and participate in the Democratic party process if the state rules permitted, (4) the Democratic party race was, for the most part, a three-person contest from beginning to end, thus providing a high degree of candidate stability across the various state races, and (5) the 1984 Democratic party nomination process provides a unique opportunity for assessing the changing coalitional makeup of the party. The candidacies of Walter Mondale, Gary Hart, and Jesse Jackson involved a contest over the "soul" of the Democratic party and the direction that it should take into the future.

Six Dimensions of Influence on Electoral Outcomes

In this study we will focus on six aggregate-level contextual and structural dimensions of influence on electoral outcomes in the 1984 Democratic party primaries and caucuses. Behind each dimension—the sociodemographic characteris-

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tics of the state, the political context of the state, the economic conditions existing in the state prior to the election, the electoral rules governing the conduct of the event, the in-state campaign expenditures of the contestants in the event, and the level of voter participation—lies an extensive literature. We will present findings regarding the impact on candidate vote shares of each of these six dimensions both separately and in a general multivariate model, evaluate the extent to which our findings support or conflict with earlier studies, and discuss the implications of our findings for the conduct of presidential politics within the Democratic party.

The Influence of Sociodemographic Factors

Group or sociodemographic influences are thought to be central to the political decisions that individuals make. The dominant intellectual traditions in the area of electoral behavior—the sociological and the social-psychological approaches—both have addressed the impact of group influences on electoral outcomes. The sociological tradition has focused almost exclusively on the role of group identifications in structuring electoral outcomes. Although those representing the social-psychological perspective (identified with Campbell, Converse, Miller, and Stokes 1964, 161) have been less convinced of the direct influence of group identifications on vote choice, even they observed that "members of these groups appear to think and behave politically in distinctive ways." More recently, however, Beck (1986, 259) and others have noted a "revival in the use of social group variables in voting analysis [that] is highly promising." This study will measure the impact of several major sociodemographic variables on the vote shares enjoyed by the three major candidates in the Democratic primaries and caucuses of 1984.

The Influence of Political Context

A second broad dimension from which explanations of electoral outcomes are commonly sought concerns general ideological context or political culture. Flanigan and Zingale (1983, 72) highlight the potential importance of state political culture or context as an independent influence on the presidential nominating process when they argue that "sometimes the culture and traditions of an area will reverse or reinforce the political tendencies of other social groupings to which the individual belongs; this may result in a pattern of political behavior different from that of people with similar characteristics in other parts of the country" (see also Klingman and Lammers 1984). Substantial research indicates that political culture in the states exercises a pervasive influence over both elite and mass behavior as well as over political structures and policy outputs (Kincaid 1982, 59). Therefore, we explore both mass and elite ideological orientations as well as key organizational and structural elements of the electoral contexts that Mondale, Hart, and Jackson confronted as they did battle in the primaries and caucuses.

The Influence of Economic Conditions

A general increase in interest in the impact of economic influences on politics has led students of political economy to ask precisely which aspects of the economic environment affect electoral behavior and to what degree. Much of the recent debate over the influence of economic conditions on electoral outcomes was sparked by Kramer's (1971) analysis of economic influences on congressional elections. Kramer's finding that economic fluctuations had a relatively strong effect on voting outcomes was vigorously challenged on both theoretical and operational grounds by Stigler (1973). The latter's basic point was that sensible

voters should not, and did not, respond to short-term fluctuations in economic performance in making their electoral decisions. Stigler found (1973, 166) "no . . . statistically significant relationship between average income performance and share of the vote received."

The debate over the impact of economic conditions on electoral outcomes has remained largely within the parameters set for it by the initial confrontation between Kramer and Stigler (see Arcelus and Meltzer 1975; Bloom and Price 1975; Tufte 1975). After surveying this and related debates over the nature and magnitude of the impact, if any, of economic conditions on electoral outcomes, Fiorina (1978, 429) judiciously concluded that "on the whole the evidence appears to indicate that economic conditions have an impact on the outcomes of congressional elections, although the specifics of the studies leave their authors wrangling about econometric techniques and their readers somewhat confused." We will attempt to alleviate some of this confusion, at least as it relates to the impact of economic circumstances on electoral outcomes during the Democratic primaries and caucuses of 1984.

The Impact of Electoral Structures

Unlike the sociodemographic characteristics of a state's electorate, the ideological disposition of its Democratic leadership and electorate, or the aggregate economic conditions pertaining in a state immediately before an election contest, electoral structures are party-specific influences on electoral behavior. Scholars as well as politicians have always been interested in the rules that structure the electoral process and in whether those rules advantage one candidate or type of candidate over others (Norrander and Smith 1985, 28; Pomper 1985, 7-8). Lengle and Shafer (1976) have argued for a truly magisterial role for party rules. They contend that "altered primary regu-

lations, over the medium run, could well determine which blocs will be influential within a party. . . . In the long run, such ground rules may even shape the grand conflicts of an epoch, indirectly but powerfully favoring some causes and not others" (Lengle and Shafer 1976, 25, 35, 40). Other scholars, most notably Hammond (1980), have challenged Lengle and Shafer on the degree of influence that rules exercise over outcomes in the nomination process. We will ask whether any of the Democratic candidates of 1984—Mondale, Hart, or Jackson—did significantly better in open- as opposed to closed-rule states or in states that distributed delegates proportionally as opposed to states that gave some sort of bonus to plurality winners.

The Impact of Campaign Spending

The results of U.S. elections seem to suggest, if not to prove, the dramatic influence of campaign expenditures on electoral outcomes. Studies of state legislative races (Giles and Pritchard 1985; Owen and Olson 1977; Welch 1976), congressional elections (Jacobson 1984; Hinckley 1983), and state level executive races (Beyle 1983; Jewell 1984; Patterson 1982) all indicate that campaign expenditures have an impact on election results.

Nonetheless, previous analyses of the presidential nomination process have suggested a very modest impact for campaign expenditures. Goldstein (1978, 171) reports that expenditures in the 1976 primaries exhibit a "mixed pattern. . . . In 15 of the 25 cases . . . the winners spent more money than the loser, but in 10 of the cases, a full 40 percent of the total, the loser spent more than the winner." Orren's (1985, 51) early report on the 1984 events concluded that "in the primaries and caucuses of 1984 there were 26 events in which the winner outspent the loser and 26 events in which the loser outspent the winner."

Much of the evidence so far presented

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has been anecdotal or, at best, bivariate considering only the direct relationship between expenditures and electoral outcomes. Although the authors cited above are undoubtedly correct to argue that out-spending an opponent has not traditionally, and did not in 1984, guarantee victory, money has always been, and was in 1984, something of an advantage. We will examine the extent to which campaign spending by each of the candidates affected their electoral performance in both the primaries and the caucuses.

The Influence of Turnout

While scholars disagree about the impact of particular factors on turnout, they have demonstrated that changes in turnout work very differently in primary as opposed to caucus settings. Of the two electoral settings, the caucus results seem to be readily interpretable, while the primary results seem to many scholars to be "thoroughly perverse" (Schier 1982, 239). Schier's recent study (1982, 237) indicates that "the caucus turnout equations clearly demonstrate the explanatory and predictive utility of certain strategic variables (including what we call rules variables) to be superior to that of any demographic variables."

The factors that influence turnout in primary election settings are much less clear. Norrander and Smith (1985, 1) write that past "studies point to several factors which should influence turnout, such as the nature of the campaign, the legal structure of the primary, and the demographic characteristics of the state. However, when considering specific variables that affect turnout, these studies are as likely to disagree as agree." Ranney, in his important study of turnout in the 1976 primaries (1977, 35), concluded that "there is little of interest to report concerning the relationship between the demographic (or the economic) characteristics of the states and their turnouts in presidential primaries." We will measure

the impact of turnout on electoral outcomes in both primary and caucus settings in 1984.

Simple Models of Influences on Candidate Vote Shares

We begin by presenting separate models for each of the six components of explanation for each of the three candidates (both for the primaries and the caucuses). The three major candidates in 1984—Hart, Jackson, and Mondale—represent distinct factions of the Democratic coalition, and therefore we expect the independent variables to perform differently in each candidate's model. We report separate candidate support equations for caucuses and primaries because the strategic environment is different for each type of election system.

In this study, the dependent variable is the proportion of the total vote received by each of the three major candidates in the presidential preference component of a state's primary or the first round of its caucus. We standardize the support for each candidate by taking the percentage of the total vote that he received in the state's caucus or primary.¹ Each regression equation will describe the impact of each dimension on the dependent variable in the model—proportion of the vote.

Generally, the model of the Mondale vote should represent the attributes of a candidate associated with the Democratic coalition as it was forged in the New Deal era—an organization candidate representing a coalition of have-nots battered by hard economic times. Jackson represents the major revision of the Democratic coalition made in 1964 when race became a dividing issue between Democrats and Republicans and when blacks became firmly entrenched in the Democratic camp. Hart, on the other hand, is not associated with have-nots in general or minorities in particular; rather, Hart represents the more affluent voters of neo-

liberal ideology who Ladd (1977; 1982) argues have transformed the Democratic party from the 1960s onward.

Sociodemographic Factors and Candidate Vote Shares

The sociodemographic groups at the core of the Democratic coalition include the poor, blacks and other nonwhites, union members and their families, Catholics and other non-Protestants, Southerners, and city dwellers in the 12 largest metropolitan areas of the nation (Axelrod 1972, 15-17; see also Abramson, Aldrich, and Rohde 1983; Petrocik 1981). Other recent research suggests that the elderly, particularly those socialized to politics during the New Deal era of the 1930s, favor the Democrats (Abramson 1975; Miller and Levitin 1976; Nie, Verba, and Petrocik 1976). Evidence also suggests that Hispanics are part of the Democratic coalition (Abramson, Aldrich, and Rohde 1983; Levy and Kramer 1973). Finally, Ladd's work (1977; 1982) indicates that a group defined as white, young, upwardly mobile professionals may be part of the Democratic party coalition. These three new groups—the elderly, the Hispanics, and the "yuppies"—will be included among the sociodemographic groups in our analysis along with the more traditional groups identified by Axelrod (1972) and others.

Our model includes a total of seven measures of the sociodemographic configuration of each state. They are: (1) proportion of the voting age population that is black, (2) proportion of the voting age population that is Hispanic, (3) labor union members as a proportion of employed persons in non-agricultural occupations, (4) Catholic, Jewish, and other non-Protestant adherents as a proportion of the total population, (5) proportion of the voting age population that is residing in urban areas, (6) proportion of the voting age population aged 65 or older, and (7) proportion of employed

persons between the ages of 30 and 44 with professional or technical occupations.

The existence of a major black candidate in 1984 made the proportion of blacks in a state a central concern of each candidate. We expect Jackson's vote share to have been heavily influenced by the proportion of blacks in a state. Despite Mondale's positive evaluation within the black community, we would expect Jackson's presence to have lead Mondale to do proportionately less well in states with large numbers of black voters. Hart was clearly not associated with the black wing of the party and we would therefore expect to see Hart's vote share diminish rapidly as the proportion of black votes in a state rises.

Jackson also made a clear plea to Hispanics as part of his "rainbow coalition," and Mondale made every effort to include Hispanics in the traditional Democratic coalition of minorities. Gary Hart, whose campaign was geared toward those who were turned off by minority group influence in the Democratic party, would not normally be expected to have done well among Hispanic voters. Nonetheless, because Hart's base was in the West, his vote share might be expected to have shown a positive correlation with percent Hispanic.

Labor union voters, Roman Catholic and Jewish voters, particularly those with strong ethnic ties, and urban voters are central to the traditional Democratic coalition (Petrocik 1981). The early endorsement of Mondale by the teacher unions and the AFL-CIO was a clear confirmation of his close relationship to organized labor. We should see a larger proportion of labor union membership having advantaged Mondale and disadvantaged Hart, while Jackson should have broken even by doing well with the black labor vote. Mondale should also have fared well in the primaries and caucuses of the states with larger proportions of Catholic and Jewish voters. Hart attempted to appeal to non-core groups in

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the party, therefore, he should have done better in states with larger proportions of protestant voters. Jackson's campaign rhetoric and his association with Lewis Farrakan, a prominent and inflammatory black Muslim leader, weakened him among nonprotestant voters, particularly Jews. In addition, we expect Mondale and Jackson to have drawn larger proportions of the vote in heavily urban states. Hart would be expected to have done better in the states with larger proportions of rural voters.

Finally, we include age variables in our sociodemographic model of candidate vote shares. In 1984 a growing proportion of the elderly were politically aware because social security was a major issue. We expect Mondale to have done well in states with a higher proportion of those 65 and over. Hart, on the other hand, appealed explicitly to a younger generation of Democrats and Independents with his "new ideas" approach and should, therefore, have done better in states where the young are present in large numbers. The advantageous effect of the percentage of elderly and "yuppie" votes on Jackson should have been quite modest as blacks young and old would have voted for him, while whites would not have.

The results of the sociodemographic analysis are reported in Table 1. Most of the variables included in the candidate models for the primaries perform in the expected direction with the proportion of black and labor union membership being the dominant sociodemographic influences.

As expected, Hart showed his greatest strength in the primary states with less traditional Democratic constituencies. Hart territory was most clearly defined in negative terms by the absence of blacks and union voters. Viewed positively, Hart territory was white, nonunion, yuppie, and rural. Jackson was well received in states with large black populations. He did not draw significantly from states with higher numbers of Hispanic, non-

Protestant, urban, or union populations, while the elderly and yuppies marginally depressed Jackson support. The Mondale vote was positively affected by the proportion of unionized voters in the state. The presence of other groups had only a modest influence on the size of the Mondale vote share.

In the caucuses we expect some differences in group support for the candidates because the participants tend to be more intensely committed to party, issues, and candidates; these are the activists as opposed to the rank-and-file voters that turn out in larger numbers in the primaries. Generally, additional sociodemographic factors are at work in the caucuses. However, with the exception of the positive relationship between yuppies and the Jackson vote and blacks and the Mondale vote, relationships are generally in the expected direction.

Political Context and Candidate Vote Shares

We include indicators for mass electorate liberalism, Democratic party elite liberalism, strength of the state Democratic party organization, and strength of the local Democratic party organization in our political context equations. These factors define the context, both ideological and partisan, that confronted the candidates when they entered a state. First, we employ an indicator of mass electorate liberalism. We selected the Wright, Erikson, and McIver (1985) measures of state ideology.³ In 1984 the general public associated *liberal* with the New Deal, Great Society, welfare state approach to public problems. States with a large proportion of liberal voters should have provided fertile ground for Mondale and Jackson, though not for Hart.

Party leaders, however, were more likely to see Hart not as less liberal for not espousing traditional Democratic ideas but rather as a *different* kind of liberal. Neoliberal political analysts like Kuttner

(1985, 25) argued that "Hart... marrie[d] the dynamism of the entrepreneurial marketplace to the security implicitly promised by democratic citizenship.... The post-New Deal trick is to devise bargains that offer security without economic rigor mortis." Therefore, we include a measure of state party elite ideology in order to tap potential differences between elite and mass ideology within

the Democratic party. We chose the McGregor (1978) state party liberalism score, which is constructed from state delegate preferences at previous Democratic national conventions.⁴ We expect Mondale, who represented establishment welfare liberalism, and Jackson, who represented more radical implementation of these same ideas, to have fared better in states with ideologically traditional party

Table 1. The Effects of Contextual and Structural Variables on Candidate Vote Shares During the 1984 Democratic Party Nomination Campaign (Standardized Regression Coefficients)

Variables	Primaries (N = 28)			Caucuses (N = 27)		
	Hart	Jackson	Mondale	Hart	Jackson	Mondale
Demographics						
Black	-.69	+.94	-.05	-.65	+.90	+.20
Labor	-.16	+.06	+.61	.00	+.14	+.36
Urban	-.26	+.05	+.17	-.27	-.28	+.28
Hispanic	-.02	+.07	+.16	+.29	+.30	+.01
Non-Protestant	+.07	+.07	-.37	-.36	+.01	+.35
Yuppie	+.22	-.05	-.20	+.10	+.28	+.10
Elderly	+.12	-.11	-.01	-.07	+.24	+.65
R ²	.64	.92	.46	.42	.73	.55
Political context						
Mass liberalism	-.34	+.16	+.22	-.13	+.18	+.32
Party liberalism	+.44	-.50	-.15	+.48	-.59	-.17
Strength of state organization	+.13	-.33	-.06	-.07	-.08	+.02
Strength of local organization	-.04	-.08	+.32	.00	-.13	+.17
R ²	.26	.39	.19	.20	.38	.10
Economic context						
Per capita income	-.26	+.55	-.13	+.18	-.23	+.01
Change in per capita income	+.41	-.11	-.64	+.16	+.04	-.25
Unemployment	-.34	+.48	+.14	-.06	+.30	+.08
Change in unemployment	.00	+.11	+.21	+.06	+.03	+.13
R ²	.49	.45	.38	.09	.16	.08
Rules						
Closed	-.15	+.19	+.14	+.23	-.33	+.41
Delegate selection	+.25	-.05	-.53	-.07	NC	+.30
R ²	.10	.04	.32	.06	.11	.10
Spending						
	+.52	+.76	+.21	+.43	+.49	+.22
R ²	.27	.58	.04	.19	.23	.05
Turnout						
Turnout	NC	-.15	+.26	+.21	-.22	-.03
Change in turnout	-.17	+.38	-.17	-.13	NC	-.06
R ²	.03	.11	.05	.05	.05	.00

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elites. Hart should have garnered a larger proportion of votes in states where the party elite historically has demonstrated a willingness to consider insurgent candidates.

Finally, we include indicators of strength of party organization, developed by Cotter, Gibson, Bibby, and Huckshorn (1984), in order to tap the potential impact of variations in the partisan context on the relative success of the Hart, Jackson, and Mondale candidacies.⁵ Because Mondale was the clear choice of the party elite, we would expect organizational strength to have benefited Mondale at the expense of Hart and, more particularly, at the expense of Jackson.

The estimations of the models for the primaries and caucuses are reported in Table 1. The results are notably weaker than the sociodemographic equations. Nonetheless, mass liberalism performs as expected: in states with a relatively liberal tradition, Jackson and Mondale are strong, while Hart is weak. The results for party elite liberalism show that Hart's neoliberal candidacy was effective where state party delegations had a tradition of supporting outsiders. Mondale, on the other hand, did best in states that traditionally have supported mainstream establishment candidates. Jackson also does well in the traditional Democratic states, though this can largely be attributed to his strength among black voters in the South. Strength of state and local party organization had relatively little impact on candidate vote shares in either the primaries or the caucuses.

Economic Factors and Candidate Vote Shares

One of the more intractable debates within the political economy literature concerns the particular economic conditions (if there are any) that influence electoral outcomes. Specifically, is it *levels* or *changes* in such indicators as income and unemployment that cause voters to

reward or punish specific candidates and parties? Disagreement, even after substantial empirical analysis, is still pervasive (Abrams 1980, 628; Crain, Deaton, and Tollison 1978; Fair 1978, 171).

Therefore, we include both level and change indicators, specifically, the state unemployment rate and per capita income and *change* in the unemployment rate and in per capita income. The levels of unemployment and per capita income are measured three months prior to the time of the caucus or primary. The economic trends are calculated as the difference between the rate twelve months before the event and three months prior to the event.⁶

Low unemployment, a decreasing rate of unemployment, high per capita income, and growth in per capita income are all signs of economic prosperity and should be positively related to Hart support. The reverse of all of these variables are signs of economic hard times and should be positively related to Mondale and Jackson support.

Estimations of the models for the primaries are reported in Table 1. The variables all perform in the expected direction with *change* in income and *level* of unemployment being the most consistent predictors. Our findings confirm that Hart did well in areas that were economically secure; where levels of unemployment were low and levels of income were high, and where rates of change in income were running strongly positive. The Jackson vote share was also profoundly affected by both levels and rates of change in economic performance. Jackson did best in states that had been hard hit by the recession of 1981-83, states in which unemployment was high and income was deteriorating. Like Jackson, Mondale ran best in states with serious economic problems, particularly those industrial states of the upper midwest in which income had declined over the previous year.

Economic factors were substantially

less powerful in influencing levels of candidate support in the caucuses. Change in income was still the most consistent predictor, working positively for Hart and negatively for Mondale and Jackson. High levels of unemployment once again provided fertile ground for the Jackson campaign. These findings for both the primaries and the caucuses strike a blow for the commonsense notion that voters are sensitive both to levels of income and employment and to changes in their rates over time.

Electoral Structures and Candidate Vote Shares

We include indicators for the degree of openness of the primary or caucus and the nature of the process used to allocate convention delegates among the candidates. State parties must decide whether their primary or caucus will be closed to non-party members, open to some voters not previously with the party (such as independents and first-time voters), or open to all of the registered voters in the state.⁷ We expect that closed events (a high score) should have increased the Mondale proportion of the vote since his appeal was directed toward party loyalists. We also expect that since blacks are overwhelmingly registered Democrats, Jackson would have done best in events closed to non-Democrats. Hart, on the other hand, should have had greater success in primaries and caucuses open to Republicans and Independents (a low score).

Rules also govern the impact electoral outcomes have on distribution of the state's convention delegates. Three general delegate distribution plans—winner-take-all; winner-take-more, bonus, or districted plans; and proportional delegate distribution schemes—have been employed by the state Democratic parties and approved by the national party.⁸

Jackson, the consistent third-place finisher, understood quite clearly that his

candidacy was better off under the proportional system (high score). We expect a positive relationship between the delegate selection system indicator and Jackson support in primaries and caucuses. Hart and Mondale both made strong efforts to win votes in states with winner-take-more and winner-take-all primary rules (low score); and hence we expect a negative relationship between these procedures and proportion of vote.

The coefficients for the primaries are reported in Table 1. The models, especially those for Hart and Jackson, are very weak. The Mondale model shows that about 30% of the variance in his vote share is explained by the winner-take-all and winner-take-more rules at work in several of the primaries (Orren 1985, 36). Hart, on the other hand, did better, though only marginally so, in the winner-take-more and proportional primary states. None of the delegate selection rules shows up for Jackson, although he does, as expected, do better in the Democrat-only closed primaries.

The caucus models include some unexpected results. In the caucuses, the Mondale vote and the direct proportionality of the delegate selection procedures are positively related (although the relationship is considerably weaker than the relationship in the primaries equation). Additionally, a closed caucus is more likely to benefit Hart and to harm Jackson.

Campaign Spending and Candidate Vote Shares

We expect the share of the vote for each candidate in the primaries and caucuses to be positively related to the level of their campaign expenditures. We further anticipate that the relationship between vote share and campaign expenditures will be strongest for Hart and Jackson since they put the bulk of their campaign expenditures into state-level activities, while Mondale divided his resources between an

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extensive headquarters operation and spending in the states (Orren 1985). Our campaign expenditure variables measure the money spent by each of the major candidates as a proportion of all state-allocated expenditures by candidates contesting the state event.⁹

The results of our analysis highlight the fact that it takes relatively modest amounts of money to activate a receptive audience. Though Jackson had comparatively little money to spend, where he was able to spend it had great impact because his message mobilized voters who might otherwise have simply stayed home. For instance, fully 58% of Jackson's vote share in primary states is explained by his spending in that state; exactly half that, or 27% of Hart's vote share, is explained by his spending; but only 4% of the variance in Mondale's vote share is explained by his spending, even though he spent more money overall than Hart and Jackson combined.

Turnout and Candidate Vote Shares

We include indicators for turnout level and turnout change in the final component of our model. Turnout is measured by the percentage of Democratic supporters participating in the caucus or primary.¹⁰ Turnout change is measured as the percentage difference in turnout between the 1980 caucus or primary and the 1984 event. Most analysts expected high turnout and turnout surge to have helped the outsider candidates, Hart and Jackson, and to have been negatively related to Mondale's vote share.

The estimations of the models for the primary and caucus cases are reported in Table 1. Neither levels of turnout nor degrees of change had a significant impact on the electoral performance of the candidates in either the primaries or the caucuses. Except for the modest advantage that increased turnout gave to Jackson in the primaries, no turnout variables

explained more than 5% of the variance in candidate electoral success.

An Integrated Model of Influences on Candidate Vote Shares

The simple models of candidate vote shares provide only partial explanations, and therefore, we turn to the development and testing of an integrated model for the caucuses and primaries for each of the three candidates. We present a model in which we combine selected variables from the simple models—sociodemographic, political, economic, electoral, spending, and turnout—in a single analysis.

The sociodemographic dimension includes three variables—urban, black, and labor—that represent the core of each candidate's strength. We expect these sociodemographic factors not only to have had a direct impact on a candidate's vote share but to have influenced it indirectly through impact on the economic and political contexts. The mass liberalism and party liberalism variables are the most powerful of the political context variables and are included as indicators of that dimension. The change in income variable is employed as the indicator of economic context. Political context and economic context should have influenced vote shares directly while affecting electoral structure, campaign spending, and participation indirectly. Electoral structure, measured in terms of the openness of the primary or the caucus, is expected to have influenced vote shares directly and campaign spending and participation indirectly. Finally, campaign spending and turnout change are expected to have had a direct impact on vote shares.

Primaries

The additive models for the primaries are quite powerful, explaining nearly two-

Table 2. The Effects of Selected Contextual and Structural Variables on Candidate Vote Shares during 1984 Democratic Nomination Campaign

Variables	Primaries (N = 28)			Caucuses (N = 27)		
	Hart	Jackson	Mondale	Hart	Jackson	Mondale
Black	-.54	.80	-.10	-.33	.58	-.09
Labor	-.25	.11	.57	-.16	-.07	.45
Urban	-.07	.00	-.06	.03	.05	.14
Change in per capita income	.37	-.07	-.50	-.01	.07	.12
Mass liberalism	.20	.06	-.17	.09	.14	.23
Party liberalism	.13	-.06	-.27	.19	-.01	-.36
Closed rules	-.14	.10	.20	.30	-.25	.09
Spending	.23	.13	-.16	.33	.37	.40
Change in turnout	.10	-.10	.20	.08	-.15	-.07
R ²	.82	.83	.65	.54	.81	.38

thirds of the variance in the Mondale vote and over four-fifths of the variance in both the Hart and Jackson votes. The results are reported in Table 2. The variables perform very much as expected. In this model the relative importance of each of the variables on candidate vote shares in the 28 primaries is much clearer because other relevant variables are controlled.

The integrated model for Hart in the primaries explains 82% of the variance in his proportion of the vote. Hart's vote was still from states with higher proportions of white, affluent, and nonunion voters. In addition, Hart's vote success was facilitated by heavy campaign expenditures in the states, by a liberal mass and neoliberal partisan ideological context, and by moderate increases in turnout. The integrated model explains 83% of the variance in the Jackson vote and highlights the overwhelming, even exclusive, importance of the proportion of blacks in a state to the proportion of Jackson's vote. Once we control for proportion black, other factors show a much more modest influence on the Jackson vote than the simple models suggested. The integrated model for the Mondale vote is less powerful than the models for Hart and Jackson, explaining 65% of the variance

in primary voting. Mondale did best in states with a strong labor movement, a battered economy, and a history of supporting establishment candidates, in which voter turnout was high and participation was restricted to registered Democrats.

Some of the endogenous relationships (not reported here) in the path model are also interesting. In the primary states, we find a moderate explanation of mass liberalism ($R^2 = .35$) and of party liberalism ($R^2 = .38$) by the demographic variables. State demographic characteristics are less powerful predictors of the economic climate in the primary states ($R^2 = .18$). Nonetheless, larger black populations, larger urban populations, and smaller union membership all serve to raise the probability that a state will have a strong economic climate. Mass liberalism, state party liberalism, rules, and economic climate all affect the money available to each candidate in the states. The most consistent predictor of spending is the economic climate. In both primary and caucus states, rising per capita income had a positive effect on Hart spending and a negative effect on the Jackson and Mondale spending.

The integrated model clarifies the relative importance of the forces affecting

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vote proportions in the primaries. The strong performance of demographic variables in the simple models holds up in these models when other influences are taken into account. With Jackson, the candidate most clearly identified with a single demographic group, the influence continues to be almost overwhelming. Mondale's labor support is also striking. Economic climate continues to be prominent in the models for Hart and Mondale. This is indicative of the broad economic group support that each candidate represented and drew upon. This economic force is overshadowed by the importance of black support in the case of Jackson. The integrated model for the primaries demonstrates the predominance of group-defined support in the states.

Caucuses

The integrated models for the caucuses are somewhat less powerful than the models for the primaries, at least in the cases of Hart ($R^2 = .54$) and Mondale ($R^2 = .38$), though the variables in the models work generally as expected. Hart ran strong in the caucus states in which the voters were proportionally more white and nonunion and the elites were more likely to support insurgent candidates. His prospects were enhanced by spending, increases in turnout, and—somewhat surprisingly—by rules restricting participation to Democrats. Jackson found the most success in states where he was able to spend liberally in order to fully mobilize his black constituency. Once the proportion of blacks in the state's electorate and spending are considered, other factors make surprisingly little difference. Mondale's support in the caucuses came from very predictable groups under expected circumstances. Once again, Mondale's life jacket was a strong labor vote in liberal states with a history of supporting establishment candidates. He also benefited slightly by closed caucuses where he was able to

spend enough to activate his adherents.

The endogenous relationships differ very little from those in the primaries. Again, the strongest relationship ($R^2 = .38$) highlights the importance of demographics in explaining party liberalism. Demographics proves to be less helpful in the caucus states than in the primary states in explaining economic climate. Again, as in the primary states, economic climate is a strong predictor of spending.

The main contribution of the integrated model to our understanding of the caucuses is to highlight the striking emergence of campaign spending. Spending, demographics, and economic climate are the most powerful predictors of candidate vote shares. However, the demographic and economic factors assume a smaller role in the case of the caucuses.

Conclusions

Clearly these integrated models tap the systematic way in which primaries and caucuses present candidates with different strategic and tactical problems. A simple way to interpret the differences between the integrated models for the primary and caucus settings is in terms of normal vote analysis. The group structure as well as the economic and ideological climates of the states are long-term effects; they are given over which the competing candidates have very little control. They can comment on them and try to position themselves advantageously in relation to them, but they cannot change them. In the primary states, these relatively intractable long-term effects are of predominant importance. In caucus states, where participation is more restricted, the impact of group structure and ideological climate are muted, leaving a more fluid political situation in which candidate strategy and maneuver may move relatively small numbers of people with decisive effect upon the outcome. Under these more dynamic conditions, short-term influ-

ences like campaign spending and rules assume a much larger role.

Of the sociodemographic groups that previous research has identified as central to the Democratic coalition, two—blacks and labor—registered a powerful influence on outcomes in both the primaries and the caucuses in 1984. Mondale drew heavily on the support of organized labor in both settings, while Jackson drew even more heavily on his black constituency in both settings. Hart forged no close relationships with the major sociodemographic groups of the Democratic coalition. This inability or unwillingness to secure a reliable base within the Democratic electorate left Hart at a substantial disadvantage.

The impact of the mass and elite ideological climate on each of the candidates worked in interestingly consistent ways in both the primary and caucus settings. The integrated model shows that all three candidates benefitted from a liberal mass environment in the caucuses, as did Hart and Jackson in the primaries. The elite party ideological context showed that Mondale ran better in states with a history of fielding traditional liberal delegates. Hart ran better in states that had a history of bolting to candidates who do not fall into the establishment New Deal mold. Jackson's vote—once we controlled for percent black—did not respond to differences in ideological context in either setting.

Our findings also serve to shed considerable light on the long-running dispute initiated by Kramer (1971) and Stigler (1973) concerning the impact of economic performance on electoral outcomes. Our findings show quite clearly that economic performance, measured by change in income, was very important in the primaries but almost totally inconsequential in the caucuses. In the primaries Mondale did well in states with slow or no growth, and Hart did well in states that had enjoyed substantial growth. For both

Hart and Mondale, economic performance was the second strongest variable in the integrated model, behind only the most powerful of the sociodemographic group influences. In the caucuses, on the other hand, Mondale and Jackson were again dependent upon their major sociodemographic support groups and upon the ideological climate, but change in income registered no appreciable impact for any of the candidates.

Campaign spending, a factor directly under the day-to-day control of the candidates and their campaign staffs, increased quite dramatically in importance in the caucus setting. In each case, spending in the caucus events assumes either the first or second place in the model; in Hart's case spending was more important than any of the sociodemographic, economic, or ideology variables; while in the Jackson and Mondale models, spending was second only to the most powerful demographic variables: blacks for Jackson and unionized labor for Mondale.

Clearly, then, the direct estimations for the primaries differ from those for the caucuses in quite dramatic and theoretically interesting ways. In sum, primaries in 1984 tended to be referenda on general economic circumstances as these impinge on broad social groups and as they were interpreted from the dominant ideological perspectives. The caucuses in 1984 tended to turn on the ability of candidates to get their central issues and themes across to their most receptive constituencies with sufficient clarity and power to move them to attend the often complex and time-consuming caucus events.

Finally, direct comparisons of the Mondale, Hart, and Jackson models provide some interesting insights into how key sociodemographic elements of the Democratic party split among these three candidates. Jackson swept the black vote, but beyond that he enjoyed only very modest support from white labor and even less support from white liberals, to say

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nothing of white conservatives. Similarly, the extent to which organized labor was critical for Mondale, while blacks were absent as an active component of his coalition, is quite stark. Hart, on the other hand, was dependent upon economically secure white Democrats. He enjoyed very little support from organized labor or from blacks. The results of 1984 pose a clear challenge to Democratic presidential candidates of the future: the very substantial problem of forging an appeal based on defending established Democratic interest groups, such as blacks and labor, while facilitating economic growth.

Notes

Note: We wish to thank Karen Brown, Shirley Russell, Eric Selbin, and Alison Sondericker for their research assistance, and James C. Garand for his comments on an earlier draft.

1. The election returns for the 1984 Democratic party primaries and caucuses were drawn from various issues of *Congressional Quarterly Weekly Report* (Congressional Quarterly, Inc., Washington, DC). The best single source for both primary and caucus returns is Congressional Quarterly's *Guide to the 1984 Democratic National Convention* (1984, 36, 42) (Congressional Quarterly, Inc., Washington, DC).

2. Data for voting age population, black population, Hispanic population, population aged 65 and over, employed persons in nonagricultural occupations, population living in urban areas, and the number of employed persons between the ages of 30 and 44 with professional and technical occupations were drawn from the 1980 *Census of Population and Housing* (Bureau of the Census, Washington, DC). Data on labor union membership is for 1980 (the latest year available) and was taken from the *Directory of U.S. Labor Organizations* (1982, 68, Table 21). Data on the number of non-Protestants by state is for 1980 and was found in *Churches and Church Membership in the United States 1980* (Glenmary Research Center, Atlanta, GA, 1982, 10-28, Table 3).

3. Wright et al. (1985) have created mean measures of state ideology by aggregating CBS/NYT survey responses to the question, "How would you describe your views on most political matters? Generally, do you think of yourself as liberal, moderate, or conservative?" We have reflected their weighted scores reported in their Table 3 (1985,

478-79) to produce an index with states at the high end being liberal and states at the low end being conservative.

4. McGregor (1978) classifies state party delegations to national conventions according to whether these delegations gave majority support to various liberal and conservative candidates to create cumulative indexes of state party ideology. The state Democratic party ideology indicator measures the number of conventions in which the state delegation supports the most liberal candidate. State delegations were counted as liberal if they gave majority support to Kefauver in 1952, Kennedy, Stevenson, and/or Humphrey in 1960, McCarthy and/or McGovern in 1968, and McGovern and/or Chisholm in 1972. The McGregor state party ideology indicator varies from one (most conservative) to five (most liberal).

5. To determine the strength of party organizations in the states, Cotter et al. (1984) constructed indicators of state and local party organizational strength based on survey responses from party leaders at both levels of the Democratic and Republican parties. We employed their measures as our indicators of state and local Democratic party strength. The specific indicators we used are found in Cotter et al. 1984, Table 2.4 for the state measure and Table 3.8 for the local index.

6. Data on state unemployment rates and per capita income were obtained from *Unemployment in the States and Local Areas* (1984) published by the Bureau of Labor Statistics of the U.S. Department of Labor (for unemployment data) and *Survey of Current Business* (1984), Table 1 (for personal income data).

7. Of the 27 primaries, the Democrats had eight in which any registered voter willing to declare allegiance to the party could participate, four in which only Democrats and Independents could vote, and 15 that were closed. Eighteen of the 28 caucuses allowed any voter declaring allegiance to the Democratic party to participate, while 10 permitted only enrolled Democrats to take part. No states allowed Independents to participate in the Democratic party caucuses. States allowing any voter declaring a Democratic preference to vote in the primary or participate in the caucus were coded one, those allowing both registered Democrats and Independents to take part were coded two, and states allowing only registered Democrats to participate were coded three. Data on the participation rules of the primaries and caucuses were obtained from issues of the *Congressional Quarterly Weekly Report* in late 1983 and early 1984 that contained preview articles on each of the state nomination contests.

8. Of the 51 events that determined directly or indirectly the number of delegates to be awarded to the Democratic candidates, 7 were winner-take-all contests, 10 held winner-take-more events, and 34 states used rules under which the available delegates

were divided proportionally among all of the candidates, reaching a threshold usually in the 10% to 20% range. The delegate allocation procedures were coded as follows: winner-take-all = 1, winner-take-more = 2, and proportional = 3. Data on the allocation rules used by the state Democratic parties were obtained from issues of the *Congressional Quarterly Weekly Report* in late 1983 and 1984 that contained preview articles on each of the state nominating contests.

9. Data on the campaign spending activities of the candidates contesting the Democratic party nomination were derived from the spending reports filed by each campaign with the Federal Election Commission (FEC): "Allocation of Primary Expenditures by State for a Presidential Candidate" (1985). We wish to thank Michelle Broussard of the Office of Public Records of the FEC for her assistance in obtaining the reports of the seven major candidates.

10. Data on the number of persons participating in the 1984 Democratic party primaries and caucuses were taken from *Congressional Quarterly Weekly Report*, June 2, 1984, 1317, and June 16, 1984, 1443, as well as updated reported by Orren (1985). To calculate the proportion of Democrats participating in either the primary or a caucus, we needed to derive an estimate of the number of Democratic party supporters in each state. This was accomplished as follows by using the work of David (1972) as a guide: we averaged the number of votes in each state cast in the general election for the Democratic candidate for the office of President in 1976 and 1980 and for the U.S. Senate seat and governorship in the two most recent general elections for each office. This raw number of Democratic party supporters was then used to calculate the proportion of Democratic voters turning out in either the primary or the caucus.

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THE INSTITUTIONAL FOUNDATIONS OF COMMITTEE POWER

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Legislative committees have fascinated scholars and reformers for more than a century. All acknowledge the central strategic position of committees in legislatures. The consensus, however, centers on empirical regularities and stylized facts, not on explanations. We seek to explain why committees are powerful. We formulate an institutionally rich rational-choice model of legislative politics in which the sequence of the legislative process is given special prominence. Committees, as agenda setters in their respective jurisdictions, are able to enforce many of their policy wishes not only because they originate bills but also because they get a second chance after their chamber has worked its will. This occurs at the conference stage in which the two chambers of a bicameral legislature resolve differences between versions of a bill. A theory of conference politics is offered and some evidence from recent Congresses is provided.

Legislative committees have fascinated scholars and reformers for more than a century. The early treatise writers (Bryce 1893; McConachie 1898; Wilson 1885), reformers early and modern (Bolling 1965; Norris 1945), and contemporary scholars (Eulau and McCluggage 1984; Smith and Deering 1984) all acknowledge the central strategic position of committees in legislatures. Differences of opinion concerning the role of committees persist, but there is a substantial consensus on a number of stylized facts:

Committees are "gatekeepers" in their respective jurisdictions.

Committees are repositories of policy expertise.

Committees are policy incubators.

Committees possess disproportionate control over the agenda in their policy domains.

Committees are deferred to, and that deference is reciprocated.

There is, however, a troublesome quality to this consensus. The items in this list (and there could undoubtedly be more) describe or label committee power, but they do not explain it. Explanations of these empirical regularities require a theory. In the case of each of these stylized facts, that is, a theory is needed to determine why things are done this way. In many cases it is insufficient to refer to institutional rules because many of the practices alluded to above either are not embodied in the rules at all or have evolved from them only slowly. It is therefore necessary to begin the theoretical analysis from first principles.

There is an added advantage to a theory that begins with first principles: although formulated to accommodate some stylized facts, such a theory will

yield new implications so that it may be employed as a discovery procedure. Consider some anomalies that the theory we formulate below can explain:

In a bicameral system, how is it possible that change in the composition of a committee or a majority in one chamber is sufficient to lead to policy change (Weingast and Moran 1983)?

Why are explicit procedures in the House of Representatives to diminish the gatekeeping monopoly of committees (specifically the discharge petition) rarely employed; and when they are employed, why do they rarely result in law?

How is it that committees maintain their influence over policy change when, once they "open the gates" by bringing forth a proposal, majorities can work their will in ways potentially unacceptable to the proposing committee?

Why do members appear to defer to committees, even to the point of defeating amendments to committee proposals that have clear majority support?

Our explanation for these stylized facts and anomalies emphasizes the enforcement of agreements and arrangements. The legislative world is one in which agreements are forged among autonomous agents. But it is a world lacking instruments or institutions that exogenously enforce such agreements (Axelrod 1981, 1984; Laver 1981). Agreements and arrangements, therefore, are subject to cheating, renegeing, and dissembling. When an arrangement persists over long periods—long enough to allow students to regard it as a relatively robust empirical regularity—then either it is cheat-proof and self-enforcing, in the sense that no one has any motive to depart from the arrangement, or there exists a, sometimes subtle, *endogenous* enforcement mechanism. Although the logic of self-

enforcement may apply, we believe that there is much to be learned from a theory incorporating explicit enforcement mechanisms.

In our view, the explanation of committee power resides in the rules governing the sequence of proposing, amending, and especially of vetoing in the legislative process. We demonstrate a surprisingly important role for the last stage of the legislative process, the conference procedure, in which bicameral differences are resolved. The *ex post* adjustment power conferred on committees in this forum provides them with subtle yet powerful means to affect the voting and proposing power of other members on the floor during the earlier legislative stages. Indeed, we show that the deference given committees on the floor is a natural consequence of the *ex post* adjustment powers wielded by committees in conference.

In the first section of this paper we briefly describe some alternative theoretical explanations of committee power. In each instance, we make explicit what we regard as the kernel of truth it contains, but we also point out crucial missing elements that ultimately render it incomplete. We provide the basic concepts of our own explanatory framework in the second section. In the next two sections, we develop the logic of committee enforcement emphasizing the importance of the manner in which the various stages of legislative deliberation are sequenced. In the fifth and sixth sections we provide both theoretical and empirical detail on the institutionalization of enforcement in the conference committee procedure. In the last section we pull our arguments together and address some extensions and applications.

Theoretical Foundations of Committee Power

A number of ideas exist in the traditional legislative literature about the foun-

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dations of committee power; some of these are at least a century old. A young legislative scholar in 1885, for example, characterized the veto power of congressional committees by referring to them as "dim dungeons of silence" (Wilson 1885, 69). As Bryce described it a few years later, "a bill comes before its committee with no presumption in its favour but rather as a shivering ghost stands before Minos in the nether world" (Bryce 1893, 157). At about the same time, the minority leader and soon-to-be Speaker of the House, Thomas Brackett Reed, emphasized another aspect of committee power—the advantages of information and expertise. He referred to the typical House committee as "the eye, the ear, the hand, and very often the brain of the House. Freed from the very great inconvenience of numbers, it can study a question, obtain full information, and put the proposed legislation into shape for final action" (cited in MacNeil 1963, 149). A third important aspect of committee power is proposal power. Although employed only occasionally in the very first Congresses, the practice of referring bills to a standing committee and not debating them in the full House until reported by that committee evolved during the period of the Clay speakership (1811–25). By 1825 it had become standard operating procedure in the House; and in the twentieth century, with rare exception, bills originate in committee.

Taking some liberties, then, we may describe the foundation of committee power as consisting of gatekeeping, information advantage, and proposal power. Underlying these is a system of deference and reciprocity, according to which legislators defer to committee members by granting them extraordinary and differential powers in their respective policy jurisdictions.

What is amazing about these foundations of committee power is that nowhere are they carved in granite. Committees, as an empirical matter, are veto groups that

may choose to keep the gates closed on a particular bill. But parliamentary majorities have recourse to mechanisms by which to pry the gates open, the discharge petition being only the most obvious. Why, then, do parliamentary majorities only rarely resort to such mechanisms? That is, why does the system of deference to committee veto judgments survive?

The question of survival also arises concerning information advantage and proposal power. As empirical matters, these are robust regularities. Yet the Speaker of a contemporary Congress is relatively free to break any alleged monopoly of proposal power held by committees through his right of recognition in House proceedings, his referral powers, his control of the Rules Committee, and his power to create ad hoc and select committees for specific purposes. Likewise, the contributions to information and expertise from the lobbyist denizens of Washington's "K Street Corridor" and an expanded congressional staff system mitigate the alleged informational advantages of committees.

Several reasons may be put forward to explain how a cooperative system of reciprocated deference is nevertheless sustained. The first and least persuasive is that no one ever has any reason to challenge it. The committee system and its division of labor, it might be alleged, are so successful in parcelling work that anyone interested in a particular subject easily obtains membership on the committee that deals with it. Under these circumstances, deference becomes self-enforcing because there are no incentives to upset the applecart. Needless to say, this explanation denies or ignores interdependence among policy areas, fiscal dependencies, and the prospect that some issues—trade, energy, and health, for example—are not amenable to a neat division-of-labor arrangement because their incidences are both substantial and pervasive.

A second, related rationale to explain deference is not so sweeping. It suggests that while the matching up of work with interested members through committee assignments is not perfect, it is nevertheless sufficient to discourage violations of reciprocity (Shepsle 1978). This view, recently popularized in more general setting by Axelrod (1984), argues that the long-term advantages of deference outweigh the occasional short-term disappointments and so serve to maintain the system.

To sum up, the argument for deference to committees claims that the benefits to be secured by violating deference and challenging a committee are either small (as in the first rationale) or not worth the costs (as in the second rationale). We believe this argument is incomplete and that its premises are not always plausible. There are, first of all, too many opportunities in which it is worthwhile to oppose (or to be seen to oppose) committee positions (Weingast and Marshall 1986). Second, the terms of deference to committees are extremely vague. Third, the behavioral forms violations may take range from minor opposition (say, going on record as having some doubts about a committee bill) to major revolt (introducing a "killer" amendment or initiating a discharge petition). In short, the concepts of reciprocity and deference are at best convenient terms of discourse. Their very vagueness, combined with what we believe are frequent and compelling occasions in which a legislator will not wish to honor them, greatly reduces the power of self-enforcement as an explanation of committee power.

The puzzle of committee power remains. The idea of deference as a form of self-enforcing *ex ante* institutional bargain among legislators cannot account for the disproportionate influence of committees in their respective jurisdictions because it cannot explain away the temptations to defect from the bargain. To be persuasive,

deference must be sustained by more explicit enforcement mechanisms. We discuss three such mechanisms that committees employ to bolster their institutional influence: (1) punishment, (2) *ex ante* defensive behavior, and (3) *ex post* defensive behavior.

A committee may discourage opposition to its actions (or nonactions) by developing a reputation for punishing those who oppose it. The current chairman of the House Ways and Means Committee, a Chicago machine Democrat who knows how to keep score, was once reported to have said of a particularly obstructionist colleague, "I wouldn't support anything he wanted, even if the deal was for everlasting happiness" (personal interview). There is also the now classic story of the efforts by Senator James Buckley of New York to reduce the scale of the nefarious Omnibus Rivers and Harbors Bill. With the "help" of the Chairman of the Senate Public Works Committee, Buckley's assault on the pork barrel produced only one result—the striking of a project for the state of New York (Reeves 1974). These anecdotes aside, it would appear that the capacity to punish and the general use of a tit-for-tat strategy by the committee provide precisely the basis for the emergence of the cooperative relationship between a committee and the rest of the parent chamber so elegantly described by Axelrod (1981, 1984).

This explanation, in our view, is most convincing in the distributive politics realm in which the committee's bills are (1) of significance to a substantial number of legislators, (2) disaggregatable by legislator, and (3) introduced on a regular basis. The first condition requires that there be some prospect for punishing any given legislator in a manner that the legislator and his or her district cannot ignore—a condition not met by some highly specialized committees like Agriculture or Merchant Marines and Fisheries. The second requires that the means to punish

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be available so that threats are credible. The third requires that occasions to punish be readily available. For many committees, punishment of this sort is available only in blunt form, if it is available at all.

A committee may induce cooperative, deferential behavior not only by (threats of) ex post punishment but also by ex ante accommodation. Surely a committee tries, when putting a proposal together, to anticipate what will pass in the parent chamber. Similarly, it will weigh reactions to its killing a bill before actually doing so. Such anticipatory behavior, however, is hardly a basis for committee power but rather is an indication of its limitations. There are other noncommittee groups that share veto power with a committee and may use that power against committee proposals. Majorities may "veto" committee bills by voting them down. The Rules Committee in the House may refuse to grant a rule for a committee bill, thereby scuttling it. The Speaker may use his power to schedule legislation and to control debate in ways detrimental to the prospects of a committee bill. A small group of senators in the U.S. Senate may engage in filibuster and other forms of obstruction. Any individual senator may refuse unanimous consent to procedures that would expedite passage of a committee bill. In short, veto groups are pervasive in legislatures; committees are but one example. Consequently, ex ante defensive behavior by committees, necessary though it may be owing to the existence of other veto groups, cannot be regarded as an influence mechanism; rather it constitutes a recognition of the influence of others.

Having greatly qualified the significance of self-enforcing reciprocity as an explanation of committee power, we have sought more explicit enforcement mechanisms. We acknowledge a role for ex post punishment and ex ante defensive behavior. But neither strikes us as an

entirely satisfactory enforcement mechanism because the conditions for the use of punishment are not met in all circumstances and ex ante defensive behavior accommodates the interests of others rather than enforcing a committee's own desires. There is, however, a third mechanism with which a committee maintains its dominance as veto group and primary policy proposer in its jurisdiction: ex post defensive behavior. We believe this to be the most potent enforcement mechanism and the least understood or appreciated.

Suppose a committee possessed an *ex post veto*. Suppose that, having molded a bill and reported it to its chamber and having allowed its chamber to "work its will," a committee could then determine whether or not to allow the bill (as amended, if amended) to become law (or, in a bicameral setting, to be transmitted to the other chamber). *The ex post veto, we assert, is sufficient to make gatekeeping and proposal power effective, even though their effectiveness appears to most observers to be the product of nothing more than informal reciprocity arrangements.*

Consider gatekeeping first. Suppose that some legislative majority could, by a discharge petition or some other bullyboy tactic, threaten to pry the gates open. If there were an *ex post committee veto*, then (aside from symbolic position taking) there would be little point to this sort of exercise. The *ex post veto* would ensure that changes in the status quo adverse to the interests of a decisive committee majority could be denied final passage. Indeed, the history of the discharge petition suggests precisely this. Even on those relatively rare occasions when a discharge petition obtained the necessary support (218 signatories), the bill of which the committee was discharged almost never became law.

Now consider proposal power. Imagine a major amendment to a committee proposal favored by a chamber majority but

opposed by a committee majority. The amendment might or might not pass, but surely even its most ardent proponents would have to consider whether the amendment were distasteful enough to the committee to trigger an ex post veto. The existence of an ex post veto would encourage the amendment proponents to work out a deal in advance with the committee, would lead to a pattern in which most successful amendments were supported by a committee majority as well as a chamber majority and, in those few instances where anticipation did not discourage amendments obnoxious to the committee, would trigger such a veto.

In the remainder of this paper, we explore in an analytical fashion the ex post veto as the enforcement mechanism that allows reciprocity and deference to work smoothly. Although our model is abstract and thus is consistent with any number of different operational forms of an ex post veto, we argue that the conference procedure, in which differences in legislation between the chambers of a bicameral legislature are resolved, provides the kind of forum in which committees get a "second crack" at a bill. We believe this kind of ex post enforcement mechanism clarifies and explains why various forms of cooperation work in legislatures such as the U.S. Congress despite their transparent fragility and vulnerability.

General Framework

We employ the well-known spatial model of committee decisions, so let us here briefly review its central ingredients. The legislature consists of n agents ($N = \{1, \dots, n\}$), each possessing well-defined preferences (continuous and strictly quasiconvex) over the points of an m -dimensional Euclidean space. We assume the space is partitioned into *policy jurisdictions*: X , a k -dimensional subspace of R^m , is a typical jurisdiction. Similarly, we assume N is partitioned into commit-

tees, with $C \subset N$ the committee whose jurisdiction is X . We shall assume that agent preferences are *separable by jurisdiction* so that we may focus exclusively on X . Thus, in X , agent i has ideal point x_i and his or her preferences are representable by strictly convex indifference contours.¹ For any $x \in X$, agent i 's preferred-to set is defined as

$$P_i(x) = \{x' \in X | x' >_i x\},$$

where $>_i$ is i 's preference relation. $P_i(x)$ is simply the convex set bounded by i 's indifference curve through x ; it contains all the points preferred by i to x . If D is the class of decisive majority coalitions so that $A \in D$ means $|A| > n/2$, then we may define the *win set* of x as

$$W(x) = \{x' | x' >_i x \text{ for all } i \in A \text{ for some } A \in D\} = \bigcup_{A \in D} \bigcap_{i \in A} P_i(x).$$

In words, $W(x)$ is the set of alternatives in X that command majority support over x . Finally, we denote a distinguished point, $x^0 \in X$, as the *status quo*.

We note in passing the best known characteristic of the spatial pure majority rule model we have just described: for almost every configuration of preferences and any $x \in X$, $W(x) \neq \emptyset$. That is, except under highly unusual circumstances, no alternative is unbeatable. This property of win sets ensures that certain sets we describe below are nonempty.

We endow the committee C in jurisdiction X with certain agenda powers. Throughout we assume that C is a *monopoly gatekeeper* in X . No change in x^0 may transpire unless C comes forth with a proposal.² That is, C has *ex ante veto power*. Second, C has *monopoly initiation power*: (1) changes in x^0 in jurisdiction X must first be proposed by C ; but, (2) once a proposal is made by C , competing proposals (normally in the form of amendments to C 's proposal) may be offered by others. Monopoly initiation power is proposal power under an *open rule*.

Finally, a committee that may withdraw one of its proposals after it has undergone modification on the floor or that is empowered to modify further or reject such proposals in some other forum (say, in a conference proceeding with its counterpart in the other chamber of a bicameral legislature) is said to possess *ex post veto power*.

In describing the various agenda powers of committees, we have in mind a specific sequence of decision making in X . Committee C may initiate the legislative process by proposing a bill to alter the status quo, x^0 . Some potential proposal, x , may make a decisive committee majority worse off compared to x^0 , that is, $x \notin P_c(x^0)$.³ In this case the committee will not bring forth the proposal but instead will exercise its *ex ante* veto power by keeping the gates closed.

If, on the other hand, there is a proposal $x \in P_c(x^0)$, which also passes on the floor, that is, $x \in W(x^0)$, then the committee might wish to bring forth a proposal. However, it is confronted with an ambiguous prospect. Should it open the gates by proposing $x \in P_c(x^0) \cap W(x^0)$, it is entirely possible that x will be amended and that the final outcome $x' \in W(x^0)$ will have the property that $x' \notin P_c(x^0)$. Thus, by opening the gates the committee could get "rolled" on the floor and left worse off than it would have been had it kept the gates closed (Denzau and Mackay 1983). If, however, C has a second move in the sequence, that is, C has an *ex post* veto, then it can protect itself from getting rolled on the floor (or restore the status quo if it does get rolled) and can influence the strategic moves of agents at earlier stages of the process.

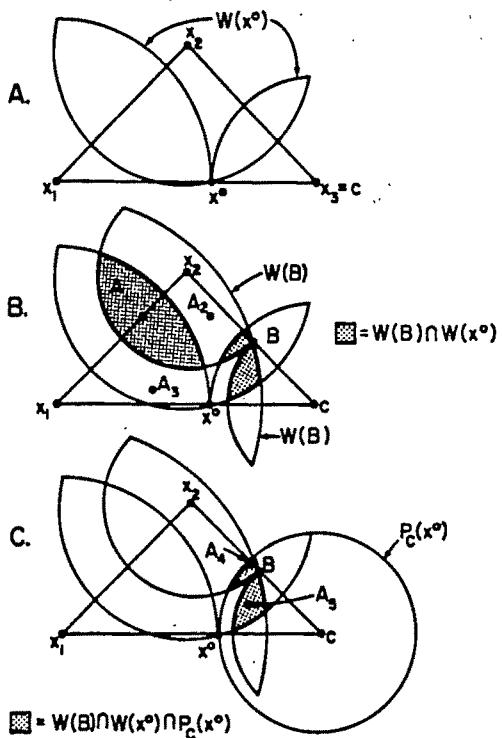
In our thinking about the institutional foundations of committee power, we place great weight on the implications of sequencing. A committee with only the power to move first—by opening the gates or keeping them closed—essentially possesses only blocking power. Once it

opens the gates almost anything can happen,⁴ and the committee is virtually powerless to alter the subsequent path. In contrast, a committee with powers at subsequent stages, especially the penultimate stage, not only affects the subsequent outcome but also influences the antecedent actions of others by conditioning their beliefs and expectations.

Rolling the Committee: Limitations of Gatekeeping and Initiation Power

To provide more precise intuition, we develop an example that illustrates committee power under various proposal and veto conditions. Figure 1a presents a three-person legislature, operating in a two-dimensional jurisdiction by majority rule. Agent 3 has various committee

Figure 1. Effect of Ex Post Veto



powers. The points x_1 , x_2 , and x_3 are agent ideal points and, to simplify the figure, we place the status quo, x^0 , on the 1-3 contract locus; our argument does not depend on this feature. The set $W(x^0)$ consists of two "petals" that are composed of the points preferred to x^0 by the floor majorities {1,2} and {2,3}, respectively.

Ex ante veto power alone is strictly a defensive tool. If x^0 lies close to x_3 , then the committee can prevent subsequent change by blocking any proposal. Figure 1b has the same setup as in Figure 1a, along with various motions— B , A_1 , A_2 , A_3 . We have also identified both $W(x^0)$ and $W(B)$. At x^0 a motion like $A_1 \in W(x^0)$ will be vetoed by the committee (thereby frustrating the preferences of a majority) since A_1 is less preferred than x^0 . Should the committee bring forth a motion like B (since $B \in P_c[x^0]$), it foregoes any future influence on the course of events. Once the gates are opened with the motion B , amendments like A_1 are in order and, in this example, $A_1 \in W(B) \cap W(x^0)$. Thus, any point in the shaded region of Figure 1b, like A_1 , could result since all such points defeat both B and x^0 in majority contests. (We assume here the *amendment procedure*, according to which an agenda consisting of x^0 , some bill B , and amendments A_1, \dots, A_t are voted in pairwise fashion in reverse order with the losing alternative deleted.) In sum, the ex ante veto—the power to bring motions to the floor or bottle them up—is a defensive tool and, while it might be valuable to the committee because of its potential threat value, it cannot assure very much for the committee.

Joining monopoly initiation powers to the ex ante veto does not improve matters much for the committee. Initiation power allows the committee to propose points like $B \in P_c(x^0)$. But such bills, once proposed, take on a life of their own over which the committee has little subsequent control. Indeed, as we have just seen, B is vulnerable to an amendment like A_1 since

A_1 can beat $B(A_1 \in W(B))$, and then it can defeat the status quo ($A_1 \in W(x^0)$). Since $A_1 \notin P_c(x^0)$, a committee proposal can lead to a decline in committee welfare precisely because the committee has no future control once it opens the gates.⁵

We have shown how limited gatekeeping and initiation power are as instruments of committee control. The former is essentially negative, and the latter provides no guarantees unless expanded to proscribe all amendments to or modifications of committee proposals (closed-rule environment). Inasmuch as we rarely encounter legislatures, empirically, that prohibit modifications of committee proposals altogether, we are left with the conclusion that committee power is essentially negative. Any attempt by the committee to promote positive changes in an open-rule environment invariably results in the possibility of a decline in committee welfare. For any committee bill, B , that is, it is almost always the case that $W(B) \cap W(x^0) \neq \emptyset$ (i.e., committee bills are vulnerable to amendment); however, because $W(B) \cap W(x^0) \subset P_c(x^0)$, amended committee bills may leave the committee worse off. In short, once the committee opens the gates, it risks getting rolled on the floor.

Rather than jumping to the conclusion that it is inevitable that a committee will get rolled if it opens the gates, let us make some finer distinctions. It is almost always the case that no matter what proposal (B) a committee offers, there are successful modifications to it that may be offered and agents with the incentive to do so ($W(B) \cap W(x^0) \neq \emptyset$). However, such modifications need not harm the committee. In Figure 1c it may be seen that $[W(B) \cap W(x^0)] \cap P_c(x^0)$ is nonempty. The two shaded regions comprise the locus of modifications in B that both pass the legislature and leave the committee better off than with x^0 . An amendment like A_4 will still be opposed by the committee (because $A_4 \notin P_c(B)$) but

is nevertheless an improvement over x^0 in the committee's preferences ($A_4 \in P_c(x^0)$). An amendment like A_5 will actually be supported by the committee ($A_5 \in P_c(B) \cap P_c(x^0)$). Thus, it is entirely possible for a committee with gatekeeping and initiation powers to enhance its welfare, even in the absence of a closed rule. But there is nothing inevitable about it: while a committee might actively promote and support modifications like A_6 , and ultimately accept modifications like A_4 , there is nothing to prevent amendments like A_1 , and there are strong incentives on the part of majority coalitions like {1,2} to push for them.

Ex Post Veto Power

Suppose now that a committee possessed ex post veto power in addition to gatekeeping and initiation powers. Once it has opened the gates and made a proposal and after the legislature has worked its will, either accepting the proposal or modifying it in some germane fashion, the committee now may either sanction the final product or restore the status quo, x^0 . A committee with an ex post veto possesses the power to protect itself against welfare-reducing changes in the status quo. The ex post veto shares with the ex ante veto this defensive property. But because of its position in the sequence of decision making, the ex post veto confers offensive capabilities as well. Coming last in the sequence, it affects prior beliefs and behavior of other agents.

In Figure 1b suppose that committee bill B stimulates the floor amendment A_1 . As noted earlier, $A_1 \in W(B) \cap W(x^0)$, with Member 1 and Member 2 preferring it both to B and to x^0 . However, $A_1 \notin P_c(x^0)$ so that, with the ex post veto, if A_1 passes, then the committee will veto it and reinstate x^0 . A vote for A_1 , then, is in reality a vote to maintain the status quo. But both members 2 and 3 prefer B to x^0 . Thus, despite a nominal preference for A_1 over B , Member 2 finds the prospect of an ex

post veto a credible threat and joins with Member 3 in defeating all amendments like A_1 . In short, while an agent like 1 has every incentive to move an amendment like A_1 against B , sophisticated calculation induced by ex post veto power leads Member 2 to depart from a nominal preference for A_1 and vote against it.

The ex post veto ensures that the final outcome will either be x^0 or an element of $P_c(x^0)$. It therefore protects a committee from being rolled on the floor. One would expect, as a consequence of ex post veto power, that many amendments nominally supported by legislative majorities will not pass on the floor if they are opposed by a committee majority. Such is the case for all amendments in the one shaded petal of Figure 1b containing A_1 . Opposed by the committee, such amendments will be voted down by sophisticated majorities.

The ex post veto does not protect against amendments in the shaded regions containing A_4 and A_5 (see Figure 1c) because the veto threat is no longer credible there. In these instances, the final outcome is still superior to x^0 in the committee's preferences. The committee may bluster, but it will not veto. Thus, some amendments (like A_4) will pass despite committee opposition, and others (like A_5) will pass with committee support. These amendments turn out to be non-problematical for committees, as we show in the next section.

There is one aspect of behavior induced by credible threats of ex post veto (such as the case of A_1) that bears further discussion. As we related in the introductory section, much is made in the congressional literature of a system of reciprocated deference. But why is deference practiced at all? Is deference unqualified and honored always and everywhere? Our predictions provide a more discriminating explanation of this aspect of deference (or the appearance of it) than does the more traditional lore. In the case of an amendment like A_1 , Member 2 may

appear to defer to the committee by voting against the amendment despite a sincere preference for it; indeed, Member 2 may rationalize his or her own behavior in this way. Thus, one might wish to label this behavior *deference*. But it should be clear that it is *deference to the ex post veto power of the committee, not deference to expertise or an instance of reciprocal cooperation*. In the absence of an ex post veto, we would not always expect to see deference by Member 2; rather, if the committee opened the gates in the first place, we would expect to see members 1 and 2 support an amendment like A_1 . Likewise, even with an ex post veto, there are some amendments to a committee bill (even some opposed by the committee) for which no deference at all will be observed. An amendment like A_4 , for example, will find majority support and no deference because the veto threat is not credible here. In our view deference is endogenous, is not everywhere applicable, and is most usefully thought of as a reflection of the strategic character of a situation. It is a property of a sequential equilibrium (Kreps and Wilson 1982).

In this light, the anomaly begging for explanation is not Member 2's counter-intuitive, seemingly deferential behavior but rather why motions like A_1 are ever made in the first place. We can allude to the symbolic position taking of Member 1 in moving A_1 , but this is surely not a very deep explanation. A more promising view incorporates the fact that agents, like Member 2, may not always be in a position to vote strategically (Denzau, Riker, and Shepsle 1985). In moving A_1 , Member 1 seeks to defeat B with a "killer amendment" that he or she knows will ultimately trigger a veto and the reinstatement of x^0 . Member 1 exploits Member 2's inability to cast a strategic (read: "deferential") vote. If Member 2 is not disabled in this way, then when A_1 is moved and defeated, we believe strategic recognition of the ex post veto is an explanation

superior to arguments about deference.

The discharge petition may be thought of in very similar terms. Suppose the original bill were A_1 and assigned to the committee of Figure 1b. Clearly, the committee's disposition is to keep the gates closed and not report A_1 . Since $A_1 \in W(x^0)$, a majority has an incentive to discharge the committee of its jurisdiction over this bill. Why, then, is the discharge mechanism rarely resorted to? And when it is employed, why does it rarely result in law? The ex post veto provides an explanation. Discharge petitions are often not worth the effort because of the strategic realities. While they get around the ex ante veto, they do not affect the ex post veto. So long as the committee gets to take a crack at the bill after its chamber has worked its will, it is in a strong position to affect the course of its chamber's deliberations. Once again, it is strategic calculation, not deference, that provides the more compelling explanation.⁶

Institutionalization of the Ex Post Veto: Conference Committees

In the United States Congress, as in most state legislatures, a bill must pass both chambers of the legislature in precisely the same form before it may be sent to the chief executive for his signature. Should a bill pass in different forms in the two chambers, a process is set in motion to reconcile differences.⁷ After the second chamber has acted on a bill, the first chamber may "concur" in the second chamber's amendments. If, instead, the first chamber "disagrees" with the second chamber's amendments (or concurs in those amendments with further amendments of its own), then the second chamber may "recede" from its original amendments (or concur in the first chamber's new amendments). Or it may, in turn, concur in the first chamber's new amend-

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ments with its own new amendments, putting the ball back in the first chamber's court. Although this process, known as *messaging between the chambers*, cannot continue indefinitely, the bill can be sent back and forth several more times in the hope that one of the chambers will accept the final position of the other. However, once a stage of disagreement is reached in which one chamber "insists" on its version of the bill and the other chamber disagrees, then one chamber requests a conference, and the other chamber accepts. While as many as three-fourths of all public laws manage to avoid the conference stage, nearly all major bills—appropriations, revenue, and important authorizations—end up in conference.

There is now a considerable body of rules and commentary on conference proceedings.⁸ Conferees of each chamber (also called *managers*) are appointed by the presiding officer; these appointments come principally from the committees of jurisdiction at the suggestion of those committees' chairpersons (some evidence is provided below). Occasionally an additional conferee is appointed to represent a particular amendment that the presiding officer (in the House) believes will not otherwise be fairly represented (like A₁ in Figure 1); but even in this exceptional case, the views of the committee chairpersons are dominant.⁹ The conferees from each chamber seek to resolve differences in the respective versions of the bill, and an agreement is said to be reached when a majority of *each* delegation signs the conference report.¹⁰ If both sign, the report and accompanying bill containing the agreement are brought back to each chamber to be voted up or down (no amendments are in order). That is, the conference report is considered under a closed rule as a take-it-or-leave-it proposal.¹¹

The conference procedure, described in simplified fashion in the preceding two paragraphs, thus does two things. First, it

institutionalizes the *ex post veto* and, as described in the previous sections, gives credibility to the committee during floor deliberations in its chamber. Second, to the extent that there is some discretion on the part of conferees on the terms to which they may agree (see below), the take-it-or-leave-it treatment of conference reports confers additional *ex post adjustment power* on the committee. It is to this latter consideration that we now proceed.

We begin with the jurisdiction X , which we assume is common to both the House and the Senate, and the status quo $x^0 \in X$. Four sets in X are of interest:

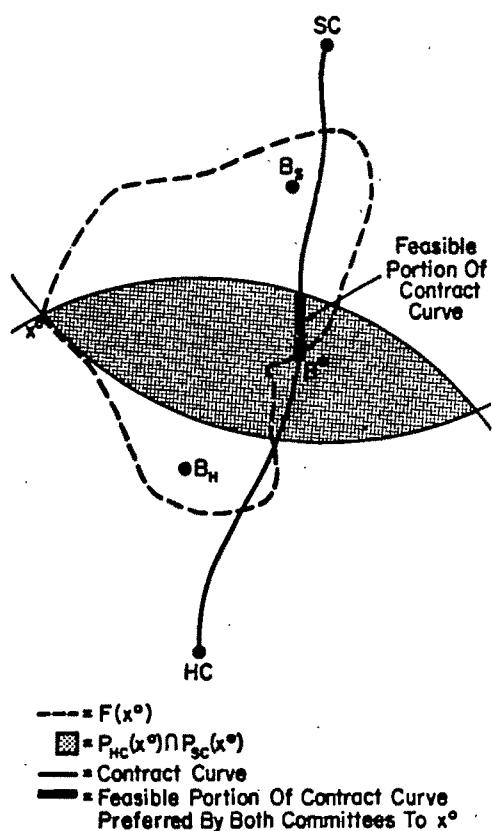
1. $W_H(x^0)$: win set of x^0 in House
2. $W_S(x^0)$: win set of x^0 in Senate
3. $P_H(x^0)$: preferred-to set of x^0 of House committee
4. $P_S(x^0)$: preferred-to set of x^0 of Senate committee

We have already seen from Figure 1 that the final outcome must be an element of $W_H(x^0)$. House majorities constrain changes in x^0 . Likewise, in the Senate $W_S(x^0)$ is a constraint set. To pass, therefore, the conference outcome must be an element of $F(x^0) = W_H(x^0) \cap W_S(x^0)$. The status quo, x^0 , may be imposed by either conference delegation (which we assume to be the relevant legislative committee in each chamber) if a proposed settlement is not an element of one of their preferred-to sets, that is, $P_H(x^0)$ or $P_S(x^0)$, respectively. Thus, we have as a necessary condition for a change in x^0 the following set inequality:

$$F(x^0) \cap P_H(x^0) \cap P_S(x^0) \neq \emptyset.$$

The *ex post veto* power of a committee follows from the fact that it represents its chamber in conference proceedings and may refuse to agree to a conference settlement. If the preferences for change of the House and Senate committees, for instance, fail to intersect ($P_H(x^0) \cap P_S(x^0) = \emptyset$), then any proposed change will be

Figure 2. Conference Committee Bargaining



vetoed by one of them. Similarly, if changes preferred in common by the two delegations fail to intersect the feasible set, $F(x^0)$, then no alteration of x^0 is possible.

We are not yet in a position to model conference proceedings explicitly, but on the basis of the set inequality above, there are some additional points to be made about the opportunities the conference mechanism presents to committees. Assume, then, that the set inequality holds. In Figure 2 we depict x^0 , $F(x^0)$, House and Senate committee ideal points (HC and SC , respectively), and the committee contract locus ($P_H[x] \cap P_S[x] = \emptyset$).

The latter curve is heavily shaded where it has common intersection with $F(x^0) \cap P_H(x^0) \cap P_S(x^0)$. Also, we have indicated the bills, B_H and B_S , that have passed the respective chambers in different forms, necessitating the conference.¹²

The respective conferees take $F(x^0)$ as a constraint and seek a negotiated settlement, say B^* , consistent with that constraint. This normally requires a compromise in which the preferences of each chamber (as reflected in B_H and B_S respectively) are to some degree sacrificed. Indeed, in Figure 2, the committees sacrifice as well, agreeing on an outcome less preferred to them than their respective chamber's bills. Different configurations of preferences, however, need not have this property.

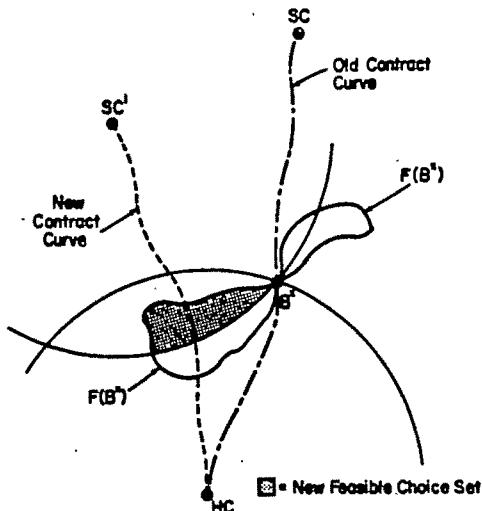
In the empirical literature on conference committees, much is made of who "wins" in conference (Fenno 1966; Ferejohn 1975; Strom and Rundquist 1977; Vogler 1970). Sometimes the outcome is closer to the House position, sometimes closer to the Senate position, and sometimes it entails splitting the differences between the chamber positions. From Figure 2 it is clear that such outcomes cannot be attributed entirely to relative bargaining skills or to which chamber acted first (explanations common in the literature). The non-convexity of $F(x^0)$ means that some compromises are infeasible (they may lie outside $F(x^0)$). Moreover, the ultimate compromise, B^* , may well lie closer to one bill than to the other, or closer to one committee's ideal than to the other's. But once again this cannot be attributed entirely to relative bargaining advantages since the relative locations of $F(x^0)$, $P_H(x^0)$, and $P_S(x^0)$ will restrict the feasible set of agreements. In Figure 2, B^* is about equidistant from the Senate committee's and the House committee's ideal. But it constitutes the best the House committee could hope for, given the constraints, whereas the Senate might have done better. In general, the configuration of chamber

preferences and of conferee preferences will determine the feasible bargaining range.

While the task of modeling conference proceedings falls under the rubric of future research, there are two more insights we can offer, one on equilibrium and the other on comparative statics. In Figure 3, B^* is the negotiated conference agreement (from Figure 2) between HC and SC. It lies on their contract locus and is an element of $F(x^0)$ (not pictured). There is no reason, however, to believe that chamber majorities are content with B^* . Since $B^* \in F(x^0)$ and since it comes back to each chamber under a closed rule, it will pass. But it is entirely possible that $F(B^*) \neq \emptyset$, as shown in Figure 3. Nonetheless, in these circumstances B^* is now an equilibrium. Since $P_H(B^*) \cap P_S(B^*) = \emptyset$ (i.e., B^* is on the HC-SC contract locus), the set inequality is violated so that despite clamoring from both chambers for change, none will be forthcoming. For every proposed change in B^* , at least one of the conference delegations will exercise its ex post veto. Any such proposal will die in conference.

Our model also yields important comparative statics results. An equilibrium point, that is, a status quo point for which the set inequality is violated, will be upset by exogenous changes in committee composition (but not by changes in *chamber* composition not affecting committee composition as well). In Figure 3, if the Senate committee's ideal shifts from SC to SC' , an entirely new contract locus is traced out, and B^* is no longer in equilibrium. This suggests two nonobvious comparative statics implications. First, the ex ante and ex post vetoes of committees may neutralize even dramatic changes in chamber composition, slowing if not blunting altogether the tracking of policy with popular preferences. Second, committee composition changes, even if restricted to only one chamber, have a disequilibrating effect. Thus, as Weingast

Figure 3. Conference Committee Comparative Statics



and Moran (1983) discovered about the Federal Trade Commission, dramatic changes in the composition of the Senate oversight committee (with no concomitant changes on the House side) in the 1970s were sufficient to set into motion a major change in policy direction at the Federal Trade Commission.

Committee Dominance of the Conference

In order for committees of jurisdiction to possess an ex post veto, they must dominate conference committee delegations. On the basis of the reports of early students of the subject (unfortunately, without much in the way of supporting evidence), such dominance has been the case for more than a century (Rogers 1922; McCown 1927). We do not present a full-blown empirical analysis here, but in order to give some veracity to our claims we have examined all conferences listed in the Congressional Information Service's Annual Abstracts of Congres-

Table 1. Conference Committee Composition

Type of Conference	Number of Conferees Not on Germane Committee							
	1981		1982		1983			
	House	Senate	House	Senate	House	Senate		
Budget ^a	0 (15)	0 (12)	0 (15)	0 (10)	0 (15)	0 (8)		
		[2]		[3]		[1]		
Appropriations ^b	0 (168)	0 (187)	0 (155)	0 (187)	0 (225)	1 (219)		
		[12]		[11]		[8]		
All others	2 (190)	3 (155)	3 (399)	3 (276)	2 (206)	0 (126)		
		[17]		[38]		[16]		

Source: Congressional Index Service, 1981-83.

Note: Cell entries give total number of conferees *not* from the committee of jurisdiction for each conference type. In parentheses are the total number of conferees. In brackets are the number of conferences.

^aNeither the Omnibus Budget Reconciliation Act of 1981 nor the Omnibus Budget Reconciliation Act of 1982 is included here. Each was an exceptional situation involving an unusually large number of conferees.

^bDoes not include omnibus supplemental appropriations or continuing resolutions.

sional Records and Legislative Histories for 1981, 1982, and 1983 (Congressional Information Service, Bethesda, MD).

Before reporting our evidence we note that a consequence of the 1970s reforms in the House and of the loose germaneness restrictions in the Senate is that many pieces of legislation are the handiwork of several committees in each chamber. A House bill, for example, might be amended in a nongermane manner by the Senate. Conferees are drawn from the committees of original jurisdiction plus additional conferees to deal (only) with the nongermane Senate amendment.¹³ Alternatively, it is occasionally the case in the House that the Speaker partitions a bill into parts and commits these to different committees for hearings and markup according to their respective jurisdictions; in the Senate, multiple referral may occur by unanimous consent. Again, conferees from all relevant committees make up the delegation.¹⁴

In Table 1 we present evidence on conference committee composition for the conferences held from 1981 through 1983.

For each year, by chamber and type of legislation, we report the number of conferees who were *not* members of the committee(s) of jurisdiction. The data are crystal clear in their message. On only one occasion in the three years, was a member—not sitting on the Appropriations Committee of either chamber—a conferee for an appropriations bill. On only a handful of occasions (fewer than 1% in the House; about 1% in the Senate) were noncommittee members conferees for legislative committee bills. And finally, on budget resolutions only members of the two budget committees were conferees.

A further perusal of the data on which Table 1 is based yields additional impressions, though we will not attach any quantitative weight to them here. First, it is almost always the case that the chairperson and the ranking minority member of the full committee from which the bill originated serve on the conference. Second, it is extremely rare for a conference to produce an agreement to which these two persons are not signatories; it

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Table 2. Subcommittee Autonomy in Conference: Appropriations Committees

House Autonomy	Senate Autonomy		
	Complete	Dominant	Partial
Complete	18	3	2
Dominant	2	1	0
Partial	0	0	0

Key:

Complete: The conference delegation was identical to the subcommittee membership.

Dominant: Either one subcommittee member was deleted, or one nonsubcommittee member was added to the conference delegation.

Partial: Subcommittee representation was neither complete nor dominant.

Note: Table includes Appropriations conferences in 1981, 1982, and 1983 exclusive of omnibus supplemental appropriations or continuing resolutions.

happens on occasion (for example, Chairman Hatfield did not sign several Appropriations conference reports), but we hesitate to draw any conclusions from these events for they are likely to involve contextual details that are not available without in-depth study of the particular cases. Third, there is considerable evidence that, in addition to full committee chair and ranking minority member, the subcommittees responsible for the bill dominate the conference delegation (see below for some additional details).

Committee dominance at the conference stage is perhaps the most complete and certainly the most obvious in our data in the area of appropriations. Moreover, the decentralization to the subcommittee level within each appropriations committee that Fenno (1966) described twenty years ago is clearly evident at the conference stage as well. In Table 2 we display the evidence for this claim for all appropriations measures (omnibus bills excepted) in 1981, 1982, and 1983. Subcommittee autonomy is said to be complete in conference if all the members (and only all the members) serve as managers. Subcommittees are dominant when either one subcommittee member was excluded from the conference or a nonsubcommittee member was included. Since the former circumstance may often

arise with no political weight attached (e.g., a Senator is out of town; a Representative is ill) and the latter occurred on only a single occasion, most of the dominant autonomy occurrences are hardly different from their complete autonomy counterparts. Finally, partial autonomy arises when more than one subcommittee member is deleted from conference. As the evidence suggests, subcommittees of both appropriations committees not only take full responsibility within their respective chambers for marking up appropriations measures and managing them on the floor but the same (relatively small) group of legislators meets year after year to hammer out a final compromise.

As a final bit of empirical corroboration, we have taken a sample of conferences by legislative committees from the 1981-83 period to see the extent to which the subcommittee autonomy evidenced in the appropriations realm carries over to other types of legislation. The results appear in Table 3. Of the 71 legislative committee conferences from the 1981-83 period, we examined the composition of 27 to see the extent to which the subcommittee of jurisdiction dominated the conference delegation. The evidence of subcommittee influence here, while not as overwhelming as in the appropriations realm, is nevertheless con-

Table 3. Subcommittee Autonomy in Conference: Legislative Committees

House and Subcommittee Membership	On Conference	Off Conference
House Members		
On subcommittee	210	248
Off subcommittee	25	720
Senators		
On subcommittee	136	72
Off subcommittee	35	204

Note: The populations are the committees of jurisdiction. The first row of each panel gives the number of subcommittee members on and off the conference delegation. The second row gives the number of non-subcommittee members (but on the full committee) on and off the conference delegation.

siderable. In both chambers subcommittee members dominate the conference delegations. In the House they constitute about 90% of the conferees; in the Senate, nearly 80% of the conferees. More importantly, the median case is one in which the conference delegation is drawn entirely from the subcommittee of jurisdiction.

Discussion

We have sought to offer a more discriminating notion of committee veto power, to embed it in a decision making sequence, and thereby to provide a firmer explanatory foundation for committee power than has been provided heretofore. Our theoretical examples and the accompanying figures illustrate the methodological tools and suggest the lines of what is a fairly general argument. Of central importance is the role of sequence. It matters, for example, whether veto power comes first (as in gatekeeping) or at the penultimate stage (as in conference proceedings). An undiscriminating treatment of committee agenda power that fails to distinguish between different sequential properties of that power is often misleading.

In emphasizing sequence and explicit enforcement arrangements, we do not intend to deprecate the ideas of self-enforcing agreements, implicit coopera-

tion, and deference that have constituted traditional stock-in-trade explanations for committee power. Surely, all of these operate. Moreover, our focus on ex post enforcement is in no way inconsistent with the fact that many participants might themselves explain their behavior as essentially deferential. It would not surprise us to find most legislators saying, "Sure, I let those people over on Education and Labor do pretty much what they think is reasonable. And they do the same for us on Armed Services. That's the way things are done around here." We would only claim that "deference" labels a behavioral regularity; it does not explain it. The theoretical question of interest is why that behavior is an equilibrium. We have, in effect, sought to give deference a rational basis by embedding it in the strategic realities produced by the sequence of decision making.¹⁵

Much work, both theoretical and empirical, remains to be done. In the body of this paper, we have only hinted at the broader generality of our argument. A first-order priority is to specify theoretical conditions more explicitly and generally. Second, we need to understand committee strategies better. What is the optimal markup vehicle that a committee takes to the floor (see Shepsle and Weingast 1981)? What amendments will committee members themselves seek to offer on the floor?

To what extent do committees (party leaders, backbenchers) anticipate the conference stage, and how do these expectations and forecasts affect their prior floor behavior? Third, we have given little attention to the strategic opportunities available to noncommittee members. Given the partial control by committees, what strategies may noncommittee members pursue to influence committee legislation? Finally, how might we properly model the conference itself, the objectives of the participants, and the constraints imposed upon them? These are all theoretical questions upon which our methodology may be brought to bear.

Empirically, there is a good deal of qualitative description and quantitative work in the legislative literature on some aspects of the problems we have presented in this paper. Most of it, however, is not tied to a theoretical framework; and, as we pointed out earlier, it is not at all obvious to us that the main preoccupation of this literature—namely, the question, who wins in conference?—is at all illuminating. Conferees are constrained by what will pass their respective chambers and this in turn determines the feasible set of agreements conferees may reach. The evidence presented in the previous section on committee (subcommittee) autonomy suggests an even more persuasive reason for doubting the relevance of this question. The conference may be less an arena for bicameral conflict than one in which kindred spirits from the two chambers get together to hammer out a mutually acceptable deal. Surely on some (many?) subjects—for example, commodity price supports—the members of the House and Senate (sub)committees who control the conference have more in common with one another than either may have with fellow chamber members.

In our analytical approach to legislative institutions, we have focused on the locus and sequence of agenda power. In characterizing legislative decision making in

terms of who may make proposals (motions, amendments), and in what order, and who may exercise veto power and in what order, we wish to emphasize that these features are not merely the minutiae of parliamentarians. Rather, they provide the building blocks from which legislative institutions are constructed. The results presented here and by others elsewhere show that different mixes of these institutional building blocks lead to different outcomes and, correspondingly, to significantly different political behavior.

In the context of the committee system in the U.S. Congress, we showed that proposal power and *ex ante* veto power are insufficient to the task of institutionalizing an effective division-of-labor arrangement. In the absence of some form of *ex post* veto power, committee proposals are vulnerable to alteration and, because of this, committees have agenda control in only a very truncated form. It is unlikely, in our view, that such a shaky foundation would induce individuals to invest institutional careers in the committees on which they serve.

Although our analysis focused on the U.S. Congress and the manner in which the *ex post* veto is institutionalized there, it should be clear that our approach is more general. Because it can, in principle, be used to study any sequence of agenda control, it can be applied to institutions that differ significantly from Congress. Thus, we would conjecture more generally that bicameral legislatures in which committees are not the central actors in resolving differences between the chambers will not possess strong committees, *ceteris paribus*.

It is in this regard that the British Parliament is of some interest. The method of resolving differences between two chambers of a bicameral legislature is of British invention. The earliest recorded evidence of its practice comes from fourteenth century England. But in England, as Rogers (1922, 301-2) observes,

It had fallen into desuetude even before the Parliament Act of 1911 so attenuated the powers of the House of Lords. Controversies between the two chambers are not serious, or, except in rare instances prolonged. . . . Since the Government stands sponsor for practically all legislation, a conference between the Ministers and leading Peers in Opposition is able to compose the differences and, indeed, ministerial responsibility is ordinarily sufficient to prevent conflicts between the chambers or the necessity for a conference.

The institutions of cabinet government reduce the need for representatives of the two chambers to meet in conference to resolve differences. The centralized leadership of the cabinet confers agenda power in *both* chambers on the same single group of ministers. They possess proposal power and they control (either explicitly or through bargaining) the amendment process. There is no need for *ex post* reconciliation since the cabinet may choose policies that will survive both chambers *ex ante*.

A detailed application of our approach to this institution is beyond the scope of this paper. However, the outline above suggests three implications. First, centralized agenda power in the Parliament implies that policy across different areas is likely to be more coordinated than in the committee-based Congress. Because the committee system in the Congress delegates agenda power area by area to different individuals with not necessarily compatible goals, coordination across policy areas is more difficult. Second, the Speaker in the House of Representatives is structurally disadvantaged in comparison to the Prime Minister in Parliament. Because the Speaker holds few of the critical elements of agenda power, he must depend extensively on persuasion to induce others to pursue his own objectives. On the other hand, the Prime Minister holds important powers over the ministers because they owe their positions to the Prime Minister rather than to an independent property rights system conveyed by seniority.¹⁶ Third, we conjecture that, because of the cabinet institution, a

system of standing committees in the British Parliament would most likely lack the sort of *ex post* veto with which congressional committees are blessed. By the argument of this paper it would be surprising if a full-blown committee system of the U.S. type were ever to develop. This is but another way of saying that institutions of *ex post* enforcement confer power on committees. In their absence, we doubt committees would play the consequential role they do in the U.S. Congress.

Notes

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1. The assumption of jurisdictional separability does not preclude inseparabilities within jurisdictions. In most of our examples, we draw indifference curves as circles, but our arguments extend more generally to any convex level sets.

2. In later discussion, we relax this requirement in order to determine what might happen if other legislators have the capacity to pry open the gates.

3. For committee C , by $P_C(x)$ we shall mean the set of points preferred to x by a *decisive committee majority*. We do not here dwell on the characteristics of such decisive sets.

4. Formal rules governing amendments and requirements like voting the status quo last do place restrictions on final outcomes. Nevertheless, this set typically contains a range of alternatives (some not in $P_C(x)$) over which the committee is unable to exercise subsequent control. See Shepsle and Weingast 1984.

5. We assume here and throughout the paper that all agents vote sophisticatedly (Farquharson 1969) and are able to anticipate the strategic moves

of others. Thus, amendments to B like A_2 and A_3 will fail. The former actually beats B ($A_2 \in W[B]$). But since it would subsequently lose to x^0 ($A_2 \notin W[x^0]$), voters 2 and 3 vote against it because they prefer B to x^0 (even though 2 prefers A_2 to B). A_3 fails even though a majority prefers it to x^0 since $A_3 \notin W(B)$.

6. Notice that our explanation does not assert that discharge petitions will never be used or never work, only that they are unlikely to be used or used successfully for bills like A_1 or A_3 . If, for whatever reasons, the gates were kept closed on a bill like A_4 , a discharge petition would work because the threat of an ex post veto is incredible. Moreover, the discharge petition may be used strategically in this case to force a committee to report a bill in a form or at a time that is less than ideal from the committee's perspective.

7. What follows is based on the masterful essay by Bach (1984) on conference procedures.

8. The interested reader may consult the rules of the House and Senate, the *Senate Manual*, *Deschler's Procedure in the House of Representatives*, and *Jefferson's Manual*. At the turn of the century, the House and Senate codified procedures. The codification, conducted by Thomas P. Cleaves, clerk of the Senate Appropriations Committee, bears his name and is known as *Cleaves' Manual of Conferences and Conference Reports*.

9. Dr. Stanley Bach of the Congressional Reference Service informs us that while the presiding officer in each chamber appoints the conferees, their selection differs in each chamber. In the Senate, appointment is by unanimous consent so that the presiding officer exercises no discretion in selection; in practice, he proposes the list submitted by the chairperson of the committee managing the bill. In practice, the Speaker of the House does the same. But the standing rules in this chamber give the Speaker discretion (and instruction) so that the final outcome is the result of bargaining between the Speaker and the relevant committee chairperson.

10. Thus, even if a proponent of an amendment opposed by the committee is a member of the conference delegation, his or her views are decidedly in the minority.

11. Technically, the first chamber to vote on a conference report has a third option—to recommit it to conference (sometimes with nonbinding instructions). Given the press of business, this often has the same effect as voting the report down. If the first chamber does pass the report, then the second chamber is left to take it or leave it (since the conference committee is disbanded after the first chamber's action). Finally, as a technical matter, if either an ex post veto is exercised or one of the chambers defeats a conference report, the bill is not necessarily dead since the chambers may return to messaging or may appoint new conferees. Again, however, the practical effect is to harm, often irreparably, the search for a House-Senate reconciliation.

12. In the figure, both bills are elements of $R(x^0)$, that is, either is preferred to x^0 by both chambers. Of course, this need not be the case inasmuch as the common circumstance is one in which one bill, say B_H , could not pass in the other chamber. The particular depiction in Figure 2 does not affect the points we make in the text.

13. An instance of this (and there are many) occurred in the Cash Discount Act of 1981, a bill managed by the House and Senate Banking Committees. The principal managers for each chamber were drawn from these committees. But one part of the bill (section 303) fell into the jurisdiction of Energy and Commerce on the House side and Labor and Human Resources on the Senate side. Additional conferees from these two panels were appointed to resolve differences in this section of the bill.

14. Thus, the Department of Defense Authorization Act of 1982 was marked up principally by the House Armed Services Committee, but sections of it were considered by the Select Committee on Intelligence and the Committee on Judiciary. Each of these panels was represented on the conference delegation with specific responsibility for those sections of the bill falling in their jurisdiction.

15. Throughout we have emphasized explicit enforcement institutions as the glue holding agreements together. In the story we tell about conference committees, however, there is a key feature for which we cite no explicit enforcement: Why does the Speaker, in his discretion, appoint committee members to the conference delegation? Without this move on the Speaker's part, committees would lose their post-floor deliberation role. Space precludes an extensive discussion here, but we think the Speaker's observance of this norm is a key to understanding what leaders must do to stay in office.

16. In this sense, there is an apt parallel between the agenda powers of a contemporary Prime Minister and a turn-of-the-century Speaker of the House.

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PARTICIPATION AND PURPOSE IN COMMITTEE DECISION MAKING

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Participation in committee decision making is an important form of legislative behavior but one we know little about. I develop a model of committee participation and test it using data drawn from staff interviews and records of the House Committee on Education and Labor. The analysis confirms that congressmen are purposive actors, but it also shows that different interests incite participation on different issues and that motivational effects vary in predictable ways across legislative contexts. If members are purposive, however, they also face a variable set of opportunities and constraints that structure their ability to act. Members and especially leaders of the reporting subcommittee, for instance, enjoy advantages in terms of information, staff, and lines of political communication. At the same time, freshman status entails behavioral constraints despite the reputed demise of apprenticeship in legislative life. Understanding such patterns of interest and ability, I conclude, should permit us to illuminate several larger questions regarding decision making and representation in a decentralized Congress.

While committees are crucial to the legislative process in Congress, participation in committee decision making is not universal. Over two decades ago, Charles Clapp (1963, 265-66) observed that less than half a committee's members regularly participated in its deliberations. More recent accounts suggest that truancy at committee meetings is severe—a general response to the growing congressional workload and the proliferation of congressional workgroups (Davidson 1981, 109-11; Dodd and Oppenheimer 1985, 46). On any given bill, the membership of a committee or subcommittee abdicates its considerable legislative authority to some subset of self-selected members.

Viewed in this light, decisions about their participation in committee affairs are among the most important that congressmen regularly make. From the mem-

ber's point of view, involvement in committee work will affect the ability to achieve any of several political goals (Fenno 1973; Mayhew 1974); from the vantage of the institution, the aggregate patterns of participation will have several important implications. The range of members involved will directly affect the capacities of Congress as a representative body: who participates (in what ways, to what extent) will determine which values, interests, or geographic constituencies are represented at this crucial stage of the legislative process. Such patterns, in turn, will shape the policy decisions that emerge (or fail to emerge) from the committee rooms. And these patterns will have a direct bearing on how widely legislative authority is dispersed within an already decentralized institution.

Despite such implications, congressional scholars have paid little attention

to the questions of who participates in committee decision making and why.¹ In this paper I take up these questions. My principal aim is to develop and test a model of committee participation.

The model begins with the premise that congressmen are purposive actors, but it relaxes the standard assumptions that goals are stable, ordered elements of the elite psyche. If, as Fenno (1973) says, not all representatives are alike in the goals they harbor, not all issues are alike in their relevance to particular goals. The behavioral effect of any particular goal, I argue, is contingent on the object of legislative action, subjectively perceived by the actor. Even for the same members and the same issues, not all decision-making forums will be equally suited to their purposes. I here attempt to identify the conditions under which specific goals will come into play and develop a measurement strategy consistent with this more complex motivational scheme.

If members are led to participate because of the interests that certain bills evoke, they also face a variable set of opportunity costs and resource constraints that structure their ability to act. Some start with greater advantages than others—in terms of political experience, substantive expertise, and lines of political communication; some have greater ability to pay the costs that participation entails. The theory of participation presented here, tailored to the context of committee decision making, specifies a set of factors that structure those costs as they vary member to member and bill to bill.

Committee Participation: Concept and Measurement

Unlike other, more studied forms of legislative behavior (the roll call vote, the committee assignment request), participation in committee decision making refers

to no single, identifiable act. Like participation at the mass level (Verba and Nie 1972, chap. 3), it is a general category that subsumes a range of different political activities. Our first task is to consider how the concept is to be drawn. What are the possible modes of involvement at this stage of the legislative process?

I identify two here: *formal* and *informal* participation. Much of a committee's decision making activity takes place in formal committee and subcommittee markup sessions. Here the substantive issues are taken up for collective consideration; specific positions are debated; the legislation is summarized, amended, and voted on. Rules of parliamentary procedure are in force; hence all members enjoy the formal right to participate. Since the reforms of the early 1970s, moreover, the vast majority of markups have been held in open session so that formal participation has also been public participation. Such markups, in fact, tend to be well attended by committee outsiders: if not by the national press, certainly by representatives of the interests likely to be affected by the matter at hand.

At the same time, much of a committee's decision-making activity occurs informally—outside the context of an official markup. For instance, one of the most important stages occurs when members and their staffs decide the provisions of the original legislative vehicle. But members participate in other behind-the-scenes ways as well—in working out subsequent compromises, planning party strategy, building support for markup initiatives, and so forth.

Two reasons suggest that the distinction between formal and informal participation is an important one. First, the official markups are sometimes fast, pro forma affairs, revealing the roles played by only the two or three major players. Exclusive reliance on markup data thus increases the likelihood of measurement

error. But the second and more important reason is theoretical, not methodological. Simply put, public and nonpublic participation may differ both in their causes and their effects. This possibility is suggested by the oft-cited distinction between "workhorses" and "show horses" in congressional policy making (Clapp 1963; Matthews 1959; Payne 1980). Workhorses are members who shun publicity and tend to their legislative duties; their goal is to gain the respect of their colleagues and thereby enhance their influence over particular legislation (Matthews 1959, 1067). Show horses, on the other hand, seek publicity and neglect serious legislative work. They use their formal access to the committee microphone to address some current or future constituency, and their activities are less likely to affect outcomes. In other words, while formal and informal participation should be related,² the former is more likely to tap a workhorse dimension of legislative behavior, the latter more likely to tap a show horse dimension. To fully comprehend the nature of committee participation, then, information regarding both modes is required.

Formal Participation

For most committees in both chambers, the available data on decision making in committee markups are surprisingly detailed and complete. The data for this exploration are drawn from the records of 17 bills marked up in the House Education and Labor Committee during the 97th Congress. They tap individual involvement in five separate activities at both the subcommittee and full committee stage: (1) attending markup, (2) voting on recorded roll calls, (3) offering amendments, (4) speaking during markup debate, and (5) setting the markup agenda (see Appendix).

Taken together, these data provide an almost exhaustive account of each member's formal participation, bill by bill. For

the purposes of this exploration, however, a more economical measure of formal participation was required. The members were coded for each bill according to whether they engaged in each of the five activities, two of which were divided into more detailed subdivisions in order to retain more information.³ The resulting seven dichotomous variables were used in a Guttman scale analysis and produced the following ranking:

- 0 = engaged in none of the activities
- 1 = attended
- 2 = voted
- 3 = spoke: minor participant in discussion
- 4 = spoke: major participant in discussion
- 5 = offered minor, technical, grammatical amendment
- 6 = offered substantive amendment
- 7 = played significant agenda-setting role

At the full committee level, the coefficient of reproducibility for this analysis was .97 and the coefficient of scalability .91, indicating a clearly hierarchical scale.⁴ The analysis of subcommittee participation was likewise clear, producing a coefficient of reproducibility of .96 and scalability of .85. The Guttman scale scores, then, provide the summary measure of formal participation that I will use here (see Appendix).

Informal Participation

Like participation in the official committee markups, informal participation covers a wide range of specific activities. Unlike participation in markups, however, informal participation produces no written record of the member's involvement. Neither does it include only those activities in which a congressman participates in person. Prior to and outside the markups, much of the deliberation takes place at a staff level, with repre-

sentatives participating through their agents. In order to assess the degree to which members participated in this informal, perhaps indirect fashion, tape-recorded interviews were conducted with the committee and subcommittee staff members who had primary responsibility for each of the bills in the sample. In the course of a semistructured interview about the particular bill's development, I sought to (1) identify the committee members who helped shape the original legislative vehicle and explore the role each played, (2) discuss the involvement of members in subsequent premarkup deliberations, and (3) identify the legislative assistants with whom the committee/subcommittee staff interacted during committee consideration of the bill.

Staffers' responses were then coded to reflect each member's behind-the-scenes involvement, using a five-point index ranging from no activity at one end to principal authorship at the other (see Appendix). For 15 of the 17 bills, at least two staffers were interviewed, one or more from the majority and one or more from the minority. For eight of the bills, both a majority committee staffer and a majority subcommittee staffer were interviewed. (The minority on Education and Labor does not have distinct subcommittee staffs.) Their responses, then, provide a general indicator of informal participation, one that proved highly reliable in two respects. First, the intercoder reliability was unusually high ($r = .94$).⁶ Second, interstaffer reliability (for those bills where more than one staffer was interviewed) was high ($r = .86$), even when the comparison was confined to agreement between the majority and minority staffer ($r = .87$). In those cases where staffers disagreed, the responses were averaged.

Participation and Purpose

Consistent with other accounts of committee work in Congress (e.g., Carroll

1958; Clapp 1963; Fenno 1973, chap. 4), participation on House Education and Labor is highly selective. The measures of participation developed here bring this tendency into sharp relief. One senior Democrat, for instance, had a median markup score of zero: he attended neither a subcommittee nor a full committee markup for over half the bills in the sample. Yet on three other bills, his involvement was clearly major, offering and passing a total of sixteen substantive amendments and dominating the behind-the-scenes action. One freshman Republican, on the other hand, had a median markup of two (with a near perfect record of attendance and voting), yet he sat silently in the markup of almost every bill and did not offer a single amendment. In short, both the bill-to-bill and member-to-member variation is considerable.

How do we account for such patterns? Why do individual members participate (more or less) in the various questions that a committee considers? The explanation offered here focuses on two sets of factors: the representative's personal goals or interests and the structure of opportunities and constraints the member confronts in the decision making context.

Congressmen's Goals

Evidence both systematic and anecdotal tells us that contemporary House members have an almost limitless range of demands on their scarce time, energy, and legislative resources (e.g., Clapp 1963; Davidson 1981; O'Donnell 1978). Committee work is especially costly. Each bill on which a member participates directly diminishes time for other profitable activities: constituency relations, floor action, or involvement in some other committee bill, to name but a few. To participate the member must expect gains from the labor. I hypothesize here that congressmen selectively participate in order to advance certain political goals.

That congressmen are purposive actors, of course, is by now a common notion. Over the last decade-and-a-half, member goals have become central to theoretical work on legislative behavior (Ferejohn and Fiorina 1975; Sinclair 1983). Nonetheless, assumptions attending the concept's application in empirical research often remain implicit and unexamined (Eulau 1984, 611-15). Two assumptions will concern me here: first, that the motivational structure of individual congressmen is unidimensional; second, that the goals of congressmen are relatively stable across different legislative contexts.

The unidimensionality assumption comes in two variations. One genre of purposive models conceives of goals as general dispositions that are similar across individuals. At this level of abstraction, elites are motivated by self-interest or personal profit, which in legislative studies takes the more particular form of the desire for reelection. In Mayhew's now famous legislative psychology, congressmen are "single-minded seekers of reelection" (1974), and a literature now too lengthy to cite rests on this simple assumption.

Other studies, of course, render the mind of the congressman more complex. In the classic statement of this view, Feno (1973) finds that congressmen are motivated by the goals of reelection, influence within Congress, and good public policy and suggests that "all congressmen probably hold all three goals" in different mixes (p. 1). But for the purposes of analysis, Feno classifies members of a given committee according to the apparently "dominant" or "primary" goal in a relatively stable goal hierarchy (pp. 5, 7). This strategy, in turn, has led some scholars since Feno to classify members and/or committees according to their central motivational tendencies (e.g., Hinckley 1975; Smith and Deering 1984). Most other studies that maintain some notion of mixed motives have tended to

view them as being ordered, with reelection normally assumed to be the first concern of the representative, other goals becoming important when reelection is satisfied (e.g., Arnold 1979; Fiorina 1974). The effect of member goals on behavior is thus assumed to be relatively stable across different contexts, though their relative impact may change over the life-cycle of a representative's career (Fenno 1978; Fiorina 1974; Weisberg, Boyd, Goodman, and Gross 1982).

I here adopt a different view, one that derives from Kingdon's study of congressmen's voting decisions (1981, chap. 10). While the point is not explicitly drawn, Kingdon seems to suggest that the conventional assumptions cannot comprehend important variations in individual behavior. His "consensus mode of decision" not only incorporates Fenno's trilogy of goals, it comprehends the possibility that different goals may motivate a single congressman at different moments (i.e., on different votes) and that different goals may motivate different congressmen at a single moment (i.e., on a particular roll call vote). Under certain conditions, he finds, goals are "evoked" (see also March and Simon 1958, chap. 3).⁶

In the model of participation used here, then, the assumptions of unidimensionality and stability are relaxed. I assume that a representative may harbor any number of goals, more or less intensely. Their prominence in the member's motivational structure will vary depending on the issue being considered and the context in which he or she acts. The behavioral effect of any particular goal, in short, is contingent on the object of legislative action, subjectively perceived by the actor. The relevance of a bill to any one of a congressman's goals is likely to inspire greater activity, and the confluence of two or more goals should incite even greater participation. Whether individual congressmen are single-minded seekers of par-

ticular goals or whether a single goal can characterize a committee of individuals thus become empirical questions, not assumptions, in the purposive model specified here.

Measuring Motivational Intensities

If the assumptions employed here are correct, then, any attempt to measure goals-as-general-dispositions is theoretically second-best. Such dispositions exist as behavioral tendencies; they emerge in the course of individual action over time and across context. In committee studies, this point has been obscured by two common methodological problems. First, committee scholars have inquired about the goals affecting assignment seeking and simply assumed that such questions tap the motivations governing postassignment behavior. Second, they have had little data regarding motivational variations or intensities. As a result, the ability to estimate the effects of member goals on committee decision making has been severely limited.

The strategy required here, then, was to tap the relevance of each of several goals to each member's behavior on each of several committee bills. The practical problems in pursuing such a strategy, of course, are substantial. Access to representatives is often difficult to secure, and, in any case, many may be reluctant to talk analytically and frankly about their motivations. As a more practical if less direct approach, some scholars have interviewed congressmen's personal staff to obtain information about member goals. Bullock's oft-cited study of freshman motivations (1976), for instance, employs this approach, and his experience is encouraging. He concluded that "the material collected from staff is as accurate as that collected from members. This assumption was verified in those instances in which interviews were conducted with both the congressman and one or more of

his staff" (p. 203). Smith and Deering (1984), likewise, relied partially on staff interviews in their recent multicommittee study.

For a number of reasons, the general strategy developed by Bullock was especially compelling here. In the first place, the need to obtain information regarding a series of issues required more time than a congressman was likely to give, even were access obtained. More important, the staff is itself intimately involved in the committee participation of the congressman, whom they keep briefed on committee activities. They look out for the member's interests and often act on his or her behalf. Information they provide is thus an important basis for the representatives' perceptions of bills, and their reactions are a basis for their perceptions. To use the language of Salisbury and Shepsle (1981a, 1981b), it is the subjective perceptions of the "congressman as enterprise" that the measures used here will tap. Several steps were taken to ensure the reliability of staffer responses, however.

First, I took special care to identify the staffer or staffers who would be best qualified to provide the required information. Second, I designed the interview schedule (discussed below) to exploit information that staffers were likely to have and willing to communicate; questions requiring staffers to read the motivational mind of their congressman were avoided. Whenever possible, finally, I interviewed two or more qualified staffers for each representative in order to systematically check the reliability of their responses, a strategy which proved possible for about 40% of the congressmen in the sample.

The interview schedule sought information regarding the relevance of each of the 17 bills to each of four possible goals:

Serving the District. As part of the interview, respondents were administered a two-page form asking them to rate the

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importance (major/moderate/minor/negligible) of each bill to their congressman's district. This proved to be a straightforward exercise. An important part of these staffers' jobs, in fact, is to keep their boss briefed on how committee legislation affects the constituency. The accuracy of staffer rankings, moreover, is supported by the agreement of responses when two or more staffers were interviewed for the same congressman. Over 96% of the rankings were within one of each other, and the interstaffer correlation was .80 ($N = 238$).

These ratings of district interest bill by bill, then, serve as the operational measure of each member's reelection goal (Fenno 1973; Mayhew 1974) or, more generally, of the desire to satisfy constituents' interests (Kingdon 1981, 246). The key assumption here is that this motivation will be evoked when issues arise that have an impact on the district. Specifically, we would expect members to participate more on those issues that they perceive to be highly salient to their constituents.

As I have conceptualized it here, however, the effect of district interest on a member's participation may cut two ways. If certain issues evoke intradistrict salience, they may also evoke intradistrict conflict (Fiorina 1974). Members may not only participate in order to promote their district's interests; they may avoid actions that could alienate specific groups. In the case of Education and Labor, such a scenario has historically held for the Republicans on labor issues (Fenno 1973). As one senior minority staffer noted: "We have a heck of a time getting new members to serve on the committee . . . and that's because of the labor side of things. We're always on the wrong side; the labor issues can only hurt you." In short, district concerns may lead Republican members to avoid visibility on labor issues as their districts' interests increase. To capture this effect, I use an interactive term

which takes the value of the product of the district variable, the congressman's party (1 if Republican, 0 if Democrat), and a dummy variable for labor issues (1 if labor, 0 otherwise).⁷ The expected effect, then, is negative.

Making Good Policy. Indicators for each congressman's goal of making good public policy (Fenno 1973) were measured in like fashion. Using a similar form, staffers were asked to rate how closely each bill related to their representative's general policy interests or commitments—interests that, from working with and observing a congressman week-in and week-out, staffers are in a good position to know. The important assumption implied in this operationalization, then, is that the desire to make good public policy is evoked when issues arise that are related to a congressman's particular set of policy interests or beliefs. Interstaffer reliability on these responses was .77.

Making a Personal Mark. The two goals discussed thus far are adapted from Fenno (1973). The third derives more directly from Lawrence Dodd's essay "Congress and the Quest for Power" (1977). Dodd argues that the most important goal of the contemporary congressman is to "make a mark" in a specific policy area (1977, 272-75).⁸ In a decentralized Congress, the realization of this goal requires substantial effort in shaping legislation at the committee and subcommittee stage. As the notion of the workhorse suggests, moreover, members who seek to influence legislation must be especially active behind the committee scenes. The effect of this variable, then, should prove greater on informal than on formal participation.

The relevance of each bill to this motivation was coded (0-3) from responses to the question

I've noticed that a congressman will sometimes select a specific issue or issue area where he wants to "make a mark"—where he might have

some particular policy making influence. Are there any Education and Labor issues for which this would be true of Congressman ____? Do any of these bills fall into that area?

For each bill cited, probes were used to determine the extent to which the member sought to claim it as personal territory. Interstaffer reliability measured .91.⁹

Promoting the President's Agenda. Finally, I hypothesize that under certain conditions individual congressmen will participate in order to promote the president's agenda on matters within their committee's jurisdiction. In the parlance of Capitol Hill, a member "serves as point man" or "carries water" for the administration. This objective, of course, is no doubt related to the other goals already discussed. *Ceteris paribus*, members of the president's party have an electoral interest in seeing that his legislative program is successful, especially if they anticipate coattail effects in a future election. Those with policy interests similar to the president, moreover, should be likely to participate on matters high on the administration's agenda. But the staff interviews suggested that certain members take on the role of administration agents out of loyalty to party and president, far in excess of the direct electoral benefits they are likely to derive and independent of their own interests in the issues the administration is pushing.

The relevance of this goal was measured by an interactive term. Interviews with the relevant committee and subcommittee staffers were first used to determine which bills in the sample were high priority on the administration's agenda (coded 0/1). Personal staffers of Republican members were subsequently asked

During 1981-82, the Reagan Administration was aggressive on a number of issues before Congress. Generally speaking, how committed would you say Congressman ____ was to carrying out the president's agenda in the area of Education and Labor?

Responses to this question were coded 0-3. The measure of each congressman's desire to promote the president's agenda is the product of these two variables. Interstaffer reliability measured .80.

Data on these five variables, then, provide reasonable if somewhat indirect indicators of individual members' motivations—motivations that may vary in intensity from congressman to congressman and bill to bill.

The Structure of Opportunities and Constraints

If member goals are important determinants of committee participation, they are not likely to provide a complete explanation. In allocating their scarce time and energy to committee affairs, representatives will not blindly pursue their interests when the benefits acquired are likely to be less than the costs incurred. I here explore factors that are likely to affect the opportunity costs and resource constraints the member confronts in deciding whether and to what extent to participate on a particular bill.

Subcommittee Membership

In the decision making process of the House committee, members of the subcommittee with jurisdiction over a bill enjoy a number of advantages, so much so that subcommittees are sometimes said to "govern" the legislative process (Davidson 1981; Dodd and Oppenheimer 1985). In the first place, the members are likely to have greater experience in the substantive issues their workgroups will address. Shepsle (1978) has shown that background in a policy area is itself one of the reasons for committee selection, and this reasoning undoubtedly holds for the subcommittee assignment process as well. In addition, many of the same issues recur in different bills within a subcommittee's jurisdiction so that the subcommittee

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member tends to have relevant legislative experience in both the politics and substance of a bill.

If subcommittee members start with superior expertise, they also enjoy greater resources to pay the marginal information costs (Hall and Evans 1985). *Ceteris paribus*, the subcommittee member will enjoy greater access to the professional staff assigned to a bill. The member's legislative assistants, likewise, are apt to have greater experience in the relevant issues. And both member and assistant are more likely to have well-developed lines of communication to interested actors both on and off the committee. The subcommittee member need not, as a result, give up as much in the way of beneficial activities elsewhere in order to pursue interests on a particular committee bill.

Subcommittee Position

Not all subcommittee members are equal, however. The opportunities for effective action are much greater for the chairman and ranking minority member of the reporting subcommittee. One of the key reforms of the early 1970s was to guarantee subcommittees their own staffs, and those staffs normally are hired and fired by the subcommittee chairman (Rohde 1974). Albeit to a lesser degree, the ranking minority member usually enjoys additional staff support as well, and both positions bring greater control over the subcommittee's agenda, the scheduling of meetings, and the progress of legislation. If such work is likely to conflict with other business the member finds important, the subcommittee leader is better able to adjust the nature and timing of events to suit his or her objectives. Thus chairmanship and ranking minority status should significantly enhance participation, both formal and informal.

If a subcommittee's chairmanship offers greater opportunities and resources, how-

ever, it also entails obligations and expectations. In particular, the chairman is obliged to manage reauthorizations of programs under the subcommittee's jurisdiction, major presidential initiatives, and other bills on which action is required. As several staffers noted, the chair is bound to be active even when not interested; this officer cannot participate as selectively as, say, the ranking minority member or full committee chair. Such role requirements suggest, then, that there is an interaction between chairmanship of the reporting subcommittee and the impact of the member's goals.¹⁰ Such interactive effects should be negative.

Full Committee Position

The growth in subcommittee importance, as most observers have noted (e.g., Davidson 1981; Deering and Smith 1985), has diminished the importance of the full committee chairman and, one might surmise, of the ranking minority member. This is not to say, however, that the policy-making advantages of these positions have wholly evaporated. Chairmen and ranking members still retain authority over substantial staffs and budgets, as well as considerable influence over the timing and handling of bills referred to their committee. We should therefore expect that, *ceteris paribus*, full committee chairmanship or ranking minority status should significantly diminish the resource constraints felt by the individual member and enhance participation in committee decision making. If the claims about subcommittee government are accurate, however, the magnitude of such effects should prove smaller than those for the respective subcommittee positions.

Freshman Status

Until perhaps two decades ago, legislative scholars emphasized that the freshman representative is pressured by house norms to remain relatively passive in

legislative affairs (e.g., Fenno 1966; Matthews 1960). Freshmen should be seen and not heard, more or less, and the failure to observe this admonition was likely to provoke sanctions by senior colleagues. More recent studies, however (e.g., Asher 1975; Ornstein 1981), have consistently argued that this norm has all but disappeared, suggesting that freshman status no longer depresses a member's role in committee decision making. The notion of participation costs sketched here suggests a different hypothesis. While group pressures may no longer restrain a new member's behavior, the freshman is more likely to be uninformed about the substance of a bill, naive about committee politics, and dependent on staff who are themselves inexperienced and inefficient. Overcoming such disadvantages requires a substantial investment of the member's scarce time and energy, an investment not likely to be sustained across a range of bills.

If the theoretical argument here is correct, then freshman status should depress participation in committee legislative work, other things being equal.¹¹ It is important to point out, however, that other things are not equal between freshmen and their more senior colleagues. In the first place, freshmen are less likely to have sought the committee, at least as a top choice. It is thus less likely that the various areas of the committee's jurisdiction will be well-suited to their goals. By incorporating specific indicators of member goals into the analysis here, however, I can control for this possibility. Second, freshmen may participate less because they are more likely to come from marginal districts; energy and attention are thus taken away from committee work in favor of other district-oriented activity—constituency relations, casework, more trips home, and the like (Fenno 1978, 215–24). I therefore control here for marginality, measured as a dummy variable that takes a value of one if the member

won with less than 55% of the vote in the last election. By controlling for these other factors, the effect of freshman status should provide a reasonably good measure of apprenticeship in committee decision making.

Minority Status

The impact on committee participation of a member's party affiliation is likely to be complex. On the one hand, the minority member faces special barriers to participation, having less access to and affinity with individuals with formal power (i.e., committee and subcommittee chairmen), and enjoying fewer staff and other resources. In the drafting of the markup vehicle and other informal activities, these constraints should be especially serious. We should therefore expect minority status to depress one's informal participation significantly. But given that the minority is more likely to have been excluded from the informal deliberations, they should be all the more active at formal markup. The markup becomes their opportunity to react to and try to amend the majority vehicle. We should thus expect minority status to enhance formal participation.

A Multivariate Analysis: Results and Interpretations

According to the argument elaborated here, then, congressmen are led to participate by the goals that certain bills evoke. At the same time, they face a variable set of opportunities or constraints that structure their ability to act. In Table 1, I summarize the specific variables that comprise these two categories and present the regression estimates of their effects on four measures of committee participation. Columns 2 and 3 present the estimates for subcommittee and full committee markup participation, respectively, as measured by the Guttman scale scores described in

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the first section; column 4 presents the estimates for a combined measure of formal participation, which takes the higher of the subcommittee and full committee values;¹² column 5 presents the estimates for the index of informal participation, where the original scale (0-4) is transformed to the same scale as formal participation (0-7) to facilitate parameter comparisons.

Given the large number of random factors likely to affect participation on a given bill (scheduling conflicts, trips

home, illness, etc.), the model performs fairly well, explaining 50% of the bill-by-bill variance in members' formal participation and 61% of the variance in their informal participation. It is especially encouraging, too, that (1) the estimates for formal and informal participation are remarkably similar even though these behavioral modes were measured using two different measurement strategies and qualitatively different data sources; (2) the differences that do appear, as I will discuss shortly, are consistent with the

**Table 1. Multivariate Analysis of Participation in Committee Decision Making:
House Education and Labor, 97th Congress**

Independent Variables	Unstandardized Regression Coefficients			
	Subcommittee Markup	Full Committee Markup	Formal Participation	Informal Participation
Goals				
Serving the district	.65 ^a	.25 ^a	.30 ^a	.15 ^c
Republican-labor interaction	-1.15 ^c	-.24	-.27	-.22
Making good policy	1.11 ^a	.50 ^a	.54 ^a	.36 ^a
Making a personal mark	.39	.61 ^a	.59 ^a	1.05 ^a
Promoting the president's agenda	.22	.60 ^a	.49 ^a	.22
Opportunities/constraints				
Membership, reporting subcommittee	—	.41 ^a	.80 ^a	.85 ^a
Chairmanship, reporting subcommittee	6.97 ^a	5.89 ^a	5.56 ^a	6.54 ^a
Ranking minority status, reporting subcommittee	2.64 ^a	1.87 ^a	2.11 ^a	1.57 ^a
Chairmanship, full committee	—	1.65 ^a	1.69 ^a	1.42 ^a
Ranking minority status, full committee	—	1.47 ^a	1.27 ^b	.18
Freshman status	-1.38 ^c	-.84 ^a	-.95 ^a	-.79 ^a
Marginal district	.61 ^c	.10	.27 ^c	.15
Minority party	-.11	.41 ^b	.42 ^b	.26
Chair-goal interactions				
Subcommittee chair, district service	-.55	-.34	-.33	-.58 ^c
Subcommittee chair, good policy	-1.22	-.55	-.58	-.51
Subcommittee chair, personal mark	-.35	-.41	-.41	-.45
Number of cases	96	493	493	493
R ²	.72	.48	.50	.61

^aSignificant at .01 level.

^bSignificant at .05 level.

^cSignificant at .10 level.

theoretical differences in the nature of these two modes; and (3) the results are reasonably similar when one moves from subcommittee to full committee even though the subcommittee stage entails a nonrandom subset of the bills subsequently marked up at full committee, includes a different population of actors, and (given that several variables at the full committee level drop out at the subcommittee level) requires a slightly different explanatory model. Of greater theoretical interest here, however, is that with few exceptions the hypothesized relationships discussed above are clearly borne out.

The Motivational Complexity of Congressmen

The theoretically most important finding of this analysis is the support given to the model's complex picture of congressmen's motivations. The most basic (if least remarkable) conclusion to be drawn is that congressmen are much more than single-minded seekers of reelection. The parameter estimates for all four goals are statistically significant in the model of formal participation, for instance, and the constituency motivation has the smallest impact of the four. In one respect, of course, this is not surprising. Education and Labor is normally thought of as a policy more than a reelection committee, and that general tendency finds support here. In participation both informal and formal, at subcommittee and at full committee, the policy coefficient is consistently correct in sign, statistically significant, and larger than the coefficient for the district service variable.

This is not to say, however, that constituency concerns are never on the minds of representatives as they decide whether and to what extent they will participate in Education and Labor decisions. The coefficients for district service are consistently positive and significant; the coefficients for the Republican-labor interaction,

moreover, are consistently negative, suggesting that under certain conditions individuals may avoid issues that cause them political problems at home. At one level, then, the analysis supports the findings of scholars (e.g., Perkins 1980; Smith and Deering 1984) who suggest that certain committees may be characterized as having "mixed goals." But the more important implication is that such studies underestimate committee members' motivational complexity. While good policy and reelection may now stand out as the principal reasons for seeking committee assignments (Smith and Deering 1984, 90), these are only two (and perhaps not even the most important two) goals that shape the behavior of congressmen in postassignment decision making. In both formal and informal participation, one of the strongest motivational influences is the member's desire to make a mark.¹³ Likewise, the member's interest in promoting the president's agenda is consistently correct in sign and proves significant in the model of formal participation.

The results of Table 1 likewise show that specific goals are evoked by the legislative context in which the member acts. As I hypothesized, the district motivation is a significant determinant of a member's public participation, but the coefficient is half as large for participation behind the scenes and borders on statistical insignificance. At the same time, the effect of the member's desire to make a mark is twice as large for behind-the-scenes as it is for public participation. To the extent members are motivated by reelection, they are apt to play the role of show horse, to exploit the public nature of the markup and, as Mayhew (1974) argues, to pursue electoral objectives without emphasizing substantive legislative work. But the individual interested in making a mark must be something of a workhorse. Significant influence over the substance of a bill seldom comes without involvement in such activities as writing, negotiating, and

building support for its major provisions. In sum, formal participation tends to be more constituency-oriented than informal participation; informal participation tends to be more influence-oriented than formal participation—precisely the findings the workhorse/show horse distinction led us to expect.

The presidential loyalty motivation likewise exhibits important but predictable differences across the two participation modes. While this goal appears to be an important determinant of public participation, no such claim can be made for participation behind the scenes. During the 97th Congress, the Reagan administration initiated a number of attacks on programs dear to the Democratic majority of Education and Labor, and the Democrats responded with ideological solidarity. On the administration's high agenda items, in particular, administration loyalists were afforded few opportunities to promote the president's agenda prior to the official markup.

The presidential loyalty motivation, then, is contingent on the behavioral context in which the member acts—a context here characterized by ideological division, with the administration pitted against the committee majority. If this argument is correct, however, we should not expect the effects to be constant over time. Because the sample here covers two years, one (1981) spanning Reagan's honeymoon period and another (1982) characterized by greater congressional independence (especially in the House), we can examine the extent to which loyalty to the administration wanes as the president moves into his second year. A reestimation of the model appears in Table 2 in which I separate the presidential loyalty variable into first- and second-year effects.

The results are interesting in several respects. First, the presidential loyalty motivation is in fact a strong, positive determinant of formal participation during Reagan's first year in office, only to

prove statistically insignificant during the second year. Equally interesting, however, is that the pattern proves just the reverse for informal participation. In the context of the highly partisan, executive-led politics of 1981, Republican members apparently found active, visible support for Reagan not only politically more attractive, they found the majority less willing to negotiate with them behind the scenes. By 1982, however, with Reagan declining in popularity and his budget cuts boding ill for Republicans in the midterm elections, the presidential loyalty motivation had generally diminished, and the remaining loyalists had gone behind the scenes. In sum, presidential loyalty influences informal participation, but its effect is indeed contingent on other political conditions.

In sum, the variations across participation modes and legislative sessions support the argument that member goals are less stable than has normally been assumed. But the contention here has been more specific: I have suggested that an individual member may be led to participate by different objectives, depending on the member's perception of the issue at hand. On this point, the evidence of the foregoing analysis is only indirect. The pattern of the parameter estimates shown could be produced by differently motivated persons, single-mindedly pursuing individual goals.¹⁴ The full data set was thus broken into congressman-by-congressman subsets, and the model was estimated for each one. While their N is small (17), such analyses provide some insight into the individual-level motivations. Two strategies were used here. The first was to test whether the signs of the goal coefficients were consistently positive. If the motivational psychology of congressmen is characterized by single-mindedness, we should expect that once the largest positive coefficient for each congressman is eliminated, the coefficients of the remaining goal variables

should randomly be distributed around zero. This did not prove to be the case, however. Only 23% of such coefficients were negative, with the mean of all coefficients positive (.21). The second was to examine the relative size of the goal coefficients for each representative. In 19 of the 29 cases (66%), the second largest positive coefficient was at least half the size of the

largest and greater in magnitude than its counterpart in the analysis of the full data set.

A final angle on the individual's motivational complexity derives from the regression analyses, conducted bill by bill. If members have stable goal structures that shape their behavior regardless of the decision context, we should expect

Table 2. Multivariate Analysis of Participation in Committee Decision Making Including "Period Effects": House Education and Labor, 97th Congress

Independent Variables	Unstandardized Regression Coefficients			
	Subcommittee Markup	Full Committee Markup	Formal Participation	Informal Participation
Goals				
Serving the district	.65 ^a	.25 ^a	.26 ^a	.12
Republican-labor interaction	-1.15 ^c	-.21	-.27	-.18
Making good policy	1.10 ^a	.49 ^a	.54 ^a	.39 ^a
Making a personal mark	.43	.60 ^a	.59 ^a	1.03 ^a
Promoting the president's agenda, 1st year	.19	.71 ^a	.68 ^a	-.13
Promoting the president's agenda, 2d year	—	.36	.32	.41 ^c
Opportunities/constraints				
Membership, reporting subcommittee	—	.44 ^a	.84 ^a	.83 ^a
Chairmanship, reporting subcommittee	6.99 ^a	5.82 ^a	5.52 ^a	6.47 ^a
Ranking minority status, reporting subcommittee	2.62 ^a	1.85 ^a	2.11 ^a	1.57 ^a
Chairmanship, full committee	—	1.65 ^a	1.69 ^a	1.35 ^a
Ranking minority status, full committee	—	1.50 ^a	1.31 ^a	-.08
Freshman status, 1st year	-1.46	-.79 ^b	-.78 ^b	-.85 ^a
Freshman status, 2d year	-.62	-.47 ^a	-.54 ^a	-.50 ^a
Sophomore status	.21	-.12	.07	-.43 ^a
Marginal district	.59 ^c	.11	.17 ^c	.18 ^c
Minority party	-.15	.43 ^b	.42 ^b	.34 ^c
Chair-goal interactions				
Subcommittee chair, district service	-.56	-.33	-.33	-.55
Subcommittee chair, good policy	-1.22	-.55	-.58	-.54
Subcommittee chair, personal mark	-.39	-.41	-.41	-.44
Number of cases	96	493	493	493
R ²	.72	.47	.50	.61

^aSignificant at .01 level.

^bSignificant at .05 level.

^cSignificant at .10 level.

relative stability in the coefficients for the goal variables across the 17 bills. The variation that appears, however, is considerable. Coefficients on the district motivation ranged from around 2.0 on the 1981 Budget Reconciliation recommendations to slightly negative on a bill that would provide a one-time \$50,000 death benefit to survivors of federal police officers. Coefficients on the policy goal ranged from near zero on a bill to re-authorize a minor educational program to 1.45 for the Job Training Partnership Act—the committee's most important initiative of the session. Two bills, in fact, evoked none of the goals, with the several leadership positions accounting for most of the variance explained. In sum, a range of evidence suggests that the approach to congressmen's motivations used here is warranted.

Institutional Position and Resource Constraints

If the multivariate analyses suggest that members are purposive, they provide clear evidence that certain opportunities and constraints structure their behavior as well. The most striking results, in fact, are those associated with membership in the subcommittee of jurisdiction. All three of the relevant coefficients are correct in sign and clearly significant, regardless of the measure or mode of participation analyzed. As hypothesized, subcommittee membership by itself enhances participation even if we focus only on the full committee markup. Especially dramatic are the effects of the subcommittee leadership positions. As one would expect, the impact of chairmanship is greater than that of ranking minority status, but both are exceedingly powerful determinants of behavior.¹⁵ The importance of subcommittee membership and position are even more apparent in legislative action behind the scenes, where professional staff and

well-developed lines of communication are crucial.

At the same time, we should not interpret the leadership coefficients solely as evidence of resources exploited. They also tap the fact that formal position can carry official obligations, leading the ranking member to serve as spokesman for his or her party's position. As I argue above, this is especially true for the subcommittee chair. This argument finds consistent support in the multivariate results. While none of the interactions between subcommittee chair and member goals is statistically significant, they are uniformly negative across the several models. Participation, such patterns illustrate, is less purposive for those with well-defined role requirements.¹⁶

The multivariate results likewise show that full committee leadership position enhances a congressman's participation in decision making activity. Superior legislative background, staff resources, and agenda control generate opportunities to intervene in a wide range of committee decisions. It is worth emphasizing, however, that such effects are apt to vary considerably from committee to committee, leadership style being a somewhat idiosyncratic thing. The findings here accord with the reported styles of both Chairman Carl Perkins and ranking Republican John Erlenborn. While staff members consistently emphasized the importance of Perkins's role in committee deliberations, he gave considerable latitude to his subcommittee chairmen and normally deferred to their judgment (Fenno 1973, 287-88; Smith and Deering 1984, 179). Of special interest, then, is not that full committee chairmanship is statistically significant but that it is not greater than it is ($b = 1.69/1.42$), especially when compared with the parameter estimates for both the subcommittee chair ($b = 5.56/6.54$) and subcommittee ranking minority member ($b = 2.11/1.57$). Similar patterns obtain for Erlenborn, who, according to

senior committee staffers, followed his predecessor's practice of letting the subcommittees go their own way.¹⁷

The Status of the Freshman, Revisited

Perhaps the most interesting results of Table 1 are those associated with freshman status. The negative effect of this variable is consistent and striking across each of the four models, even equalling the impact of subcommittee membership. However much their status may have improved, freshmen apparently have not achieved participatory equality, even when one controls for such things as marginality, full and subcommittee leadership position, and the relevance of committee legislation to member goals. This is not to say, however, that House norms enforced by senior colleagues keep newcomers off the committee stage, even after they are ready and willing to participate. Fenno (1973, 101-5) concluded that such norms were already moribund on House Education and Labor in the late 1960s, and his finding was confirmed by the staffers interviewed here. Rather, apprenticeship of a different sort operates in committee decision making, one deriving from the relative costs and resource constraints faced by the newcomer in learning a complex political process and sometimes esoteric subject matter.

Additional support for this argument is provided by Table 2. If the concept of apprenticeship used here is correct, we should expect to see the negative effects diminish as the members and their staffs gain experience and expertise over the course of the first term. Freshman status is here separated into its first- and second-year effects, and a dummy variable for sophomore status is included to provide a point of comparison. The patterns of coefficients are the same in the estimation of all four models. The negative effect is strongest in the first year, smaller but still significant in the second year, and smaller

still during the sophomore term, remaining significant only for behind-the-scenes participation. By the time of the second term, we might surmise, members have become more familiar with the decision making process, but they and their staffs have not fully developed informal lines of communication with the relevant senior actors.

Minority Status and Institutional Leverage

As hypothesized, minority status enhances participation in formal deliberations but not in informal ones; however, the evidence on the former is somewhat mixed. The effect on markup participation proves positive and significant in the full committee but not the subcommittee model. One reason for this may be that majority preferences are in relative flux at the subcommittee stage but become solidified in the form of the full committee markup vehicle. The full committee markup thus provides the final and most visible opportunity for the minority to express its objections as the bill moves to the floor.

That the coefficient for minority status is positive in the model of informal participation is also somewhat surprising, though the staff interviews pointed to elements of the institutional context that render this finding less anomalous. For the first time in nearly three decades, the 97th Congress ushered in Republican control of both the Senate and the White House. While this immediately and dramatically enhanced the status of Senate Republicans, Education and Labor staffers also cited positive effects for Republicans in the House. "Because we have a Republican Senate," one minority staffer explained, the committee majority "has come to recognize the value of getting the minority on board.... If they tick off the Republicans totally, what we will usually then do is get to the Senate people and say 'We need to kill this bill.' That unspoken

threat—the realization that the Republicans control the Senate and the White House—has been very important. During the Carter Administration you found that [the committee Democrats] were much less willing to accommodate Republican concerns." The institutional leverage of the Republican party, in short, created greater opportunities for minority participation generally (see also Fenno 1973, 75) even though it diminished the willingness of Democrats to negotiate with conservative Republicans acting as administration loyalists.

Conclusion

The model of committee participation developed here begins from the premise that congressmen are purposive actors, who participate because they want to. What members want is to advance their political goals. But having borrowed on a now common theme in the literature on legislative behavior, I here develop a more complex motivational psychology. If, as Fenno (1973) says, not all representatives are alike in the goals they harbor, then not all issues are relevant to particular goals. For the same member of Congress and for the same issue, not all legislative contexts will be equally suited to the member's goals.

This view of purposive behavior finds strong support in the results presented here. Four different goals—serving district interests, making good policy, making a political mark, and promoting the president's agenda—affect behavior on House Education and Labor. Subsidiary analysis suggests, moreover, that different goals are evoked by different issues and that motivational effects vary in predictable ways across legislative contexts. Not only are congressmen not, in Mayhew's phrase (1974), "single-minded seekers of reelection," to characterize them as single-minded seekers of any goal does considerable injustice to the complex

set of motives that shape their behavior, even on a single committee and a small sample of bills.

According to the model developed here, however, congressmen cannot always do what they want. In deciding whether and to what extent to participate in particular decisions, the committee members face a variable set of opportunities and constraints that structure their ability to act. Members and, especially, leaders of the subcommittee of jurisdiction enjoy advantages in terms of information, staff, and lines of political communication—resources that subsidize the considerable opportunity costs that participation entails. So do the chairman and ranking member of the full committee despite the relative diminution of these positions in the last decade. Each of these positions exhibits a strong, positive effect on participation in committee decision making even when we control for member interests. At the same time, the freshman member faces analogous resource constraints. In participation both formal and informal, at full committee and subcommittee, freshman status significantly diminishes a member's decision making role. The implication here is that apprenticeship remains important for understanding legislative behavior, though the sociological assumptions commonly imbedded in the concept no longer hold.

Perhaps the most important implications of this study, however, lie less in what it does than in the directions it suggests. For the study of legislative decentralization, the findings regarding subcommittee members and leaders pose several questions worth careful analysis (see also Hall and Evans 1985; Sinclair 1983). Under what conditions do subcommittee nonmembers actively review subcommittee actions? To what extent do the relative advantages of subcommittee position translate into influence over committee outcomes? To what extent do such advantages structure activity and

influence on the chamber floor? If we are to develop a theory of influence appropriate to a decentralized Congress, patterns of selective participation at each stage of the legislative process require greater attention (Hall 1986, chaps. 6 and 7).

For theoretical work on committee decision making, likewise, the present study suggests a new line of inquiry. Owing to Fenno (1973), one tradition in this area emphasizes member goals as the principal determinants of committee decisions. Price (1977; 1985) persuasively argues, however, that goals have proven too general to explain substantive policy outcomes. More formal models, in turn, build upon individual preferences to explain decisions, but such work has shown too little concern with observed patterns of committee behavior (Panning, 1983; but see Gilligan and Krehbiel 1986; Shepsle and Weingast 1986). The study of participation here suggests a potentially fruitful connection between these two traditions: the general interests that empirical scholars refer to as "member goals" shape the decision to participate; it is the preferences of participants that structure the legislative outcome.¹⁸ An education and labor bill to reform public pension regulation, for instance, was almost a simple summary of the preferences of subcommittee chairman Phillip Burton and ranking Republican John Erlenborn. The interests of very few others were even modestly evoked; their (ill-formulated) preferences thus never entered the decision-making calculus. A theory of committee decision making, in short, should not only tell us how players will bargain but how it happens that members come to be players in the first place.¹⁹

For students of representation, finally, committee participation should prove an important concern. Committees and subcommittees, we know, are not microcosms of the parent chamber. The way in which committee assignments are made guarantees that these groups are deep in

"interesteds" and unrepresentative of the regional, ideological, and seniority patterns evident in the parent chamber (Shepsle 1978, 259). But, as I suggest in this paper, the membership rolls provide only an inexact sketch of who makes the policy decisions at the committee level. Participation is selective and purposive. The range of interests, values, and geographic constituencies thus expressed on any given issue is likely to be even more narrow, the bias of interests represented more severe, than the study of committee assignments can reveal. Such behavioral patterns require greater attention if we are fully to understand the phenomenon of representation in a decentralized Congress.

Appendix

Sampling

The samples of members and bills analyzed here are very close to being the full populations for Education and Labor during the 97th Congress. I attempted to gather the appropriate data from both staff interviews and committee records for all 33 of the congressmen who sat on the committee for the entire congress, and I succeeded for 29 (88%). The sample of bills (17) represents 61% of all bills that received any kind of markup action during the two-year period. In choosing the sample, I included bills from the various subcommittees in rough proportion to the number that they reported for full committee action, but I generally excluded bills for which the legislative markup (both at full and subcommittee) was merely perfunctory. In the end, the sample reflected a mix of major and minor bills and spanned a range of domestic policy areas: education, welfare, labor-management relations, child abuse, national employment policy. The records were missing for the subcommittee markups of two bills in the sample.

Formal Participation: Data and Coding

Data on members' markup participation were gathered separately for subcommittee and full committee and coded by the author. The following criteria were used:

Attendance. A member who appeared at one or more of the markup sessions held on a given bill was credited with attending.

Voting. A member who personally responded to one or more roll calls during a markup was coded as a voting participant. While proxy voting is commonly practiced in House committees, senior committee staffers noted that specific voting instructions are seldom provided by the member relinquishing a proxy. Thus, voting by proxy was coded here as not having voted.

Speaking. Participation in markup debate was measured by counting the number of transcript lines each member spoke on a given bill and then classifying the counts into major and minor speaking categories. In making the classifications, several thresholds were tried without producing changes in the codes for more than a very few observations. I thus settled on the one that seemed intuitively most reasonable: if a member spoke more than "his or her share" (1 / 33 members, or 3%) of the lines in the markup transcript, the member was classified as a major speaker. If the percentage of lines spoken was greater than 0 but less than 3%, a member was classified as a minor speaker. In counting the lines of a given meeting, administrative remarks by the presiding member were eliminated, as were responses to roll call votes and quorum calls.

Amending. The data on this activity were coded according to whether each amendment offered (1) would make technical or grammatical changes in the legislation or was otherwise cited as being minor and

noncontroversial or (2) entailed substantive changes in the legislation. Where there was doubt about the appropriate category, the amendment was assumed to be substantive.

Agenda setting. A member was credited with setting the markup agenda if that member (1) called up or was otherwise responsible for authorizing the bill used as the markup vehicle or (2) offered and succeeded in passing an amendment in the nature of a substitute to the vehicle. While the markup records normally revealed who the principals were in constructing the vehicle, in a few cases committee staffers identified other members who played a significant agenda-setting role. In those cases, the member was coded on the basis of the staff reports.

As noted in the text, Guttman scale analysis was used to reduce these several kinds of data into a summary measure of formal participation. Using this technique, of course, generates the loss of a significant amount of information. A member's amending activity, for instance, is reduced from the number of amendments offered at a particular markup to whether the member offered any at all. But, with the exception of speaking and amending, the activities tended to be either dichotomous or trichotomous so that more sophisticated scaling was not possible. Thus it was not obvious what method of data reduction would be best. I here opted for one that captured several different kinds of activity, could be replicated easily, and provided comparability across bills in different committees and subcommittees.

Informal Participation: Data and Coding

The data on informal participation were gathered through committee staff interviews conducted in late 1983 and early 1984. The individuals with primary responsibility for staffing each bill in the sample were identified by the minority

and majority staff directors. Of the 23 that were identified (several staffers were responsible for more than one bill), 22 agreed to an interview, producing a response rate of 96%. Staffers were guaranteed anonymity, and when permission was given (91% of the time), the interviews were recorded. The transcripts were then coded independently by the author and a research assistant, using the following categories:

- 0 = none. No mention of congressman or congressman's agent in any behind-the-scenes capacity.
- 1 = minor. Member/agent cited as expressing support/opposition for or interest in some aspect of the bill, but reference to behind-the-scenes activity occurs rarely and only in passing.
- 2 = moderate. Member/agent mentioned as actively pushing one specific point of view or provision or as being party to a key compromise but not a frequent participant in behind-the-scenes deliberations.
- 3 = major. Member/agent one of the central players in most informal negotiations, compromises, staff meetings, and so forth. Perhaps some role in drafting provisions of markup vehicle but not a principal author.
- 4 = a principal author. Member/agent clearly responsible for the shape of the original vehicle, as well as playing leadership role in all subsequent negotiations, compromises, staff meetings, and so forth.

As noted in the text, intercoder reliability was high. Where disagreements did occur, we resolved them through a joint reconsideration of the item. When a mutually agreeable solution was not possible, the codes were averaged.

Personal Staff Interviews

Data on each member's goals were gathered from interviews with the staffer(s) who worked most closely with the member on Education and Labor affairs. These individuals were identified for me by senior committee staffers. The "best person to talk to" was interviewed for 29 of the 33 members. For 12 members (41% of the sample), a second qualified staffer was interviewed in order to check the reliability of the responses (see discussion in text). Anonymity was guaranteed, though these interviews were not recorded. Notes were taken during the interview and expanded immediately afterwards. The interviews were conducted from March to May 1984.

Notes

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1. The most important exceptions are Price (1972) and Fenno (1973, chap. 4). Other works that touch on the subject include Carroll (1958, 27-37), Clapp (1963, 264-68), Manley (1970, 90-97), Payne (1980), Perkins (1980), and Smith and Deering (1984, chap. 5). The importance of participation in political decision making has been a more direct concern of students of organizational behavior, however (see, for instance, Cohen, March, and Olson 1972; and March and Simon 1958).

2. Even if key provisions or compromises are proposed and discussed prior to markup, they must then be offered in markup (either as part of the markup vehicle or as separate amendments to it), justified or opposed by the actors involved, and then formally ratified or rejected. Thus, even though much may go on outside the markup, the markup

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offers some indication of who participates behind the scenes.

3. It is important to emphasize that the unit of analysis here and throughout is neither the congressman nor the bill. Rather, it is the act of participation or, to be more precise, the series of concrete acts that can be summarized as the participation of congressman i on bill j . The full committee data set thus includes the number of congressmen (29) times the number of bills (17) in the sample, or 493. See also Kingdon (1981, 15-16).

4. There are, however, certain inherent reasons why the data should be hierarchically structured. For instance, it is logically impossible for one to speak at a committee markup without attending it. The more detailed set of categories, in fact, builds greater hierarchy into the scale. As a more reasonable test, a separate analysis was conducted that included only those items not logically ordered. The results of this analysis were likewise clear: the coefficient of reproducibility was still .95 and the coefficient of scalability, .79.

5. Lest the coder reliability appear better than it is, I should point out that for all but two bills in the sample (indeed for most bills that move through the committee system), only a fraction of the members was at all involved in behind-the-scenes activities. The majority of members are thus never mentioned, their participation score obvious. When the test is confined to the subset of cases where a congressman is mentioned in the interview, however, the reliability remains satisfactory: $r = .78$.

6. Some notion of a goal hierarchy remains in Kingdon's model in that congressmen first consider the relevance of a vote to their districts; only after this will they consider the goals of good policy and intra-Washington influence. But Kingdon suggests that specific goals are evoked by the issue under consideration so that certain goals may simply drop out of the decision calculus (1981, 247). While his analysis focuses primarily on the goal of reelection, Shepsle's study of committee assignment requests (1978, esp. pp. 45, 103-4) entails similar motivational assumptions.

7. These indicators thus indirectly tap two dimensions of policy content—salience and conflict—that other legislative scholars have cited as important determinants of legislative behavior (e.g., Fiorina 1974; Hinckley 1975; Kingdon 1981; Price 1978). The difference here is that I capture the subjective perception of salience and conflict, specifically focused on the member's own constituency. I thus need not assume them to be objective characteristics of a bill nor that, say, national salience, intersubjectively perceived, is equivalent to political salience for the individual congressman. Regarding such assumptions, see Hinckley 1975, 545-56; and Price 1978, 568-69.

8. The area the member "carves out," of course, is often one where he or she has well-defined, intense policy interests. The interviews suggested that this

does not always hold, however, and the correlation between the indicators for the policy and personal mark goals was relatively modest ($r = .27$).

9. The high reliability in this case derives from the fact that any given congressman can stake credible claims to only a very few issues. Thus most of the bills were never cited in the course of an interview and were coded zero. Still, staffers were fairly consistent in identifying bills for which the congressman felt some territorial interest, and the accompanying discussions confirmed that they interpreted the question properly. Where the test of reliability is confined to those bills where there was at least one nonzero value, the interstaffer correlation was .68.

10. For the period of this study (1981-82), the presidency and the committee were controlled by different parties, so that the goal of promoting the president's agenda is zero for all observations of subcommittee chairmen. It thus drops out of the analysis here.

11. To a lesser degree, the same argument applies to new transfers. None appeared in the sample under study here, however, so that transfer status drops out of the model.

12. This summary measure is useful because the subcommittee markup measure does not comprehend participation at the full committee stage, and the full committee measure does not comprehend the fact that subcommittee members need not (always) repeat at full committee what they have achieved in subcommittee.

13. The results are somewhat mixed on this point however. Given the growing importance of subcommittees in writing legislation (Davidson 1981), we should expect the effect of this variable to be greater on subcommittee participation than full committee participation. While the coefficient is correct in sign at the subcommittee level, however, it is not significantly far from zero. One problem this no doubt suggests is that I am not altogether able to disentangle the effects of the congressman's desire to make a mark (as it is evoked by a given bill) from the effect of subcommittee leadership position.

14. Were this the case, however, we would expect to see significant goal-congressman interaction effects. The equation was thus reestimated with dummy variables for each congressman interacting with each of the goal variables. In the estimates for formal participation, only nine out of a possible 92 interactions were significant at the .10 level, the variance explained was not appreciably enlarged, and the other coefficients were relatively stable. Nearly identical results obtained for the reestimation of informal participation as well.

15. This is not to say, however, that the ranking members or the subcommittee units control committee outcomes. Making inferences about legislative influence from such data requires considerable caution (see Hall and Evans 1985; and Hall 1936, chap. 6).

16. Insofar as I am able to measure, at least. Sub-

committee chairs tend to score at or near the top of the scales used here; thus the negative coefficients may partly be due to the scales being bounded. Considerable interview evidence corroborated the interpretation offered here, however (see Hall 1986, chap. 3).

17. Erlenborn was also new to the position. In fact, he did not assume ranking status until the death of John Ashbrook midway through the period studied here.

18. Of course, the distinction between goals and preferences is hardly a clean one; in fact, the two terms are often used interchangeably. As I use them, the evocation of some general goal determines whether (and with what intensity) a member will have crystallized preferences and the dimensions along which those preferences will be ordered (see Hall 1986, chap. 7).

19. The tendency of formal theorists to ignore nonparticipation follows from the exclusive attention given to one form of participation—voting. Since votes are more or less valuable commodities and the act of voting requires little in the way of time and resources, nonparticipation is seldom a rational option. However, the more general assumption that participation is cost free, this study suggests, is wholly unrealistic.

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INSTITUTIONAL STRUCTURE AND THE LOGIC OF ONGOING COLLECTIVE ACTION

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Work by Axelrod, Hardin, and Taylor indicates that problems of repeated collective action may lessen if people use decentralized strategies of reciprocity to induce mutual cooperation. Hobbes's centralized solution may thus be overrated. We investigate these issues by representing ongoing collective action as an n-person repeated prisoner's dilemma. The results show that decentralized conditional cooperation can ease iterated collective action dilemmas—if all players perfectly monitor the relation between individual choices and group payoffs. Once monitoring uncertainty is introduced, such strategies degrade rapidly in value, and centrally administered selective incentives become relatively more valuable. Most importantly, we build on a suggestion of Herbert Simon by showing that a hierarchical structure, with reciprocity used in subunits and selective incentives centrally administered, combines the advantages of the decentralized and centralized solutions. This hierarchical form is more stable than the decentralized structure and often secures more cooperation than the centralized structure. Generally, the model shows that the logic of repeated decision making has significant implications for the institutional forms of collective action.

The logic of collective action is rather discouraging. As Mancur Olson stressed in his pioneering work (1965), common interests may not sustain common effort. Although every potential member of an interest group may benefit from a piece of legislation, if a person could enjoy the fruits of the legislation without contributing to the collective cause, then pure self-interest will induce that person to free ride on the actions of others. And as for interest groups, so for political parties, labor unions, professional associations, indeed any collectivity that relies on voluntary action. The logic cuts a wide swath across political organizations.

Not all organizations, however. Olson argued that small groups tend to be privi-

leged in that they are more likely to realize collective goals effectively. It is large groups that suffer most acutely from free rider problems, to the point that a large potential group may fail to form at all. Olson and others who have examined the tendency of large groups to remain latent have proposed four reinforcing reasons:

1. If the benefit in question involves rival consumption—Hardin's commons, for example—then the per capita share of benefits falls as the collectivity increases.
2. On the production side, the contributions of individuals may become less important to overall group performance as size increases.
3. Problems of communicating, organizing, deciding—the transaction costs of

interest groups—intensify in large groups.

4. Social incentives, positive ones such as feelings of solidarity and negative ones such as shame, are weak and unreliable in large organizations.

Olson, recognizing that some large groups (labor unions, professional associations, farmers' organizations) have been viable and often effective for prolonged periods, argues that this is due to a finessing of collective action problems. In such groups it is selective incentives that stabilize participation; the collective goals are achieved as a byproduct of individual contributions aimed at the selective rewards (1965, 132). Where selective incentives are infeasible—for example, cartels, which are legally precluded from punishing firms that break the cartel's policy—cooperation is fragile, prone to dissolving into bouts of mutual defection.

Olson's work stimulated a fundamental rethinking of the origin, strategy, and performance of interest groups. Inevitably, this ambitious and influential theory has been criticized on several fronts. It has been noted that his prediction about the effects of size on overall group performance does not apply to nonrival goods for which the significance of individual contributions declines slowly as a group increases in size (Chamberlain 1974). In such circumstances, total group output rises with total membership. Thus, collective action problems do not inevitably become prisoner's dilemmas as groups become larger (Taylor 1976, 17–25).

Most importantly, perhaps, Olson's analysis is static, depicting collective action as a one-shot game. When a single-period game satisfies the conditions defining a prisoner's dilemma, then the collective action problem is indeed serious, for it is well known that in the one-shot prisoner's dilemma, defection—free riding—is a dominant strategy. This is an extremely strong property. When one strategy dominates all others, the dominant strategy is a player's best move no

matter what everyone else does. Moreover, the dominance of defection in a multiperson prisoner's dilemma holds regardless of the number of players: it is as true for two as it is for two thousand.¹

However, it is also well known that defection is *not* the dominant strategy if the situation is repeated over time, as are many collective-action problems. Indeed, in the iterated prisoner's dilemma, there is no dominant strategy so long as the future is sufficiently important. In the repeated context, interdependence is paramount: what is best for player 1 depends on player 2's moves and vice versa. And again, this property holds regardless of the number of players. Even in enormous groups there need not be a dominant strategy, not even defection.

This last statement may occasion some surprise, but it is true. It is also consistent with Olson's formal analysis, for his model covered only the one-shot prisoner's dilemma. Although it is tempting to extend the conclusions outside the scope of his model, such extensions may be unwarranted. It is intuitively clear that all sorts of political arrangements are possible when there are repeat encounters, arrangements that may be impossible in one-shot affairs.

Most importantly, it is possible to sustain cooperation in the iterated game, a point noted by political scientists (Axelrod 1984; R. Hardin 1982; Taylor 1976) and economists (Friedman 1971; Radner 1980, 1986). The basic idea in all these works is conditional cooperation: offering stick-and-carrot combinations of rewarding cooperation with cooperation and punishing defection with defection. So long as one's partner does not discount the future too heavily, such strategies can secure the cooperative outcome because the short-term gains of double cross will be outweighed by the long-term costs of being mired in mutual noncooperation. Hence, joint cooperation is a Nash equilibrium, that is, holding fixed the strategy

of one player, conditional cooperation is the other's best response. Thus, repetition matters a great deal.² The one-shot game has only one Nash equilibrium—joint defection; the iterated game has many equilibria, including cooperative ones.

This benign potential of repetition has led students of the prisoner's dilemma to reconsider the necessity of centralized solutions. Hobbes ([1651] 1968) and his intellectual descendants such as Garrett Hardin (1968) have seen the authority of government as the key remedy to the shortcomings of decentralized choice. Because everyone may want the collective good supplied or, as in the "tragedy of the commons," protected, an imposed policy may win unanimous support. However, in the setting of the repeated game the centralized solution may be unnecessary (Axelrod 1981, 316; Taylor 1976, 9–12).

Yet despite the potential of decentralized strategies of reciprocity to enforce cooperation, these scholars doubt that the argument can be extended to the large numbers case. As Taylor comments in a subsequent book,

Unfortunately, [voluntary cooperation] is less likely to occur in large groups than in small ones, since a conditional cooperator must be able to monitor the behavior of others in the group so as to reassure himself that they are doing their parts and not taking advantage of him. Clearly, as the size of the group increases, this mutual monitoring becomes increasingly difficult and the "tacit contract" of conditional cooperation becomes increasingly fragile. (1982, 53)

Axelrod (1986) and Russell Hardin (1982, 53) have related reservations, as does Olson (1982, 23–35).

These doubts are buttressed by evidence. Robyn Dawes, reviewing a large body of experimental work, notes that "all experimenters who have made explicit or implicit comparisons of dilemma games with varying numbers of players have concluded that subjects cooperate less in larger groups than in smaller ones" (1980, 186). Researchers studying reciprocity in the field usually describe dyadic

ties or small groups (Schmidt, Scott, Lande, and Guasti 1977, *passim*). The restriction of strategies of conditional cooperation to the small numbers case appears to be a robust empirical regularity.³

These theoretical speculations about the fragility of conditional cooperation in large groups, as well as the evidence indicating that it is most common in small n settings, suggest that a close inspection of the repeated game leads us full circle back to Olson's main conclusions. His static analysis of the difficulties confronting large groups seems to have a dynamic equivalent. Though at first glance iterated encounters appear to solve the dilemma, in real organizations the problems of monitoring, of misperception, of uncertainty about the underlying strategies of one's colleagues, cast doubt on the ability of decentralized strategies of reciprocity to stabilize cooperation.

Our aim in this paper is to formalize these intuitions about the fragility of cooperation in the repeated game involving large numbers and to show that this instability has implications for the institutional structure of interest groups and other organizations striving for collective benefits. Our analysis is game-theoretical. We represent the problem of collective action as an n -person repeated game under uncertainty. We do so not to introduce mathematical complexity for its own sake but to capture the mundane yet vital problems faced by political organizations.

To provide a baseline of comparison, we first analyze the simple context of perfect monitoring. Here we contrast a decentralized structure, where everyone uses trigger strategies of conditional cooperation, with a centralized regime that, in the manner of Olson's by-product theory, selectively administers inducements and punishments. In this baseline section the decentralized régime fully exploits the advantages of repeated encounters, uncontaminated by informational problems.

In the second section we show that,

once one introduces some realistic monitoring problems, Olson's insights into the difficulties facing large groups can be sustained in the context of repeated play. The model reveals that monitoring imperfections and causal uncertainties degrade the value of decentralized strategies in large groups. Simultaneously, such informational problems enhance the relative performance of the structure that centrally administers selective incentives—much as Olson had argued in his by-product theory of interest group viability.

Most importantly, we show in the third section that these problems of monitoring and inference can also give rise to a federated or nested institutional structure (Simon 1969) for very large groups. We take up two types of nested structures. The first structure combines decentralized reciprocity strategies at the local level with coarse selective penalties administered by a central office. Hence, the nested structure fuses the advantages of conditional cooperation (anarchy) and centralized authority (the state). By so doing, it illustrates that the repertoire of institutional forms is much richer than the classical pair of anarchy and the state on which so much attention has been focused. Via the second structure we explore a hypothesis of Russell Hardin that a federation of local units may sustain cooperation in a decentralized manner, without any central authority at all (1982, 196–197).

Perfect Monitoring

For concreteness, we interpret our organization as an interest group lobbying for a collective benefit. Each member of the organization can either work hard (cooperate) or slough off (defect). For simplicity, we normalize the effects of each agent's choices on output: if a member participates by working hard, then one unit of output is created; if the member shirks, no output is created.

Since this perfect monitoring case is deterministic, the relation between effort and output is certain; every member can guarantee a contribution of one or zero. We assume that no one directly observes the effort of anyone else but everyone observes the collective output perfectly. Further, everyone knows the individual and organizational production functions. These latter assumptions allow everybody to make inferences about effort, that is, to infer what proportion of the organization was working hard and to do so without error. (Note, however, that the identity of shirkers cannot be ascertained.)

Collective benefits increase linearly in the contributions of the members, at a rate of b per unit of output. A member who participates bears a personal cost of c . For simplicity we assume that everyone is risk-neutral, so utility is linear in net benefits. If k_i denotes whether member i is working ($k_i = 1$) or shirking ($k_i = 0$), the resulting utility for that member is

$$U_i = \left(\frac{b}{n} \sum_{j=1}^n k_j \right) - ck_i \quad (1)$$

This implies that all members of the group face the same strategic situation, an assumption that we will drop shortly.

If everyone cooperates, the output will be n , and the gross benefits will be bn . Thus for a divisible good the payoff equation (1) indicates that under full cooperation every member will reap net benefits of $bn/n - c = b - c$. To ensure that the situation is a prisoner's dilemma, we assume that

$$b > c > \frac{b}{n} \quad (2)$$

Thus everybody is better off if everybody cooperates ($b - c > 0$), but in the one-shot game it is a dominant strategy to defect since the personal gain of participation b/n is smaller than the private cost c .

Note that the benefit share of each person— b/n —allows us to interpret the col-

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lective good as one characterized by non-rival consumption (a pure public good), in which case b/n denotes the productivity of a person's contribution. In the context of a pure public good, the productivity of each individual falls as the group grows. In the context of a collectively produced good exhibiting rival consumption, productivity can be independent of group size, but the benefits must be shared with more people. In either case b/n falls as the organization grows larger. To simplify the exposition, we will always refer to the case of rival consumption, wherein the per capita benefit of a unit of output is always b/n .

Condition (2) ensures that the group is large enough that rival consumption or decreasing marginal productivity of individual effort guarantee that each member's choice is a prisoner's dilemma. The result, in the one-shot game, is that everybody shirks and no collective good is provided. In the repeated game, however, collective defection is no longer the only equilibrium.

In the repeated game each person's utility is just the discounted sum of his or her utilities in the basic game. We assume that in every period there is a constant probability that the game will end or, equivalently, that the game goes on forever but the members discount the future. In either case, each member maximizes the discounted sum of his expected utility; if $U_{i,t}$ denotes the expected utility of player i in period t , i 's objective is to maximize

$$\begin{aligned}W_i &= U_{i,1} + \delta U_{i,2} + \delta U_{i,3} + \dots \\&= \sum_{t=1}^{\infty} \delta^{t-1} U_{i,t},\end{aligned}$$

where the parameter δ captures both the probability of termination and the players' time preference. Naturally,

$$0 < \delta < 1$$

In the context of a repeated game, it is well known (see, e.g., Friedman 1971)

that, so long as the members do not discount the future too much, an appropriate trigger strategy will produce enduring cooperation by all members. The strategy is simple. With a group of size n , if everyone works hard the output will be n every period. Accordingly, the strategy is to adopt the following cutoff rule: if the output falls below n in any period, do not participate for the next T periods. With this rule, punishment is triggered if even one member sloughs off. Note that since the group's payoff is a deterministic function of individual effort, punishment is never invoked erroneously: the inference that output fell below the maximum because someone was free riding is perfect. The Appendix proves the propositions that follow.

Proposition 1

If relative to group size n the punishment phase is sufficiently long and the members do not discount the future very heavily, there is an equilibrium in which all members cooperate every period. However, given the discount rate, there is an upper size limit n^ beyond which the cooperative outcome cannot be upheld.*

This outcome is a Nash (best response) equilibrium, as it will be in all subsequent propositions.⁴

The length of the punishment phase, T , depends upon how much the members discount the future: the less the future matters, the longer punishment must last to support the cooperative outcome.⁵ More specifically, T must be large enough so that

$$\frac{1 - \delta^{T+1}}{1 - \delta} > \frac{b - b/n}{b - c}, \quad (3)$$

and δ must be large enough so that

$$\delta > \frac{c - b/n}{b - b/n} \quad (4)$$

Rearranging the righthand side of (4) to yield $nc - b/nb - b$ is instructive, for it shows that this term is increasing in n , the group size. Thus for any fixed δ there is a critical threshold on group size, n^* , beyond which cooperation cannot be sustained. The value of this critical size is a function of the costs and benefits of participation, as well as of the discount parameter.⁶ The intuitive reason for this effect is as follows: The benefit of cooperation in any period is independent of group size. Each member obtains a payoff of $(b - c)$ if everyone participates; on the other hand the short-run gain to a member by shirking ($c - b/n$) is higher in larger groups. Consequently for a given δ , the loss from suspended cooperation in later periods $(\delta(b - c)/1 - \delta)$ is independent of group size n and for large enough groups is insufficient to deter members from realizing the short-term gains from shirking.

Before moving on to the centralized regime, we wish to generalize this result. The generalization is interesting in its own right and will prove useful in later sections of the paper. Typically, the prisoner's dilemma is analyzed under the assumption of symmetric payoffs, and proposition 1 used that assumption. This condition is often not satisfied empirically. For many reasons, participation is more costly for some people than for others. For example, in all interest groups some members have more political skill, some less. Because ability and effort are partial substitutes, the politically inept must exert more effort to generate the same contribution. Thus, cooperating imposes higher private costs on the low-ability members.⁷ Though they face higher private costs, we assume that even the low-ability members face a prisoner's dilemma: they, too, are better off if everyone contributes than if no one does. Thus we have two levels of c satisfying

$$b > c_1 > c_2 > \frac{b}{n}$$

We assume that one's ability is private information, though everyone knows the distribution of ability in the population at large. Then provided that conditions (3) and (4) hold with c replaced by c_1 , it is straightforward to prove the proposition that follows.

Proposition 1a

If the punishment phase is sufficiently long and members do not discount the future too heavily, there is an equilibrium in which both low and high cost members cooperate.

It is important to note that both propositions cover large groups, subject to the qualification (noted above) that the discount factor be sufficiently large relative to group size. With both symmetric and asymmetric payoffs, completely decentralized strategies can produce the organizational equilibrium of full cooperation. This is not particularly intuitive. Hardin, for example, doubts that decentralized strategies will work in very large groups (1982, 40). We agree. The point of propositions 1 and 1a is twofold: first, to exhibit conditions that suffice for supporting a decentralized, cooperative equilibrium; second, to help us examine the robustness of the result. Note that the informational conditions are very restrictive. Everyone observes the group outcome *without error*, and everyone knows that the relation between effort and outcome is deterministic. Together, these two conditions guarantee that everyone knows that payoffs can fall only because of deliberate shirking. There are no external disturbances that could unpredictably reduce rewards. Consequently, the group need not worry about mistakenly triggering a punishment phase. Accordingly one cannot make the trigger strategy too draconian, for the punishment phase will never occur in equilibrium.

In such an informationally simple world, Hobbesian solutions of centralized authority are unnecessary. However, to

provide a baseline of comparison for later sections, we introduce a centralized regime. In this arrangement, the members of the interest group establish an association with a central office or headquarters. Headquarters is given the authority to monitor the members and to expel shirkers from the association.⁸ Because shirkers are no longer members in good standing, they cannot receive the benefits that accrue to members of the association.

Thus, this regime presumes either that the good in question is excludable—not a pure public good—or, as in Olson's by-product theory, that the organization has recourse to auxiliary incentives that can be selectively withheld from shirkers, in amounts calculated to offset the free collective good enjoyed by a defector. As Olson noted, these selective incentives may be only loosely related to the manifest political purposes of interest groups (e.g., legal insurance provided at lower rates for AMA members); their essential feature is that they can be allocated in a discriminating manner. Naturally, organizations that can neither expel members nor selectively withhold auxiliary benefits have a smaller strategic repertoire, for their only feasible strategies involve imposing rewards and punishments diffusely on the entire group, as in the trigger regime described earlier. Though propositions 1 and 1a indicate that recourse only to diffuse schemes is not a problem when all inferences are certain (and the group is below the critical size n^*), it seems plausible that in the real world this limited repertoire would impair group performance, much as Olson argued. We shall show below that there are good reasons for believing this to be so.

We assume there is a fixed probability q that a member will be monitored in a given period. This probability is independent over time. If the member is observed and found to be shirking, he or she is expelled and randomly replaced by someone from the outside population.⁹ One

might imagine a queue of outsiders who desire admittance into the group. Being expelled yields the same reward as when everyone free rides. Expulsion is permanent.

In such a regime the intensity of monitoring—the value of q —is clearly pivotal. If monitoring is very good (q sufficiently close to 1), everyone will participate since by assumption both low- and high-skill members are better off contributing than being thrown out. If monitoring is very bad, no one will participate. The interesting circumstance occurs when q takes on intermediate values. The difference between low and high skill can then matter a great deal, particularly if the private costs of participation vary significantly. Because members with little political ability incur high private costs of contributing to the group's lobbying campaign, it seems plausible that they might be tempted to shirk as long as they were unlikely to be caught. High-ability members on the other hand might not risk expulsion. This intuition is correct.

Proposition 2

Assume that the two types of members experience sufficiently different private costs of participation and that the probability of detection is intermediate. Then if there are enough high-ability types, there is an equilibrium in which all low-ability members free ride and all high-ability members contribute.

Note that in such a world, observing a member shirking even once reveals with certainty that he or she is of low ability. Since such members will never contribute to the common weal, all other members will agree to expel the deviant forever, for there is a chance that the replacement will have high ability and will participate. Accordingly, the policy of permanent expulsion is sensible.

The relevant range for the monitoring probability q is

$$\frac{1-\delta}{\delta} \frac{c_2 - b/n}{b(1-f) - c_2} < q \\ < \frac{1-\delta}{\delta} \frac{c_1 - b/n}{b(1-f + 1/n) - c_1} \quad (5)$$

This condition expresses a simple property: for high-cost participants, the possibility of being observed must be small relative to the benefit-cost ratio of defecting versus contributing ($c_1 - b/n$ versus $b(1-f + 1/n) - c_1$, respectively); for low-cost contributors, the opposite must hold. The private costs of participation of the two types of members are sufficiently disparate if the following inequality is satisfied:

$$b > c_1 > \frac{b}{n} L_1 > \frac{b}{n} L_2 > c_2 > \frac{b}{n}, \quad (6)$$

where

$$L_1 = \frac{1-\delta+\delta q + \delta q n(1-f)}{1-\delta+\delta q}, \text{ and}$$

$$L_2 = \frac{1-\delta + \delta q n(1-f)}{1-\delta + \delta q}$$

If $(1-f)$, the fraction of hard-working high-ability members in the group is high, membership in this group is valuable. Condition (6) states that the risk of being expelled from such a successful group is sufficient to deter high-ability individuals from shirking but not the low-ability members.

Note how this proposition contrasts with proposition 1a. Whereas the decentralized trigger strategy enforces participation from everyone, the centralized regime allows some members to free ride. Proposition 2 accords with the folk wisdom about the distribution of work in organizations: a fraction of the population does most of the work. Of course, if the probability of detection is very high,

no one will risk shirking, and the two kinds of solutions will produce the same result of universal cooperation. But even then, there is no positive reason to turn to the Hobbesian solution. Thus, under the informational conditions described above —flawless observation of group payoffs and individual contribution a known function of individual effort—the case for centralized authority seems fairly weak.

Decentralized and Centralized Structures with Imperfect Monitoring

It is unlikely that such pristine informational conditions obtain in many interest groups. Therefore let us now introduce the merest smidgen of uncertainty in the relation between members' efforts and group performance (real or perceived). For instance, a member may try but fail to contribute to group performance because of an obstacle beyond the member's control. A person may be unable to come to an important meeting, despite best intentions, because of a sudden crisis at home. Or, despite everyone's contributing, group performance may be misperceived to have been inadequate.¹⁰ In this section, we shall examine the consequences of very small errors of perception or external uncertainties and argue that the relative performance of the decentralized and centralized structures may alter dramatically compared to the world of complete certainty.

The environmental disturbances or monitoring errors that arise in real organizations are, typically, imperfectly correlated across different members. Disturbances can be purely idiosyncratic, affecting the link between effort and contribution of a single member. Or they can be systematic, for example, a surprisingly sharp recession that simultaneously impairs the well-being of everyone in an interest group. The degree of correlation can thus vary greatly. However in what

follows we shall consider only the case of idiosyncratic disturbances.¹¹ Thus, we shall assume that a member's participation, instead of guaranteeing a high perceived contribution, will do so with some probability \bar{p} . With probability $(1 - \bar{p})$, which we shall take to be small, a member's hard work comes to naught in that period. On the other hand, free riding continues to guarantee a contribution of zero.¹²

The introduction of this tiny bit of uncertainty opens a wedge between collective output and individual effort. With the rest of the informational assumptions unchanged, it is now possible that output will fall below n despite everyone's working hard. The problem is that such a shortfall could also be due to free riding. To deter individuals from shirking in the decentralized regime, they have to retain the noncooperative phases if group performance is deemed inadequate. Because such punishment phases will occasionally be triggered off by mistake, the expected utility of a representative member will be reduced relative to an error-free world of perfect inferences. How far it falls obviously depends on the probability that group performance falls below the commonly agreed-upon cutoff, despite maximal effort from all members.

This probability of breakdown is sensitive to two factors: (1) the cutoff level demarcating adequate from inadequate group performance and (2) the size of the interest group. The latter factor is important because of the independent nature of uncertainties in the contributions of different players. For example, if the cutoff level is maximal group performance n , group output will clear this with everyone participating only if everyone contributes successfully. The probability of this occurring is \bar{p}^n , which drops surprisingly fast as n increases. For example, if n is 50—a rather small interest group—and $\bar{p} = .99$, then the probability that everyone contributes successfully is only .605. Con-

sequently, in every period there is almost a 40% chance of triggering a punishment phase of universal shirking, even though no one was at fault. Large groups may respond to this problem by lowering the cutoff level, a point we shall address shortly.

If even intense effort by all concerned is so likely to lead to a breakdown of cooperation, one may doubt whether it is feasible in large groups to provide members with adequate participation incentive. However, it is possible to show that if the uncertainty at the individual level is small, then such incentives can be adequately provided by the threat of reversion to noncooperative phases.

Proposition 3

Assume that individuals do not discount future payoffs too much, in the sense that condition (4) holds. Then for a given group size, if the exogenous idiosyncratic uncertainty $(1 - \bar{p})$ is small, there is a trigger strategy equilibrium where each member is motivated to participate in normal periods.

However for any given rate of discounting and size of the group, there may exist many trigger strategy equilibria corresponding to different choices of the performance cutoff level (denoted by C_n for an n -size group) and the length of punishment periods (denoted by T_n). It is important to note that choice of C_n and T_n are significant elements in the design of the decentralized organization; they specify the mutually agreed standard defining adequate group performance and the consequences of failing to meet this standard. The feasible alternatives for $\{C_n, T_n\}$ are those that induce all members of the group to participate in normal cooperative periods.¹³ Further, both choice variables will affect the ex ante payoff to each member: C_n will define the probability of breakdown of cooperation; T_n , the num-

ber of periods of defection thereafter.¹⁴ It can be shown that members of different abilities will rank different feasible alternatives $\{C_n, T_n\}$ identically, so the group would unanimously approve the cutoff level and punishment period that maximizes the ex ante payoff of a representative member, from among the feasible set. Because such a choice would express the best each member could expect from the decentralized regime, it is thus relevant in evaluating members' preferences between this and alternative organizational forms such as the centralized regime. It would thus be extremely convenient to derive a complete analytical description of the best choice of $\{C_n, T_n\}$ for any group size n . Such a result still eludes us. We can, however, describe some of the properties of the best trigger strategy equilibrium.

Proposition 4

(1) *The best cutoff level C_n for a group of size n must be less than $n\bar{p} + 1$, where $n\bar{p}$ is the expected level of group performance when all members participate; (2) if for a given cutoff level C_n , the threat of a T -period punishment phase motivates all members to participate in a normal period, then the threat of any longer ($T' > T$) punishment phase also motivates them but generates a lower expected payoff for every member.*

The first part of this proposition says that the cutoff level should not be significantly larger than the expected group performance in a cooperative period. The intuitive reason is that cutoff levels much higher than the mean make a breakdown of cooperation too likely and can be lowered while preserving participation incentives. But if the cutoff level is smaller than or equal to the mean, lowering it may destroy incentives. The second part of the proposition implies that if a particular cutoff level C_n can feasibly provide participation incentives (i.e., combined with

some T -length punishment phases), the group should choose the smallest T that provides these incentives in conjunction with C_n .

Given only such a limited analytical characterization we shall be forced hereafter to concentrate on some numerical examples where we can explicitly calculate the payoffs group members can get from the best trigger strategy equilibrium. Such payoffs will be compared to what they can get from the centralized regime. Before we do this, we need to verify that with small idiosyncratic uncertainties, the centralized regime will be characterized by behavior similar to that in the complete certainty context.

Proposition 5

Assume (5) and (6) hold and that members detected shirking in the centralized regime are permanently expelled. Then if the idiosyncratic uncertainty ($1 - \bar{p}$) is small, there is an equilibrium in the centralized regime where every low-ability member always shirks and every high-ability member always participates. Further, the ex ante payoffs in this equilibrium to every member can be explicitly calculated.

Note that with small uncertainties ($1 - \bar{p}$) in the contribution of a participating member, a given high-ability member may accidentally generate a low contribution and be expelled permanently with probability $q(1 - \bar{p})$.¹⁵ Thus the expected payoff of a high-ability member is lower than in a world of no uncertainty or error. However, what is of interest is this payoff relative to the best members can expect from a decentralized trigger strategy regime.

In a numerical example we have computed the best payoffs players can expect in the decentralized structure when the chance of an idiosyncratic disturbance is

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Table 1. Performance of the Decentralized Regime under Imperfect Monitoring

Group Size	Cutoff (C_n)	Shortfall Allowed	Punishment Period (T_n)	Probability of Collapse	Expected Payoff, High Ability	Expected Payoff, Low Ability
25	24	1	2	.026	4,552	3,793
50	48	2	5	.014	4,522	3,768
75	72	3	20	.007	4,506	3,755
100	97	3	7	.018	4,374	3,645
125	122	3	4	.037	4,258	3,549
150	146	4	11	.018	4,277	3,564
175	171	4	6	.032	4,185	3,487
200	195	5	20	.016	4,218	3,515
225	220	5	10	.027	4,110	3,425
250	245	5	7	.041	3,982	3,318
275	269	6	19	.022	4,062	3,385
300	294	6	11	.033	3,952	3,294
325	319	6	8	.047	3,832	3,193
350	343	7	21	.026	3,931	3,276
375	368	7	12	.037	3,839	3,199
400	393	7	9	.050	3,721	3,101
425	417	8	26	.029	3,818	3,181
450	442	8	15	.039	3,707	3,089
475	467	8	11	.052	3,596	2,996
500	491	9	49	.031	3,717	3,099
1,000	986	14	22	.082	2,848	2,373
1,500				not sustainable		

one in a hundred. In Table 1 we show how the decentralized structure performs in groups ranging from 25 to 1,500.¹⁶

The table reveals two important patterns. The first was anticipated: the larger the group, the smaller both average payoffs tend to be. There are some minor exceptions, but the overall decline is clear. For example, in a group of 1,000 the payoff of a high-ability member is 62% of the payoff's value in a group of 25. The centralized structure—which is insensitive to size—gives high-ability members a payoff of 4,289, thus beating the trigger strategy regime in groups of 125 or more. The deterioration of the decentralized structure is due to the increasing fragility of decentralized cooperation; other things being equal, the larger the group the bigger the chance that an idiosyncratic disturbance will drive the group's performance below the cutoff. Thus in an organi-

zation of 1,000, there is in every period more than an 8% chance of an accidental breakdown of cooperation, compared with less than a 3% chance in the smallest group. And this occurs even though the large group reduces its fragility by tolerating larger shortfalls in performance, C_n being 14 below maximum performance when $n = 1,000$ but only one below the maximum in the smallest group.¹⁷

One might wonder whether the instability problem can be further ameliorated by lowering the cutoff levels even more than reported here. This, however, will not work, since doing so makes the group too likely to achieve the target level even if the n^{th} player deliberately shirks. Thus, cutoff levels smaller than Table 1's do not provide adequate effort incentives. Thus the decentralized structure faces a trade-off between increasing its stability and maintaining incentives for participa-

tion. Reducing the cutoff level enhances stability but it also eventually makes individuals too dispensable, destroying participation incentives.

Naturally, if the monitoring uncertainties are less pronounced (\bar{p} closer to 1), the decentralized regime is less unstable and its performance degrades less quickly as size increases. Nevertheless, the qualitative pattern remains the same: even if the chance of a disturbance is only one in a thousand, size impairs performance. (The authors will provide a printout of results for the case of $\bar{p} = 0.999$ upon request.) Thus even in this nearly deterministic setting the centralized institution beats the trigger regime in moderately large groups, in particular those over two thousand members. As conjectured, the centralized regime is superior in large organizations.

The table's second important pattern is indicated by its last column. By doing calculations for every possible cutoff level, we found that no $\{C_n, T_n\}$ pair supports cooperation in a group of 1,500 members when $\bar{p} = 0.99$. This result generalizes.

Proposition 6

Given the level of uncertainty $(1 - \bar{p})$ in individual contributions corresponding to maximum effort, there is a critical group size n^ above which there is no cooperative trigger strategy equilibrium.*

This result is relatively intuitive. The equilibrium of the decentralized regime requires that if $n - 1$ members cooperate, it must be sufficiently likely that the n^{th} member will be pivotal, that is, without his or her contribution, the group's performance will fall below the cutoff. The probability that the n^{th} member is pivotal is just the probability that the rest of the organization generates an output of $C_n - 1$. Because the binomial distribution flattens out for large numbers of trials, this probability falls arbitrarily close to zero for sufficiently large n . Hence the likeli-

hood that any individual will be pivotal becomes vanishingly small in very large groups, and the decentralized regime can no longer sustain cooperation.¹⁸

Note that this upper-bound n^* on the feasible size of groups is quite distinct from the bound of n^* described in the previous section. The former arises from the probabilistic character of conditional cooperation under uncertainty. The latter, which simply expresses the relative benefits of defecting now versus the costs of foregoing future cooperation, holds under the deterministic conditions of perfect monitoring and has no probabilistic content. Indeed, under the parametric conditions of Table 1, n^* equals infinity—and hence is not a binding constraint—whereas n^* does limit the feasible size of groups.¹⁹

Federal Structures

In the previous section we have seen that in the presence of even small uncertainties or perceptual errors, both the purely decentralized and centralized solutions to the collective action problem may exhibit drawbacks. The decentralized structure based on trigger strategies collapses too often; the centralized regime may tolerate some free riders. When this happens, one naturally wishes to explore alternative institutional forms that might perform better. In particular, are there alternatives that are more stable than the trigger strategy regime, as well as less burdened by free riding than the centralized one?

The instability so characteristic of the trigger strategy structure is rather similar to the kinds of instability problems discussed by Herbert Simon (1969). Simon argued that the ubiquity of uncontrollable disturbances skews the evolution of organizational structures toward those that localize the effects of such shocks. He hypothesized that hierarchic forms—in the sense of a nested structure of units

within units—provide the desired type of protection. So we now consider a hierarchical or federated structure for the interest group. The group divides itself up into a number of local chapters; it also retains a central office. Instead of monitoring the performance of individuals, the central office monitors the collective performance of local chapters. To maintain comparability across organizational forms, we assume headquarters has the same monitoring resources as in the fully centralized expulsion regime. Because it is monitoring fewer units—chapters instead of individuals—the probability that any chapter will be observed is proportionately higher. For example, if the group has one hundred members, with $q = 0.01$ in the expulsion regime, then with 10 chapters, each one has a 0.1 chance of being observed. Thus we assume that in an n -sized group comprising m local chapters with $j = n/m$ members in each chapter, the central office audits the performance of each chapter with probability jq .²⁰

A number of variants of the federal structure are possible, depending on the nature of the collective good and the instruments of control available to the central office. We first describe a context where the collective good exhibits rival consumption or has the quality of a local public good. Thus if someone is suspended from the organization, that person cannot enjoy the collective benefit for the relevant time period. (Consider, for example, a disbarred lawyer, or a union local denied strike funds by the national organization.) In this setting we will analyze a federal structure that uses both decentralized and centralized strategies: the former at the local intrachapter level and the latter between headquarters and individual chapters. Regarding the latter, the central office audits the performance of any chapter of size j with probability jq . If in any period this is observed to fall below the threshold level bj , all members of that chapter are suspended from the

organization *in that period*. In such a period these members get a payoff of zero. If chapter performance is observed to be at the required level, each member receives the normal benefits of membership, b . In periods when a chapter's contribution is unobserved, the center treats those members exactly as it did the last time it monitored that chapter. Thus if at any date the chapter's performance is found wanting, members are suspended until the next time headquarters observes their collective performance and verifies that it satisfies the required level.

Within every chapter, members employ a decentralized trigger strategy. They start by cooperating—even the low-ability ones. But if at any date a chapter is found inadequate by headquarters and suspended, every member will shirk for the next T periods, thereafter returning to the strategy of conditional cooperation. Given the strategy of the central office, this chapter will remain suspended for the entire all-shirk phase because throughout this time it will contribute nothing. Even after the chapter's noncooperative phase ends after T periods and it resumes contributing at level bj , it may remain suspended if it so happens that headquarters does not monitor its performance. The next proposition shows that if each chapter is of a certain minimum size, these trigger strategies form an equilibrium pattern of behavior.

Proposition 7

Assume that the interest group is federated into m subgroups with $j = n/m$ members in each subgroup and that the collective performance of each subgroup is monitored with probability jq every period. Suppose that the central office uses the strategy of suspending all members of a subgroup in any period when its performance is observed to fall below bj , until the next time it discovers the subgroup's performance is equal to the

desired level b_j . In all other periods, each subgroup member obtains a benefit level b of the collective good. In this regime if each subgroup has a minimum size j given by the condition

$$j > \frac{1}{q} \left(\frac{1}{\delta} - 1 \right) \frac{c_1}{b - c_1} \quad (7)$$

and if the idiosyncratic uncertainty $(1 - \bar{p})$ is small, there exists an equilibrium where each member of the subgroup chooses the following trigger strategy: participate (i.e., $p = \bar{p}$) at date 1 and thereafter unless at date t the center suspends the subgroup for inadequate performance, in which case shirk for the next T periods. The expected payoff to each member from this equilibrium can be explicitly calculated.

In contrast to the purely centralized regime based on monitoring individuals, the federal regime is beset by no free riding. In this nested structure, all members participate in cooperative periods because a failure in individual performance causes the chapter's performance to fail, thus inviting punishment with probability jq . In contrast, an individual's failure in the centralized regime invites punishment with the lower probability q . Even though punishment in the federal regime is a temporary suspension rather than permanent expulsion, if jq is large enough the federal structure provides better participation incentives than the centralized regime. This translates to a condition that, given q , the size j of the subchapter must be large enough—which is expressed by (7).

While the federal structure's use of decentralized trigger strategies eliminates free riding, it does not create the degree of instability characteristic of a purely decentralized (unfederated) structure because in the federated structure the trigger strategies are only used locally, within chapters. This contains the instability arising from uncontrolled disturbances or

monitoring errors and does not threaten members of other chapters. Thus the overall organization is protected from local shocks. Given that decentralized strategies generate more instability, the larger the relevant group, it is clear that each chapter must not be too large if a federated structure is to outdo the unfederated one. This contrasts with the requirement of (7) that to prevent free riding each chapter not be too small.

Hence we must check the feasibility of federated structures. Are there chapters small enough so that the nested organization is significantly more stable than the purely decentralized structure but large enough to ensure there is less free riding than in the purely centralized system? In the example considered in the previous section, condition (7) requires each chapter to have at least 3 members. Even if we consider chapters with 10 members each, a typical member obtains expected payoff $(4,514.5 - 9.1c)$ from the federal structure that exceeds the payoff to high-ability members in the centralized regime. Thus, in this example, whenever the group is large enough for the centralized organization to yield higher payoffs than the purely decentralized regime, the federal structure dominates them both.²¹ However, this should not be taken to mean that some federated structure invariably dominates the two unfederated regimes. For instance, if monitoring in the centralized system is good enough, it may not be burdened by any free riding at all. In that case it could dominate any federated institution of the type discussed so far by reducing the risk of being suspended or expelled by the center. The point is that if the centralized structure is characterized by free riding (which it will with sufficient dispersion in private costs of participation), then a federated form can improve on it, as well as on the purely decentralized structure if the overall group is large.

So far we have discussed the context of

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a local public good that members can be temporarily suspended from enjoying. However, many interest groups such as lobbies typically cannot bar shirking members from enjoying the benefits of a piece of legislation they help pass. In such circumstances interest groups may still be able to punish members selectively if they have additional instruments of control like those Olson argued were important in solving collective action problems in large groups. A previous version of this paper describes variants of the federal structure that can be employed in this case (Bendor and Mookherjee 1985, prop. 7).

Finally, we wish to point out that other variants of the federal institution may be useful in different contexts. The structure described in proposition 7 uses centralized monitoring and punishments of subunits, along with decentralized strategies within chapters. It thus relies on centralized authority to enforce punishments. Alternatively, Russell Hardin has suggested that large groups without any internal authority structure at all may be able to resolve collective action dilemmas by using a federated structure (1982, 184). He argues that despite the absence of a central authority, subunits may be able to regulate themselves via decentralized strategies. Such self-regulation could arise if there were multiple activities going on simultaneously in each chapter. For instance, the existence of subunits inevitably promotes social interaction among members so that task-oriented and social activities coexist. Hardin argues this enables local chapters to regulate themselves for two reasons. First, multiple activities provide everyone with multiple opportunities for judging the character and preferences of peers. The significance of maintaining a good reputation is thus enhanced. If a colleague plays dirty in one setting, one might suspect that person of free riding in another. Second, multiple games imply multiple instruments of control. Even if the group's

instrumental actions are aimed at securing a pure public good that cannot be denied to free riders, the social activities provide potential sanctions that can be adjusted to a fine tolerance.²²

We now formalize the second idea—the presence of multiple instruments created by multiple overlapping activities—to show how a federated structure relying on decentralized strategies alone can overcome collective-action problems. Assume that members interact (1) globally by contributing to the organization as a whole and (2) locally by participating in social activities with people in the same chapter. Both global and local games are subject to a prisoner's dilemma.²³ A member k of subgroup g must choose p_k and e_k , contributions to global and local activities respectively. As before, the global collective good is subject to monitoring uncertainties. The local activities are, however, perfectly observed—a simplifying assumption that is quite inessential. Thus while p_k has to be chosen between 0 and \bar{p} , e_k can be chosen between 0 and 1. The utility of member k in any period is then

$$U^k = \frac{b}{n} \sum_{g=1}^m \sum_{i \in I_g} p_i + \frac{\beta}{j} \sum_{i \in I_g} e_i - c_k(p_k + e_k), \quad (8)$$

where

$$b > c_k > \frac{b}{n} \text{ and } \beta > c_k > \frac{\beta}{j}, \quad (9)$$

and I_g denotes the set of members of chapter g .

Proposition 8

Assume (8) and (9) hold and also that

$$\delta(\beta - c_1) > \left(c_1 - \frac{b}{n}\right) + \left(c_1 - \frac{\beta}{j}\right) \quad (10)$$

Then if $(1 - \bar{p})$ is small, there exists an

equilibrium with the following properties: 1) all members always choose global participation \bar{p} ; and 2) local participation is maximal ($e_i = 1$) unless at the previous date either the chapter's collective contribution to the global organization or the scale of local social activity fell below b_j , in which case $e_i = 0$.

Here inadequate subgroup performance is discouraged by the threat of defection in the local game. Members never defect on their contributions to the global organization, thus stabilizing the latter. Condition (10) describes what is required for this to be an equilibrium: the short-term benefits from shirking in both global and local activities must be outweighed by the prospect of a collapse of local cooperation in the next period. The existence of overlapping local benefits and the prospect of a breakdown of cooperation in these games provide an alternative to the selective punishments administered by a center in the previous regime of proposition 7.²⁴

However, we must emphasize that this method of providing incentives to contribute to the global cause is vulnerable to local collusion. A chapter can profitably free ride on other subgroups' contributions to the global organization, simultaneously cooperating on local activities. Such forms of local collusion are especially likely in large organizations where local units are alienated from the rest.²⁵ While the foregoing analysis upholds Hardin's point that it is possible for very large groups to organize collective action in a completely decentralized fashion in the presence of overlapping local interactions, the prospect of local collusion may also explain why most examples of such forms of collective activity are of mass movements wherein individuals strongly identify with the main cause, thus reducing the chance of local collusion.

Naturally, other chapters can try to deter collusion in a particular chapter. But they must tread carefully. If they threat-

ened to withdraw their own contributions, then in effect they would have returned to the completely decentralized regime, whose drawbacks we know well. Alternatively, the other chapters could selectively sanction the deviant subgroup, thereby returning to the dual institution of proposition 7. We believe that large groups that initially rely on completely decentralized, self-regulating subunits will either work their way toward dual regimes or will suffer pronounced instability as the subunits withdraw effort in retaliation for real or perceived shirking.

Conclusions and Implications

Since Hobbes, problems of collective action have fascinated and troubled political scientists. In the main, this interest has resulted in a concentration on two polar solutions: cooperation sustained by decentralized strategies of reciprocity (anarchy) and cooperation enforced by central authority (the state). These alternatives certainly merit our attention. They do not, however, exhaust the set of institutional forms that can ameliorate problems of collective action. We believe that this focusing on the polar pair of completely decentralized and completely centralized solutions has led us to overlook an important class of organizational forms: nested structures that combine strategies of conditional cooperation at local levels and strategies of selective incentives at the global level.

This class is not the pipe dream of institutional designers; it already exists. In fact, it is a robust empirical regularity. Several generations of researchers in industrial sociology and organizational behavior have concluded that every formal organization is laced with networks of informal exchange and reciprocity.²⁶ Even in the tightly controlled settings of prisons, decentralized strategies of

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conditional cooperation appear spontaneously.²⁷

Yet their scope is limited. Reciprocity strategies are generally confined to the small numbers case: a small work unit, a legislative faction, a union local. Typically, a formal structure involving centralized means of eliciting and enforcing cooperation links the small subunits. This, too, is a robust empirical pattern (Meyer 1972; Starbuck 1965).

Of course, local strategies do not always reinforce global ones. Often they conflict. Industrial sociology is filled with case studies of how work groups informally collude against the headquarters of a firm, and there are numerous accounts of similar "departmentalitis" in the Soviet bureaucracy (Granick 1959). Therefore, it would be patently false to claim that the mere existence of dual strategies indicates that a group has solved its problems of collective action. Instead, our point is that to the extent that a large organization has ameliorated these problems, it may well have done so via a nested structure in which formal centralized strategies and informal decentralized ones reinforce each other. Further, this nesting is particularly important when inferential problems make the decentralized strategies destabilizing.

Conversely, when such federated systems are infeasible, Olson's conclusions reappear in a dynamic and probabilistic form. In large nonfederated groups that rely on decentralized strategies, cooperation is fragile at best. Iterated encounters raise the possibility of cooperation; given imperfect observation, iteration also implies repeated breakdowns. And in very large groups decentralized cooperation cannot be sustained at all.

Empirically, of course, nested structures are the rule. Among interest groups, it is virtually impossible to find a single one of any substantial size that does not exhibit a "Chinese box" form. Trade associations are built out of firms, them-

selves hierarchic systems; the American Medical Association has well-organized state associations; the AFL-CIO is nested many layers deep, with member unions organized into locals.²⁸

Hierarchic structures are ubiquitous for many reasons. Clustering by subunits makes it easier for newcomers—especially new leaders—to understand an organization, thus easing problems of bounded rationality. The hierarchical decomposition of problems takes advantage of knowledge dispersed throughout an organization and economizes on communication and other information-processing tasks (Geanokopolos and Milgrom 1984; March and Simon 1958). It reduces transaction costs of intraorganizational bargaining, and, as we have emphasized here, it localizes the effects of exogenous shocks (Simon 1969).²⁹ The nested structure of large organizations may well be overdetermined. Regardless of the cause, large interest groups and other organizations will almost always have federal structures. The smallest subunits provide a natural breeding ground for decentralized conditional cooperation. This suggests that the real choice is not between anarchy and the state but between different forms of dual control.

Limitations

We see three significant limitations of our models. First, all of our results depend on the "shadow of the future" looming large enough. This has a subjective and an objective component. Subjectively, the members of a group must not discount the future too heavily, as we have often noted. Objectively, there must not be too much turnover in the organization. We have finessed the objective component by assuming that no one voluntarily leaves the group, no one dies, no one emigrates. How far cooperation will fall once these assumptions are relaxed is an interesting question. As is common in most repeated

play models with farsighted strategic actors, it seems likely that it will be hard to prevent members near retirement from free riding. However, since most interest groups are composed of several overlapping generations, free riding by senior members need not lead to organizational collapse. Indeed, a pattern of contributing a lot when young and free riding when old may be an implicit social contract that is a Nash equilibrium (Cremer 1986).

Second, in the expulsion and federal regimes we have not explicitly modelled the behavior of the group's headquarters. The central office never shirks its duty; it always expels people when it discovers free riding. Moreover, headquarters never exceeds its duty either. Abuse of power never occurs; headquarters never manipulates the rest of the group for its own ends. Michels's "iron law of oligarchy" has been assumed away.

It would be interesting to relax these assumptions by modelling a repeated game between leaders and ordinary members. This game itself may be an iterated n -person prisoner's dilemma. Both sides might be better off abiding by the terms of a deal than if they both defect, but each side may do better still by defecting while the other side cooperates (e.g., the leaders by enjoying the perquisites of office while neglecting their duties).

The third limitation is perhaps the most important. Game-theoretical models such as ours can explain why certain patterns persist; they do not explain how they arise. The emergence of cooperation is a hard problem—one that may require other methods of analysis.³⁰ Of course, when there is only one Nash equilibrium in a repeated game, one might predict that will be the limiting outcome of many of the adjustment paths. To use the vivid language of Markov processes, such an outcome is an absorbing state. Once the system gets there, it stays there. And, in all likelihood, it will get there eventually.

However, in most repeated games there

are multiple equilibria, often an embarrassment of riches. This is so for the iterated prisoner's dilemma. Consequently, an interest group may grope its way toward a cooperative equilibrium, sustained by intricate institutional forms wherein global inducements complement local reciprocity, or it may grope toward the "social trap" (Cross and Guyer 1980) of minimal cooperation, in which everyone could be better off but in which the prevailing codes rob every individual of reasons to exert any effort. Which history unfolds may depend on idiosyncratic events, for example, an unlucky early defeat that creates widespread suspicion of shirking. Consequently, explaining why patterns persist may be more amenable to theoretical analysis than may explaining how they arose.³¹

We suspect, however, that this issue of dynamics strengthens one of our conclusions: the purely decentralized regime is even less viable than we have suggested. A group that is trying to move toward a common strategy of conditional cooperation faces formidable coordination problems. During the disequilibrium of trial-and-error adjustments, members will use different cutoff levels and punishment periods of varying length. The resulting potential for confusion, for misperceptions that slow down the convergence towards an equilibrium institution, would be enormous. Indeed, an explicit examination of evolutionary paths may well reveal that nested structures, building as they do on "stable subassemblies" (Simon 1969), are even more important than our equilibrium analysis indicates.

Appendix

Proof of Proposition 1

To prove the first part, we show that if conditions (3) and (4) in the text are satisfied, then at any stage of the game the postulated trigger strategy is an optimal

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response for any given member i to the use of the same strategy by all other players. Within any punishment phase, choice of contribution affects the decision of no other player in the future, so it is optimal for i to choose $p_i = 0$. Outside punishment phases, the most i can obtain for the remainder of the game at any given date t by defecting is

$$b\left(1 - \frac{1}{n}\right) + \delta^{T+1} \frac{(b - c)}{1 - \delta} \quad (\text{A1})$$

since the remaining $(n - 1)$ members are choosing to cooperate at t , but cooperation will break down in periods $(t + 1)$ to $(t + T)$ and then start again from $(t + T + 1)$ onwards. By not defecting at t , i 's payoff from the remainder of the game is

$$\frac{(b - c)}{(1 - \delta)}, \quad (\text{A2})$$

which is greater than (A1) by virtue of (3). That there exists an integer T satisfying (3) is guaranteed by (4).

To prove the second part of the proposition, note that the worst equilibrium from any member's point of view is the one where everyone defects perpetually as this gives each member a min/max payoff of zero. Hence, using results in Abreu (1982), perpetual cooperation can be supported as an equilibrium outcome if and only if unilateral shirking by single members is not worthwhile to them, provided such deviations are followed by the group reverting to the all-defect equilibrium. Equation (4) is a necessary and sufficient condition for this and identifies the critical group size relative to δ :

$$n^*(\delta) = \frac{b - b\delta}{c - b\delta} \text{ if } c > b\delta,$$

and ∞ otherwise.

Q.E.D.

Proof of Proposition 2

Consider a low-ability member who obtains a discounted payoff

$$V_1 = \frac{b(1 - f)}{1 - \delta(1 - q)}$$

in the postulated equilibrium. Given the strategies of the other members, a unilateral switch by this member to a cooperative action in any period (followed by reversion to the postulated strategy) will lead to a discounted payoff of $\{b(1 - f + 1/n) - c_1 + \delta V_1\}$ instead of V_1 , and the condition

$$\begin{aligned} c_1 &> \frac{b}{n} \left[\frac{1 - \delta + q\delta + q\delta n(1 - f)}{1 - \delta + q\delta} \right] \\ &\equiv \frac{b}{n} L_1 \end{aligned}$$

implies this will be unprofitable. Since in discounted dynamic programming strategies that are unimprovable in one step are optimal (Whittle 1982), this shows that low-ability members are playing an optimal strategy. Similarly a high-ability member is choosing an optimal response, as such a member obtains discounted payoff

$$V_2 = \frac{b(1 - f) - c_2}{1 - \delta}$$

in this equilibrium, and this exceeds what he would obtain by shirking once, that is, $b(1 - f - 1/n) + (1 - q)\delta V_2$, if

$$\begin{aligned} c_2 &< \frac{b}{n} \left[\frac{1 - \delta + q\delta n(1 - f)}{1 - \delta + q\delta} \right] \\ &\equiv \frac{b}{n} L_2 \end{aligned}$$

Q.E.D.

Proof of Proposition 3

As before, it suffices to show that in any normal period it is optimal for any member i to choose \bar{p} , given the strategies of other members. The stationary policy p_i yields i an ex ante utility of

$$V_i(p_i)$$

$$= \frac{b(1 - 1/n)\bar{p} + (b/n - c_i)p_i}{1 - \delta p_i \bar{p}^{n-1} - (1 - p_i \bar{p}^{n-1})\delta^{T+1}}$$

since the probability of triggering off a defection phase is $(1 - p_i \bar{p}^{n-1})$. Let $S(p_i, \bar{p}, T)$ denote the derivative of V_i . Then as \bar{p} goes to 1 and T to ∞ , S converges pointwise to the function

$$\frac{\delta b(1 - 1/n) + (b/n - c_i)}{(1 - \delta p_i)^2}$$

which is positive for all p_i , by virtue of (4). Since the convergence is also uniform, there exists $\epsilon > 0$ and integer \bar{T} such that whenever $0 < 1 - \bar{p} < \epsilon$ and $T > \bar{T}$, \bar{p} maximizes V_i . Q.E.D.

Proof of Proposition 4

Given a cutoff level C and length T of noncooperative phases, the value function for member i in a cooperative period—given that all other members are pursuing the trigger strategy corresponding to (C, T) —is

$$\begin{aligned} V_i(p_i; C, T) = & b\left(1 - \frac{1}{n}\right)\bar{p} \\ & + \left(\frac{b}{n} - c_i\right)p_i \\ & + \delta \{S_i(p_i, C)V_i(\bar{p}; C, T) \\ & + [1 - S_i(p_i, C)]\delta^T V_i(\bar{p}; C, T)\} \end{aligned}$$

where $S_i(p_i, C)$ is the probability that collective output of the group—when $(n - 1)$ members are choosing \bar{p} , and the remaining member is choosing p_i —will achieve at least the cutoff level C . Clearly,

$$\begin{aligned} S_i(p_i, C) = & \text{Prob}[Bi(n - 1, \bar{p}) \geq C] \\ & + p_i \text{Prob}[Bi(n - 1, \bar{p}) = C - 1] \end{aligned}$$

if $Bi(n, \bar{p})$ denotes a binomial random variable corresponding to n trials with success probability \bar{p} , and

$$V_i(\bar{p}; C, T)$$

$$= \frac{(b - c_i)\bar{p}}{1 - \delta^{T+1} - \delta(1 - \delta^T)\text{Prob}[Bi(n, \bar{p}) \geq C]}$$

Hence for (C, T) to provide adequate effort incentives, it is necessary that $V_i(p_i; C, T)$ is increasing in p_i , that is (C, T) must satisfy the condition

$$\left(\frac{b}{n} - c_i\right) + \delta(1 - \delta^T)V_i(\bar{p}; C, T)$$

$$\cdot \text{Prob}[Bi(n - 1, \bar{p}) = C - 1] > 0,$$

and any such "feasible" pair gives rise to expected payoff $V_i(\bar{p}; C, T)$ for member with participation cost c_i . Part (a) of the result follows from the fact that $V_i(\bar{p}; C, T)$ is always decreasing in C , while $\text{Prob}[Bi(n - 1, \bar{p}) = C - 1]$ is decreasing in C as long as $C > np + 1$. Part (b) of the result follows from the fact that $\delta < 1$ implies that $(1 - \delta^T)V_i(\bar{p}; C, T)$ is increasing in T , while $V_i(\bar{p}; C, T)$ is decreasing in T . Q.E.D.

Proposition 5: Payoff Calculations and Proof

In this equilibrium a high-ability player attains ex ante expected utility (at the initial date) of

$$\begin{aligned} & \frac{(b - c_i)\bar{p}}{1 - \delta[1 - q(1 - \bar{p})]} - \frac{b\bar{p}f}{1 - \delta[1 - q(1 - \bar{p})]} \\ & \left[\frac{1 - \bar{p}}{1 - fp} + \frac{\bar{p}(1 - f)\{1 - \delta[1 - q(1 - \bar{p})]\}}{(1 - fp)\{1 - d\delta[1 - q(1 - \bar{p})]\}} \right] \end{aligned} \quad (A3)$$

where

$$d \equiv 1 - q(1 - fp).$$

At date 1 the expected number of low-ability members in the group at the end of date t is

$$E(m_t) = \frac{nf}{(1 - d)}$$

$$\{(1 - \bar{p})q - dt + d^t[1 - q(1 - \bar{p})]\} \quad (A4)$$

Proof. That any low-ability member will choose $p_i = 0$ always follows from an

argument similar to that used in proposition 2. For a high-ability member, it suffices to guarantee that \bar{p} close enough to 1 implies that the expected number of low-ability members at any date and following any history does not exceed its value at date 1, that is, $n(1 - f)$. This is shown in Mookherjee (1984). So we simply need to verify (A3) and (A4).

Given m_{t-1} low-ability members at the end of $(t-1)$, that is, $(n - m_{t-1})$ high-ability members at the beginning of t , the expected number of low-ability members at the end of t is

$$\begin{aligned} E(m_t | m_{t-1}) &= m_{t-1} - qm_{t-1} \\ &+ qfm_{t-1} + (n - m_{t-1})(1 - \bar{p})qf \\ &= dm_{t-1} + n(1 - \bar{p})qf, \end{aligned} \quad (\text{A5})$$

since the expected number of low-ability members expelled at t is qm_{t-1} and of high-ability members is $(n - m_{t-1})(1 - \bar{p})q$, while fraction f of all incomers are low-ability. (A5) implies that

$$\begin{aligned} E(m_t | m_{t-k}) &= d^k m_{t-k} \\ &+ n(1 - \bar{p})qf(1 + d + \dots + d^{k-1}) \end{aligned}$$

so

$$\begin{aligned} E(m_t) &= E(m_t | m_0 = nf) \\ &= \frac{nf}{1 - d} \{(1 - \bar{p})q - d\}_{t+1} \\ &+ d^t [1 - q(1 - \bar{p})]\}, \end{aligned}$$

which is (A4). The expected utility for a high-ability player at date t when viewed from date 1 is

$$\frac{b}{n}[n - E(m_{t-1}) - c_1] \bar{p},$$

so the long-run expected utility is

$$\begin{aligned} &\sum_{t=1}^{\infty} \delta^{t-1} \{1 - q(1 - \bar{p})\}^{t-1} \\ &\cdot \left\{ \frac{b}{n}[n - E(m_{t-1}) - c_1] \right\} \bar{p} \end{aligned} \quad (\text{A6})$$

since the effective discount must incorporate the probability $q(1 - \bar{p})$ of a high-ability player's being expelled at any date. Using (A4) reduces (A6) to (A3). Q.E.D.

Proof of Proposition 6

Using the result in part (b) of proposition 4, it suffices to show that for given \bar{p} there does not exist in sufficiently large groups any cutoff level that provides members with effort incentives (in normal periods) in conjunction with infinitely long punishment phases. It is easily verified that low-ability players will cooperate in normal periods, given any cutoff C and length of punishment phases $T = \infty$, if and only if

$$\text{Prob}[\text{Bi}(n - 1, \bar{p}) = C - 1]$$

$$> \frac{c_1 - b/n}{\delta}$$

Since the binomial distribution has the property that for given \bar{p} , the modal probability is of order $1/\sqrt{n}$ for large n , it follows for sufficiently large n that

$$\text{Prob}[\text{Bi}(n - 1, \bar{p}) = C - 1]$$

$$< \frac{c_1 - b}{\delta} \text{ for all possible values of } C,$$

which establishes the result. Q.E.D.

Proposition 7 (Dual Regime): Payoff Calculations and Proof

In this equilibrium, member i obtains ex ante expected utility

$$\frac{\{b(1 - jq(1 - \bar{p}^j)) - c_i\} \bar{p} + \delta^{T+1} D_1}{1 - \delta \{1 - jq(1 - \bar{p}^j)\} - \delta^{T+2} D_2} \quad (\text{A7})$$

where

$$\begin{aligned} D_1 &\equiv jq(1 - \bar{p}^j) \\ &\frac{(bjq\bar{p}^j - c_i) \bar{p}}{1 - \delta(1 - jq) - \delta^{T+1} jq(1 - \bar{p}^j)}, \end{aligned}$$

$$D_2 \equiv \frac{j^2 q^2 (1 - \bar{p}^j) \bar{p}^j}{1 - \delta(1 - jq) - \delta^{T+1} jq(1 - \bar{p}^j)}$$

Proof. Define an *abnormal* period to be any one of the T periods following an observation that the subgroup's performance was less than b_j . Then future strategies of all members are unaffected by performance in such periods, making it optimal for each to choose $p_i = 0$ non-cooperatively.

Define a *normal* period t as one where at $(t-1)$ the subgroup was not punished by the center (i.e., when performance was last monitored it was b_j , or it was never monitored before). Finally, define a *seminormal* period as one following a sequence of T abnormal periods since completion of which the subgroup has not been monitored. Clearly, seminormal periods arise with positive probability if $jq < 1$ and T is finite. In such periods the center will end the punishment only if performance is monitored and found adequate. Seminormal differ from normal periods in two ways. If performance is unobserved (a) the subgroup is punished that period and (b) the following period will be seminormal rather than normal. Nevertheless we show below that when (7) holds, if \bar{p} is close to 1 and T is large enough, then in both normal and seminormal periods every member will chose \bar{p} , given that all others are doing likewise.

Let V_t^N and V_t^S denote the expected utility of i for the remainder of the game from t onwards if it happens to be normal and seminormal respectively. Then

$$\begin{aligned} V_t^S &= p_s^i [bjq\bar{p}^{j-1} - c_i] \\ &+ \delta \{(1 - jq)V_{t+1}^S + jqp\bar{p}^{j-1}V_{t+1}^N \\ &+ jq(1 - p_s^i\bar{p}^{j-1})\delta^T V_{t+T+1}^S\}. \end{aligned} \quad (A8)$$

$$\begin{aligned} V_t^N &= [jqp\bar{p}^{j-1} + (1 - jq)]b\bar{p} - c_N p_N^i \\ &+ \delta \{(jqp\bar{p}^{j-1} + 1 - jq)V_{t+1}^N \\ &+ jq(1 - p_N^i\bar{p}^{j-1})\delta^T V_{t+T+1}^S\} \end{aligned} \quad (A9)$$

where p_s^i and p_N^i are strategies chosen at t if it is seminormal and normal respectively. It suffices to show that postulated strategy $p_s^i = p_N^i = \bar{p}$ is *unimprovable*, for \bar{p} close to 1 and T to ∞ (see Whittle [1982, chap. 28]). Under the postulated strategy V_t^N reduces to (A7) and V_t^S reduces to

$$V_t^S = \frac{(b\bar{p}^j jq - c_i) \bar{p} + \delta\bar{p}^j jq D_3}{1 - \delta[1 - jq[1 - \delta^T(1 - \bar{p}^j)]] - \delta^{T+2} D_4} \quad (A10)$$

where

$$\begin{aligned} D_3 &\equiv \frac{b\bar{p}(1 - jq) + \bar{p}(bjq\bar{p}^j - c_i)}{1 - \delta[1 - jq(1 - \bar{p}^j)]}, \\ D_4 &\equiv \frac{j^2 q^2 \bar{p}^j (1 - \bar{p}^j)}{1 - \delta[1 - jq(1 - \bar{p}^j)]}. \end{aligned}$$

Letting \hat{V}_t^S and \hat{V}_t^N denote V_t^S and V_t^N obtained by replacing V_{t+1}^S , V_{t+1}^N and V_{t+T+1}^S by (A10), (A7), and (A10) respectively in (A8) and (A9), it can be checked that

$$\frac{\partial \hat{V}_t^S}{\partial p_s^i} = \frac{\partial \hat{V}_t^N}{\partial p_N^i},$$

and

$$\lim_{\substack{\bar{p} \rightarrow 1 \\ T \rightarrow \infty}} \left(\frac{\partial \hat{V}_t^S}{\partial p_s^i} \right) = bjq - c_i$$

$$+ \frac{\delta jq}{1 - \delta} [(bjq - c_i) + b(1 - jq)],$$

which is positive, given (7). Hence it is optimal to choose p_s^i and p_N^i equal to \bar{p}_N^i equal to \bar{p} , given that this strategy is to be followed in the future, if \bar{p} is sufficiently close to 1 and T to ∞ .

Q.E.D.

Proof of Proposition 8 (Completely Decentralized Federal Groups)

Define a period to be *normal* (N) if both local collective activity and subgroup contribution to the global organization

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were maximal in the previous period. Otherwise call a period *abnormal* (A). It is clear that given that all other members are following the postulated strategy, it is optimal for member i to choose $e_i = 0$ in an abnormal period.

Let (p_N, e_N) denote contributions in normal periods and p_A the contribution to the global organization in an abnormal period. Then

$$\begin{aligned} V^N(p_N, e_N) &= \frac{b}{n}[(n-1)\bar{p} + p_N] \\ &+ \frac{\beta}{j}(j-1 + e_N) - c_i(e_N + p_N) \\ &+ \delta\bar{p}^{j-1}p_NV^N(p_N, e_N) \\ &+ \delta(1 - p_N\bar{p}^{j-1})V^A(p_A), \text{ if } e_N = 1; \\ &= \frac{b}{n}[(n-1)\bar{p} + p_N] \\ &+ \frac{\beta}{j}(j-1 + e_N) - c_i(e_N + p_N) \\ &+ \delta V^A(p_A), \text{ if } e_N < 1. \end{aligned} \quad (\text{A11})$$

$$\begin{aligned} V^A(p_A) &= \frac{b}{n}[(n-1)\bar{p} + p_A] - c_ip_A \\ &+ \delta\bar{p}^{j-1}p_AV^N(p_N, e_N) \\ &+ \delta(1 - p_A\bar{p}^{j-1})V^A(p_A). \end{aligned} \quad (\text{A12})$$

It is obvious from (A11) that e_N is either 0 or 1. Also if e_N is 0 then the best p_N is also 0. Let \bar{V}^N and \bar{V}^A denote (A11) and (A12) when their right-hand sides have $V^N(p_N, e_N)$ and $V^A(p_A)$ replaced by $V^N(\bar{p}, 1)$ and $V^A(\bar{p})$ respectively. If then suffices to show that for \bar{p} sufficiently close to 1, the following hold:

- (a) $p_N = \bar{p}$ maximizes $\bar{V}^N(p_N, 1)$.
- (b) $\bar{V}^N(\bar{p}, 1) > \bar{V}^N(0, 0)$.
- (c) $p_A = \bar{p}$ maximizes $\bar{V}^A(p_A)$.

To prove (a) and (c), note that

$$\frac{\partial \bar{V}^A(p)}{\partial p_A} = \frac{\partial \bar{V}^N(p, 1)}{\partial p_N} = \left(\frac{b}{n} - c_i \right)$$

$$+ \delta\bar{p}^{j-1}[V^N(\bar{p}, 1) - V^A(\bar{p})],$$

which converges to

$$\left(\frac{b}{n} - c_i \right) + \delta(\beta - c_i) \quad (\text{A13})$$

as \bar{p} converges to 1. (10) implies (A13) is positive. To prove (b), note that

$$\begin{aligned} \lim_{\bar{p} \rightarrow 1} [\bar{V}^N(\bar{p}, 1) - \bar{V}^N(0, 0)] &= \delta(\beta - c_i) \\ &+ \left(\frac{b}{n} - c_i \right) + \left(\frac{\beta}{j} - c_i \right), \end{aligned}$$

which is also positive by virtue of (10). Q.E.D.

Notes

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1. Of course there are collective action problems where the situation is less stark, e.g., it may be worthwhile to contribute as long as a certain fraction of the group is doing so as well (Schelling 1978) or it may be worthwhile for a particularly large member to participate by himself if there are payoff asymmetries (Olson 1965, 45).

2. For some corroborating evidence in the case of the two-person game, see Murnighan and Roth (1983).

3. To be sure, dyadic ties may form very large networks: A deals with B , who reciprocates with C and so on. But in such circumstances each tie of reciprocity binds only two people.

4. In fact, it is a bit stronger than Nash, for the strategies ensure that the threat of retaliation is credible, whereas some Nash equilibria require non-credible threats. Equilibria supported by credible threats are known as perfect equilibria (Selten 1975). All the equilibria presented in this paper can be shown to be perfect in Selten's sense.

5. As noted earlier, discounting the future completely makes the game in effect a one-shot affair, and noncooperation becomes the only equilibrium strategy.

6. Specifically,

$$n^* = \frac{1 - \delta}{c/b - \delta} \text{ provided } \frac{c}{b} > \delta.$$

It is intuitively reasonable that the critical size increases in the benefit b and decreases in the cost c .

7. This idea can be generalized to cover n different levels of private costs (Mookherjee 1984), but because the qualitative insight is the same, we use the example of two types of members. For convenience we refer to the two types as low- and high-ability, but there are many other reasons why the costs of cooperation vary across members.

8. It may be possible to monitor and impose punishments on a decentralized basis (Axelrod 1986), particularly in small homogeneous groups. Because we are interested in cooperation in large groups, we assume that this authority has been delegated to a central office.

9. This assumption is made to ensure that the size of the group does not automatically dwindle as some members are expelled. It will be inessential in the present case where monitoring is perfect. It is, however, convenient in the context of imperfect monitoring, a case we shall soon consider. There it is also relatively inessential for large groups, provided the fraction of shirkers in the group is not large. At any rate it is a realistic assumption for professional groups that can adjust entrance requirements appropriately to replace expelled members. For other interest groups the expulsion of some members should not affect group size materially in the long run, as new generations of young members join the group over time.

10. Alternatively, this uncertainty may be interpreted as the possibility that decision makers are not always perfectly rational—a fraction of the time each member may make the mistake of choosing a suboptimal alternative. In the game-theoretical context this significant departure from the classical assumption of complete rationality was pioneered by Selten (1975).

11. A previous version of this paper (Bendor and Mookherjee 1985) also considers the case of perfectly correlated uncertainty. Our basic argument will, however, extend to imperfect correlation insofar as one can view such a case as a mixture of perfectly correlated and independent uncertainties.

12. This assumption can also be weakened, i.e., free riding could with a small probability yield a positive contribution. Because this modification complicates the calculations while leaving our essential results unchanged, we use the simpler assumption noted in the text.

13. We are excluding from consideration trigger strategy equilibria that induce only high-ability members to work in cooperative periods, with low-ability members shirking always. Such an arrangement would be dominated by the centralized struc-

ture since it would have the same amount of free riding and would be less stable as well.

14. The chance of breakdown is given by the probability that a binomial random variable with n trials and \bar{p} probability of success falls below the cutoff C_n .

15. Note that this uncertainty may also capture mistakes that the monitor can make in assessing the contribution of any member, so our model does not depend on the assumption of a perfect monitor.

16. To calculate the results of Table 1, we set $b = 500$, $c_1 = 100$, $c_2 = 20$, and $\delta = 0.9$. We then used a brute force algorithm to find the values of C_n that would support an equilibrium for $T_n = \infty$. Next, using the second part of proposition 4, we searched for the smallest T_n that would sustain a given C_n . Finally, for a given n , we selected the $\{C_n, T_n\}$ pair that maximized expected payoffs to the members. Though proposition 4 does not include this property, it turned out that the lowest sustainable C_n was always the best.

17. Note that every time the shortfall increases, the punishment period T_n rises as well. This more severe threat compensates for the less demanding performance standard in order to provide the required participation incentives.

18. This proposition can easily be extended to the case of continuous output. For example, suppose that an individual's contribution is a function of effort plus a normally distributed disturbance of mean zero. As before, disturbances are independent over time and across members. Since collective output is the sum of individual contributions, it, too, will be normally distributed, with a variance equal to the sum of the variances of the individual perturbations. Hence once again the output distribution will flatten out in large groups, and for sufficiently large n , the chance that any member will be pivotal will fall arbitrarily close to zero. Thus, although in our model collective performance is discrete, the validity of proposition 6 does not rest on this assumption.

19. At first glance proposition 6 may seem to contradict proposition 3's claim about the existence of a cooperative equilibrium in a trigger strategy regime. However, proposition 3 asserted that for a fixed n , a trigger strategy equilibrium existed provided the disturbance $(1 - \bar{p})$ was sufficiently small. Proposition 6, on the other hand, holds the disturbance constant and varies n .

20. This assumption of linear increases in the monitoring probability is convenient for computing the numerical examples. The qualitative pattern of results, however, really depends on the much weaker assumption that the probability a chapter will be monitored increases monotonically with its size.

21. It should be emphasized that expected payoffs in both the centralized regime and the federated regime with fixed chapter size are independent of the

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size of the overall organization. This is in contrast with the unfederated decentralized structure, which tends to perform more poorly for larger organizations.

22. Of course industrial sociologists have long recognized that informal social pressure is often brought to bear on those who do not pull their weight in instrumental activities. Hardin's contribution is to set this in the context of the n -person repeated play of the prisoner's dilemma.

23. The assumption that the local game is a prisoner's dilemma may not be satisfied if the interactions are purely social in nature. Yet the local games may still help regulate the global one. For example, suppose each member of a chapter faces a binary choice of being friendly $\{f\}$ or unfriendly $\{u\}$ to other members of the chapter. Because most people like to reciprocate social affect, there will be two Nash equilibria in the one-shot game, a benign one $\{f,f\}$ and an unpleasant one $\{u,u\}$. The existence of two equilibria indicates this is not a prisoner's dilemma; nevertheless, these equilibria provide opportunities for punishing peers who shirk in the global game. Suppose $n-1$ players use the following twin strategies: (1) always contribute to the global cause; (2) locally, be friendly to everyone who works hard for the global benefit and ostracize free riders for T periods. (See Dawes, McTavish, and Shaklee [1977, 7] for some fascinating data on how experimental subjects spontaneously create such strategies.) For sufficiently big T , the n^{th} player's best response may be to contribute to the global good and to uphold the local social code. Of course, as usual this would be only one perfect equilibrium. There probably are collusive equilibria wherein the local social code involves defecting from the global concern and punishing nondefection.

24. One could generalize proposition 8 by allowing the monitoring of the local activities to be imperfect and by permitting the costs of local participation to differ from those of contributions to the global good, as in (8). The equilibrium would remain essentially unchanged.

25. For some evidence on how easily subgroups develop out-group bias, see Kramer and Brewer (1984) and the references cited therein.

26. The list of all relevant citations would be extremely long. See, for example, Crozier (1964); Dalton (1959); Gouldner (1954, 1960); Hecko (1977); Matthews (1960); Miller and Form (1964, 280-81); Moore (1951, 282-90); Roethlisberger and Dickson (1939, 525); Sapolsky (1972, 122); Selznick (1949, 251-52); Shils and Janowitz (1948); Simon, Smithburg, and Thompson (1950, 87); Stouffer, Lumsdaine, Lumsdaine, Williams, Jr., Smith, Janis, Star, Cottrell, Jr. (1949); Zaleznik (1956, 1965); and so on.

27. This regularity is so pervasive that the ambitions of early organizational reformers such as Frederick Taylor to design organizations from top to bottom, to create systems unmarked by any infor-

mal structure, are now recognized as utterly utopian.

28. See Truman (1951, 112-29) for a description of the hierarchic structure of several important interest groups in the United States. More recently, see Knoke and Wood (1981, 37-41).

29. Note that in our model the benefits of localizing shocks can be obtained by creating only one level of subunits. Deeper nesting is unnecessary. Consequently, other considerations are required to explain the empirical prevalence of tall hierarchies.

30. Axelrod's simulations of the evolution of cooperation (1984) strongly suggest that computer simulations could be a very valuable tool for exploring dynamics.

31. Though their analyses were not game-theoretical, functionalists in sociology and anthropology have similarly argued that explaining the origins of institutions was a problem for historians rather than social scientists. See Stinchcombe (1968, 105) for a lucid explication of this view.

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INFLUENCES ON
EXECUTIVE AND
CONGRESSIONAL
BUDGETARY
PRIORITIES,
1955-1981

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A reduced form equation system is used to analyze the influence of economic, political, and institutional influences on the budgetary priorities of the executive branch and Congress during fiscal 1955-81. Three related issues are considered: the extent to which political and macroeconomic factors affect priorities; the degree of interdependence among the components of the federal budget and between spending and revenues; and differences between the executive branch and Congress with respect to these issues. Both types of interdependence are present within both executive branch and congressional budgeting, although this interdependence is stronger within the executive branch. The influence of economic conditions on budgetary outcomes is strong but varies considerably across spending categories. There is no evidence of a political business cycle. Political variables exert a modest influence on the budgetary outcomes examined; differences between Democratic and Republican budgetary policies, as well as differences in the budgetary priorities of different presidential administrations, are small by comparison with the differences between executive and congressional policies.

We analyze and compare the budgetary priorities of the U.S. Congress and executive branch during a substantial portion of the postwar period in order to better understand the effects of the condition of the economy on these priorities. We measure budgetary priorities as the shares of gross national product accounted for by the major components of federal spending—defense, domestic controllable, and domestic uncontrollable outlays—in the budgets emerging from the congressional and executive branch policy processes. We address three related issues: the extent to which political and macroeconomic factors influence budgetary priorities; the

extent of interdependence among budgetary components and between spending and revenue; and the differences in these traits between the executive and congressional budgetary processes.

An important feature of the analytic framework we develop is the integration of several groups of influences on federal spending that have been considered separately in previous work. While the annual federal budget represents an important statement of policy in many areas, extending from macroeconomic policy objectives to program-specific spending priorities, the empirical literature on budgeting has rarely considered the full set of potential influences on

budgetary outcomes. Such fragmentation means that budgetary models and theories, each of which incorporates a subset of the multiplicity of goals embedded in the annual federal budget, often yield inconsistent or mutually contradictory results—models of “political economics,” for example, reach conclusions that differ substantially from those of “incrementalists.”

Our model allows for an empirical assessment of the relative influence on budgetary outcomes of fiscal pressures, political variables, economic fluctuations, program-specific spending pressures, and spending pressures in other programs. The nature of tradeoffs among competing policy objectives is estimated statistically, rather than being assumed. Our analysis of budgetary outcomes is both disaggregated, separately examining different components of the budget, and comprehensive, since it includes most of the components of federal spending. The model also distinguishes between the budgetary decisions of the executive branch and Congress, in contrast to much of the previous literature.

Fragmentation in the Budgetary Literature

Inasmuch as the federal budget affects policy in many different areas, scholars have approached the analysis of budgets from varied intellectual perspectives. The resulting heterogeneity means that individual studies of budgetary processes and outcomes have been unable to build on prior studies, while factors deemed central in some parts of the literature have been ignored in other work. Tests among competing theories of budgeting have been hampered by this heterogeneity.

One major strand of the empirical literature has considered the sensitivity of macroeconomic policies to political influences.¹ While the literature in political

economics has been provocative, it has neglected several important aspects of the relationship between political variables and economic outcomes. Political economy studies have rarely specified the policy instruments employed to manipulate macroeconomic outcomes. Observed economic outcomes also have been widely assumed to be intended outcomes. Rather than proposing a detailed theory of the formulation of policy goals and the implementation of policies to realize these goals, scholars have endowed policy makers with remarkable powers of forecasting and fiscal “fine tuning.”²

In addition to ignoring the link between outcomes and intentions, much of the literature on political economics has aggregated executive and legislative policy processes. Policy outcomes have been described “as if” they resulted from the actions of a single actor, usually assumed in the U.S. context to be the president.³ Such aggregation of executive and congressional branches ignores the interplay of political and institutional factors that determine budgetary outcomes. A number of works, for example, Niskanen (1971, 1975), Fiorina (1978), Shepsle (1983), and Kamlet and Mowery (1983, 1985a),⁴ have suggested that the institutional structure of Congress limits interdependence in budgetary decisions and supports spending growth, implicitly contrasting congressional with executive budgetary behavior. Nonetheless, the empirical implications of these theoretical differences between executive and congressional budgetary incentives have rarely been examined.

If the political economy literature has neglected institutional structure and the policy process, the literature on the bureaucratic politics of budgeting, which includes most of the incrementalist interpretations of budgeting, has largely ignored traditional political influences. Within this literature, bureaucratic policy processes are governed by organizational

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procedures and decision rules and display little sensitivity to higher-level political influences (Padgett 1981).

The tradeoffs among competing policy objectives that are inherent in budgetary decision making also receive little attention in the empirical literature. Some studies, for example, restrict their focus to total spending or the size of the deficit (e.g., Hicks 1984; Lowery and Berry 1983; and Pack 1985). These studies cannot provide an explanation of spending patterns in specific program areas, however, nor are they able to incorporate the influence of programmatic priorities on total spending. The omission is serious, because macroeconomic stabilization is but one of a number of influences on budgetary outcomes. Multiple policy objectives are pursued simultaneously through the budget; commitments to individual programs, for example, may restrict the freedom of congressional or executive branch policy makers to pursue economic policy goals. The few works incorporating tradeoffs among competing policy objectives (e.g., Auten, Bozeman, and Cline 1984; Domke, Eichenberg, and Kelleher 1983; Fischer and Crecine 1981; and Russett 1982) typically have assumed the nature of such tradeoffs (e.g., assuming that the budgetary process is purely top-down, with fiscal policy goals dominating programmatic objectives), rather than estimating or testing such assumptions.

Other studies, including most incrementalist and "arms race" models of defense spending, have assumed that the budgets of individual programs are independent.⁵ This research presupposes the budgetary process to be purely bottom-up; each agency's budget is independent of spending pressures within other expenditure categories, fiscal policy priorities, or high-level political and economic considerations.

The analytic framework we propose acknowledges the wide range of objectives that may influence budgetary out-

comes and allows their relative importance to be determined empirically, rather than by assumption. This analysis extends Fischer and Kamlet (1984) in several ways. We examine congressional as well as executive budgeting, consider the importance of political and economic influences on budgetary outcomes in greater detail, and analyze uncontrollable and controllable domestic programs as separate budgetary components. The model provides a framework within which both the Competing Aspirations Levels Model (CALM) framework of Fischer and Kamlet (1984) and the broader literature on incrementalism and political economy can be situated.

Modelling Budgetary Tradeoffs

Interdependence in budgetary decision making refers to the extent to which budgetary outcomes in specific categories are sensitive to (1) pressures for growth in other programs and (2) fiscal policy pressures, as these latter influences are reflected in projected revenues and implied surplus or deficit levels. Consider the following general specification:

$$D = f_d(X_d, X_c, X_u, X_T, C, U, T) \quad (1a)$$

$$C = f_c(X_d, X_c, X_u, X_T, D, U, T) \quad (b)$$

$$U = f_u(X_d, X_c, X_u, X_T, D, C, T) \quad (c)$$

$$T = f_T(X_d, X_c, X_u, X_T, D, C, U) \quad (d)$$

$$D + C + U + I = T \quad (e)$$

Within this system, D represents defense outlays for a given year, C is the level of controllable nondefense outlays, U is the level of uncontrollable nondefense outlays (excluding interest payments), I is the level of interest payments, and T is the level of total outlays. The influences on the level of spending in each category include the amounts spent on other categories. Exogenous, program-specific influences are captured in the X_d , X_c , and X_u terms, respectively affecting defense, con-

trollable domestic, and uncontrollable spending. For example, last year's defense budget, the perceived level of the Soviet military threat, and the level of armed conflict all may affect defense priorities.

This model also incorporates macroeconomic objectives and influences on total spending. These influences are captured in the X_T term, which is a vector of variables measuring various dimensions of macroeconomic goals and the economic environment. The X_T term includes the level of projected revenues and the state of the economy, both of which are hypothesized to affect fiscal policy objectives and therefore the desired level of total outlays. X_T also includes political influences, such as the party composition of Congress and the political affiliation of the president. These aggregate political and economic influences may be included as well in one or more of the X_c , X_d , or X_u vectors, reflecting the fact that they directly affect spending in a specific category. Changes in the level of unemployment, for example, which are included in X_T , may affect uncontrollable spending directly, through the operation of such automatic stabilizers as unemployment payments and thus are also included in the X_u vector of variables.

Equations (1a)–(1d) are structural equations modelling the influence of various factors on defense outlays, controllable domestic outlays, uncontrollable outlays, and total outlays, respectively. Equation (1e) is simply an accounting identity, indicating that total expenditures are by definition the sum of the individual expenditure components. Any model of budgetary decisions must be consistent with this accounting constraint. Equation system (1) is a system of simultaneous, structural equations. Its reduced form representation is

$$D = h_d(X_d, X_c, X_u, X_T) \quad (2a)$$

$$C = h_c(X_d, X_c, X_u, X_T) \quad (b)$$

$$U = h_u(X_d, X_c, X_u, X_T) \quad (c)$$

$$T = h_T(X_d, X_c, X_u, X_T) \quad (d)$$

$$T = D + C + U + I \quad (e)$$

The system represented by equations (2a)–(2e) can incorporate both program-specific and aggregate influences on budgetary priorities while at the same time testing for interdependence. The hypothesis that spending levels within different budgetary categories are independent of one another assumes that equations (2a)–(2e) are identical to the following system:

$$D = g_d(X_d) \quad (3a)$$

$$U = g_u(X_u) \quad (b)$$

$$C = g_c(X_c) \quad (c)$$

$$T = D + U + C + I \quad (d)$$

In this formulation, defense budgets are formulated solely with reference to defense considerations; no influence is exerted by nondefense or fiscal priorities. Similarly, uncontrollable domestic and controllable domestic budgetary decisions are each made solely on the basis of parochial considerations and are not influenced by defense or fiscal priorities. Incrementalism is one example of such a model. In the pure incrementalist specification, g_d , g_u , and g_c are linear and X_d , X_c , and X_u are simply prior-year budget allowances. In any independent process model, fiscal policy priorities (e.g., revenues) exert no influence on budgetary allocations. Total expenditures are the sum of $g_d(X_d) + g_u(X_u) + g_c(X_c) + I$.

The hypothesis of budgetary interdependence may be tested by examining the role of nonparochial variables in equations (2a)–(2c). Nonparochial influences fall into two broad classes. One class is program-specific variables that affect spending in other programs. For example, interdependence between other budgetary categories and domestic controllable spending is present if X_d or X_u exerts a significant influence in equation (2b). Yet another form of budgetary inter-

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dependence hypothesizes that fiscal policy goals constrain total spending and thus affect spending on individual programs—indeed, this top-down fiscal constraint underpins the interdependence among spending categories. A number of researchers have argued on the basis of statistical and archival evidence that budgetary interdependence within the executive branch is enforced by the presence of a top-down spending constraint due to the salience of deficits or surpluses for economic stabilization (e.g., Fischer and Kamlet 1984; Mowery, Kamlet, and Crecine 1980). The effect of these non-parochial, aggregate influences on budgetary outcomes may be tested by examining the impact of the variables in the X_T vector.

In converting the functional restrictions of equation system (2) into a form amenable to statistical analysis, we assume that equation system (1)—and therefore equation system (2)—is linear. For the hypothesis of independence in budgetary decision making to be supported, the parameters in equation system (2) for variables representing parochial influences on spending in one category must be equal to zero in the equations for other budgetary categories.

In addition to examining interdependence in budgetary decision making, equation system (2) also allows for tests of a number of the hypotheses associated with the political economy literature. For instance, the influence on budgetary outcomes of political variables, such as the party of the president or the proximity of elections, can be tested by incorporating these variables in the X_T , X_u , X_c , and/or X_d vectors. The influence of these variables on budgetary outcomes can be assessed within a framework that is less likely to confound their influence with that of program-specific spending pressures or other influences on fiscal and budgetary policy (e.g., the state of the economy).

Data Sources, Variable Construction, and Specification Issues

Dependent Variables

The variables employed in our regression analyses are listed and defined in the Appendix. We examine three dependent variables: controllable domestic outlays, uncontrollable domestic outlays, and defense outlays. Defense outlays refer exclusively to outlays by the Department of Defense. Uncontrollables include the major domestic entitlement programs and exclude interest payments (which are responsive to factors other than those affecting the remaining uncontrollable programs). Controllable domestic outlays are computed as the difference between total spending and the sum of defense, uncontrollable, and interest outlays. Each dependent variable is computed for the president's budget, the budget following congressional action, and the final estimates of outlays after the end of the fiscal year. Outlays, rather than budget authority, were utilized because fiscal policy pressures (reflected in the budgetary surplus or deficit) typically operate through outlays.⁶ All expenditure and revenue variables are measured as a share of GNP.

Predetermined Variables

The model incorporates as predetermined variables both independent and lagged endogenous variables. The independent variables consist of projected revenues, inflation, unemployment, the level of armed conflict in which U.S. forces are engaged, the perceived Soviet threat, the number of northern House Democrats, and dummy variables indicating the proximity of national elections, the administration in office, and the party of the president. These variables are defined and discussed in the Appendix.

All of our specifications include lagged

endogenous variables. Most models of budgetary decision making ascribe great importance to prior-year expenditure allowances. Incrementalism, for instance, views prior-year budgetary allowances as a "base" and argues that policymakers focus on slight increments or decrements from this base. Similarly, prior-year budgetary allowances play a central role in models of spending pressure, where these growth pressures are assumed to be approximately proportionate to past expenditure levels. The use of lagged endogenous variables in our regressions raises problems of specification and estimation.⁷ Despite these difficulties, lagged endogenous variables are included in these equations because of the substantive importance of prior-year spending levels in most theories of budgetary decision making. In addition, the inclusion of lagged values of the dependent variables helps to control for lags in the adjustment of spending to exogenous influences.⁸

Specification Issues

Several specification issues arise in the empirical application of our analytic framework. One issue concerns the formulation of the variables measuring unemployment and the level of armed conflict—should they be represented in first difference form or as absolute magnitudes? Either formulation of these variables can be defended, depending on assumptions concerning the nature of policy maker perceptions and responses. The formulation of these explanatory variables is based on a priori hypotheses and empirical investigation and is discussed in greater detail in Kamlet and Mowery (1985b). The Appendix briefly describes the formulation of each variable for the benchmark specifications. In general, the estimated values of the parameters for other variables in our analysis were not significantly affected by the substitution of alternative formulations of these variables.

A second issue concerns the nature of the lags used for the U.S.-USSR tension, unemployment, war, and inflation variables. These lag structures are also based on a combination of theory and experimentation. The measure of Soviet military tensions employed in the analysis of spending for fiscal year t is that prevailing during calendar year $t-2$, that is, the perceived Soviet threat for the year during which the executive branch formulates fiscal year t 's budget. The choice of lags for the economic variables incorporated in the equation for the president's budget was straightforward because forecasts of these variables employed in the formulation of the president's budget generally were available. In the absence of comparable forecasts for congressional budgeting, we used for fiscal year t the inflation and unemployment rates for the second and third quarters of calendar year $t-1$. This formulation assumes a purely extrapolative model of inflationary and unemployment expectations, that is, we assumed that current trends are expected by congressional actors to continue into the future.⁹ Finally, we chose the level of armed conflict that prevails during calendar year $t-1$ as the basis for forming expectations about the level of armed conflict during fiscal year t . This assumption is discussed in greater detail in Fischer and Kamlet (1984). As was the case above, the coefficients for the other variables in our analysis were not substantively affected by the lags chosen for these variables.

Comparing Executive and Congressional Budgetary Behavior

In discussing our empirical results we take the figures proposed in the president's budget as a measure of presidential priorities and the congressional modifications of these figures as a measure of congressional budgetary priorities. This interpretation does not rule out presidential involvement in the congressional budget process so long as presidential policy

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goals remain consistent with those of the presidential budget document. If changes in presidential goals following the release of the budget occur in a nonrandom fashion across spending programs and years, however, this interpretation of the presidential and congressional budgets may be incorrect. For instance, misrepresentation or biased estimation of economic and demographic assumptions or other program data could allow for the release of a fiscally prudent president's budget that slides into a growing pool of red ink during congressional consideration. Attributing responsibility to Congress for all alterations in the president's spending requests under these circumstances clearly is unrealistic. Systematic bias in presidential short-run forecasting, however, seems modest (see Kamlet, Mowery, and Su 1986). Executive branch economic forecasts also must be consistent with revenue estimates in the president's budget. Much of the impact of biased forecasts is controlled for by the inclusion of contemporaneous revenue estimates as independent variables in both congressional and presidential regressions.

Apart from biases in forecasts and cost estimates, the president's budget may also reflect strategic positioning relative to anticipated congressional sentiment. However, the nature of any bias that this behavior imparts to the presidential budget is unclear (see Kiewiet and McCubbins 1985a, 1985b), nor are there strong reasons to believe that such biases will not offset one another across programs and years. Still, even if systematic gaming is present in the president's budget, the analyses of executive budgetary priorities can be interpreted as reflecting the budgetary priorities the executive branch wishes to convey to the public, rather than the president's true budgetary priorities. This interpretation of the president's budget as a political document is not new, but neither is it uninteresting.

Estimation Results

Tables 1-3 contain the results of specifications for domestic uncontrollable, defense, and domestic controllable spending for the executive branch and Congress jointly (Table 1), the president's budget (Table 2), and congressional budgetary actions on presidential budget requests (Table 3). These specifications test for the existence and extent of budgetary interdependence, the responsiveness of budgetary actions to economic fluctuations, and the impact of two political influences—the proximity of presidential and congressional elections and the party composition of Congress—on budgetary outcomes.

If budgetary interdependence is enforced by the presence of a top-down spending constraint, the regressions analyzing executive branch spending for the three budgetary components should yield a positive coefficient for the revenues term and negative coefficients for the variables associated with parochial spending pressures for other spending categories. Additionally, based on the atomistic nature of congressional decision making and the difficulties of implementing top-down control in a decentralized setting, we hypothesize that interdependence in executive branch budgetary outcomes is greater than in congressional budgetary outcomes.

Our priors are more diffuse concerning the economic and political variables. The economic variables measure the extent to which economic stabilization objectives influence the target spending total. They also capture the influence of macroeconomic conditions on program-specific spending outcomes. These top-down and bottom-up channels for spending pressure may be mutually reinforcing or offsetting. For instance, higher unemployment may increase the spending target total as a means of offsetting a recession. This top-down expansionary influence on uncon-

trollable spending will be reinforced by the operation of automatic stabilizers that affect uncontrollable spending, as expenditures on such programs as unemployment insurance grow. The impact of increased inflation, however, on top-down and program-specific spending pressures may be quite different. Increased inflation will reduce targets for total spending, as a countercyclical response. This decrease in the target total will exercise a depressive top-down influence on uncontrollable spending. Simultaneously, the impact of inflation on indexed uncontrollable programs will increase spending pressures associated with these programs.

Hypotheses concerning the influence of political variables center on the empirical validity of the "political business cycle," as well as the importance of political ideology in affecting both public spending and macroeconomic policy objectives. A number of authors, including Nordhaus (1975), MacRae (1977), Tufte (1978), Frey and Schneider (1978a, 1978b, 1979), and Hicks (1984), report empirical results that suggest that the economy is manipulated so as to reach a peak during election years.¹⁰ Because the disbursement of federal funds takes place primarily during the calendar year following that during which the budget is considered by Congress, the year immediately prior to the election year (i.e., the third year following a presidential election) should acquire a positive and significant coefficient if presidential elections induce greater spending. The findings of Hibbs (1977) and others that political ideology significantly influences spending behavior and macroeconomic policy leads to the hypothesis that Republican control of Congress or the White House is associated with lower spending, *ceteris paribus*, with the possible exception of defense outlays.

The results in Table 1 concern budgetary outcomes at the conclusion of congressional action, including prior-year spending levels among the predetermined

variables. These results therefore incorporate the joint impact of executive and congressional budgetary processes on budgetary outcomes. Defense spending (column 1) is heavily influenced by parochial defense considerations but displays substantial interdependence. Defense spending is negatively influenced by uncontrollable spending pressures and positively by revenues. A one-dollar increase (decrease) in revenues leads to an estimated 30¢ increase (decrease) in defense spending. An increase (decrease) in uncontrollable spending pressures, as reflected in a one-dollar increase (decrease) in prior-year uncontrollable spending, leads to an estimated 18¢ decrease (increase) in defense spending.

Interdependence within the uncontrollable and controllable spending categories is weaker. For uncontrollable programs, the coefficients for two of the three parochial defense variables, the parochial controllable spending variable, and the revenues variable have signs that are consistent with interdependence, although none is statistically significant. For controllable domestic programs, one of the five relevant coefficients, that for the perceived Soviet threat measure, has a sign consistent with interdependence and is significant. None of the other four coefficients, however, is statistically significant or the sign hypothesized by interdependence. The only other significant coefficient among the spending pressure variables in the domestic controllables regression is that for prior-year controllable expenditures, a result consistent with a purely bottom-up or incrementalist decision process.

The results presented in the last column of Table 1 employ the sum of the three expenditure components as the dependent variable. This specification allows for an assessment of the net impact of the various budgetary, political, and fiscal policy variables on total spending. Total expenditures are positively influenced by

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parochial spending pressure variables for all three expenditure categories, as well as the level of available revenues. In other words, total federal spending is determined by a combination of bottom-up (i.e., program-specific) and top-down (i.e., revenue-based) pressures.¹¹

The coefficients for the variables other than the lagged endogenous ones in Table 1 are also of interest. Our measure of the level of armed conflict is significant in the defense spending specification and in the equation for total spending. The perceived Soviet threat variable assumes the

**Table 1. Benchmark Specification, Congressional Outcomes
Regressed on Prior-Year Variables**

Independent Variables	Dependent Variable					
	Congressional Defense Outlays	Congressional Uncontrollable Outlays	Congressional Controllable Outlays	Congressional Total Outlays		
Constant term	-.02 (.02)	-.02 (.02)	.01 (.02)	.01 (.02)	.04 (.03)	.02 (.03)
Prior-year actual uncontrollable outlays	-.18 ^a (.10)	-.18 ^a (.09)	.79 ^b (.10)	.80 ^b (.08)	.19 (.15)	.80 ^b (.16)
Prior-year actual defense outlays	.54 ^b (.14)	.51 ^b (.11)	-.15 (.15)	-.15 (.12)	.09 (.21)	.47 ^b (.23)
Level of armed conflict	.93 ^b (.32)	.90 ^b (.30)	-.27 (.34)	-.26 (.29)	.43 (.48)	1.10 ^a (.52)
Perceived Soviet threat measure	.88 (.66)	.99 ^b (.35)	.38 (.68)	.40 (.59)	-1.78 ^a (.97)	-.52 (1.10)
Prior-year actual controllable outlays	.02 (.14)		-.17 (.15)	-.17 (.13)	.52 ^a (.21)	.37 (.23)
Congressional revenues	.30 ^a (.11)	.32 ^b (.06)	.10 (.12)	.08 (.09)	-.05 (.17)	.35 ^a (.18)
Congressional unemployment measure	.27 ^a (.10)	.26 ^b (.09)	.06 (.11)	.05 (.09)	.21 (.15)	.54 ^b (.16)
Congressional inflation measure	-.12 (.07)	-.13 ^a (.06)	.09 (.07)	.10 ^a (.06)	-.13 (.10)	-.16 (.11)
Second year after presidential election	-.17 (.20)		.04 (.21)		.16 (.30)	-.03 (.32)
Third year after presidential election	-.09 (.20)		.06 (.21)		-.06 (.30)	-.09 (.33)
Fourth year after presidential election	-.40 ^a (.19)	-.32 ^a (.14)	-.05 (.20)		-.33 (.29)	-.78 ^a (.31)
Northern House Democrats	.59 (.44)	.56 (.41)	.17 (.47)		.25 (.66)	1.02 (.71)
R ²		.97	.97	.99	.58	.89
R̄ ²		.95	.96	.98	.22	.79
Durbin-Watson	2.41	2.39	1.82	1.79	2.29	2.45
						2.44

Note: Standard errors in parentheses.

^aSignificant at a 95% confidence level, one-tailed test.

^bSignificant at a 99% confidence level, one-tailed test.

expected sign in the defense and domestic spending equations. Its significance in the defense outlays equation is in some contrast with work of other scholars who have examined the statistical link between Soviet military behavior and the U.S. military budget.¹²

Increases in unemployment (controlling for revenues) have a stimulative effect on all three components of total spending, and a positive and significant effect on total spending. Each category of expenditures, as well as total spending, responds countercyclically to unemployment fluctuations. Moreover, the significant positive coefficient for unemployment in the defense spending equation suggests that the response of spending to unemployment includes both discretionary and automatic components. Higher inflation (controlling for revenues) is associated with lower spending for defense and controllable domestic programs, also consistent with a countercyclical spending posture. However, increases in inflation are associated with *increases* in spending for uncontrollable programs, which partly offset the countercyclical response to inflation of the other budgetary components.¹³ The procyclical impact of inflation on uncontrollable spending may reflect the operation of benefit indexing as well as discretionary benefit increases for uncontrollable programs during the inflationary 1970s.

Controlling for revenues, a 3.4% increase in inflation is required to decrease federal spending by an amount equal to the spending increase associated with a 1% decrease in unemployment.¹⁴ Combining these results with those of Kamlet and Mowery (1986) concerning the impact of inflation and unemployment on revenues allows for an assessment of the total effect of inflation and unemployment fluctuations on spending (i.e., including the impact of economic fluctuations on spending that operates through revenue fluctuations). Accounting for the

impact of economic fluctuations on revenues increases the disparity in the responses of federal spending to inflation and unemployment, with a 4.2% increase/decrease in inflation required to invoke the same total spending response as a 1% decrease/increase in unemployment. This seemingly asymmetric response of federal politicians to inflation and unemployment is consistent with arguments advanced by Buchanan and Wagner (1977) and others.

Is a political business cycle operating within this model? The coefficient for the dummy variable representing the third year after a presidential election generally has the wrong sign to support a political business cycle interpretation of budgetary behavior, and the only significant election proximity variable is that for the fourth year after a presidential election. The coefficient for this variable is negative in all the specifications and statistically significant in the defense and total spending equations. Rather than pursuing expansionary fiscal policies in the budget that takes effect during a presidential election year, budgetary outlays are held down for the budget debated during the campaign, which takes effect the following year. This effect seems strongest for defense spending. The results do not allow rejection of the hypothesis that political business cycles may be induced by manipulating the disbursement of expenditures *within* an election year, for instance, greater spending in the six months prior to the election. Election year effects on spending may also be more pronounced for individual programs included in one of the aggregate spending categories examined here.¹⁵ Nonetheless, these results cast considerable doubt on the relevance of the electoral business cycle to this instrument of federal economic policy.

Political ideology, as measured by the number of northern House Democrats, also exerts a modest influence on budget-

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ary outcomes. The northern House Democrat coefficient is positive in all the regressions in Table 1, but the coefficients are small and statistically insignificant. Other measures of congressional ideology—total House Democrats, Senate Democrats, total Democrats in the House and Senate, and a measure of congressional ideology derived by Foole and Daniels (1985) also yielded small and statistically insignificant coefficients. These findings must be interpreted cautiously, however, in view of the high level of aggregation of our data.¹⁶

The results of estimating a modified benchmark specification for presidential budget requests are shown in Table 2.¹⁷ Defense spending again displays strong interdependence. In addition to being positively influenced by the parochial defense variables, the defense budget is negatively influenced by uncontrollable expenditure pressures and positively by available revenues, results that are similar to those in Table 1. In contrast to the results in Table 1, the results in Table 2 for uncontrollable spending in the executive branch suggest considerable inter-

Table 2. Presidential Budget Figures Regressed on Prior-Year Variables

Independent Variables	Dependent Variable				
	Defense Outlays	Uncontrollable Outlays	Controllable Outlays	Controllable Outlays	Total Outlays
Constant term	.00 (.01)	-.00 (.01)	.02 (.02)	.03 (.02)	.02 ^a (.01)
Prior-year actual uncontrollable outlays	-.13 ^a (.06)	.81 ^b (.08)	.07 (.13)	.06 (.11)	.75 ^b (.09)
Prior-year actual defense outlays	.60 ^b (.10)	-.16 (.13)	.13 (.20)	.04 (.18)	.57 ^b (.14)
Level of armed conflict	.85 ^b (.22)	-.52 ^a (.27)	1.10 ^a (.45)	.53 (.48)	1.44 ^b (.31)
Perceived Soviet threat measure	.02 (.39)	-.46 (.42)	-1.15 (.78)	-.14 ^a (.07)	-1.60 ^b (.53)
Prior-year actual controllable outlays	.07 (.09)	-.04 (.11)	.32 (.18)	.31 ^a (.16)	.34 ^b (.13)
Presidential revenues	.18 ^b (.06)	.17 ^a (.07)	.05 (.13)	.09 (.11)	.39 ^b (.09)
Presidential unemployment measure	.07 (.05)	-.04 (.06)	.07 (.09)	.09 (.08)	.10 (.06)
Presidential inflation measure	-.07 ^a (.03)	.02 (.03)	-.02 (.06)	-.04 (.05)	-.07 ^a (.04)
Fourth year after presidential election	-.07 (.09)	.11 (.11)	-.46 ^a (.19)	-.48 ^b (.13)	-.42 ^b (.12)
Johnson dummy variable				.59 ^b (.26)	
R ²		.99	.99	.51	.63
R̄ ²		.98	.99	.25	.40
Durbin-Watson	1.70	2.38	1.97	2.45	2.43

Note: Standard errors in parentheses.

^aSignificant at a 95% confidence level; one-tailed test.

^bSignificant at a 99% confidence level, one-tailed test.

dependence. The revenues coefficient in column 2 is positive and significant. All three parochial defense variables have negative coefficients. The coefficient for the level of armed conflict variable is itself significant, while an F-test indicates that the three defense variables collectively exert a significant influence on uncontrollable spending at the .95 level. Within the executive branch, uncontrollable expenditures respond positively to revenues and negatively to defense and controllable spending pressures; that is, executive budgetary decision making for uncontrollable spending exhibits considerable interdependence.¹⁸

As in Table 1, controllable domestic programs display no interdependence within the executive branch. Indeed, the level-of-armed-conflict variable exerts a positive and significant influence on controllable domestic programs. This result, however, is probably due to the simultaneous growth of the Vietnam War and expansion of many Great Society programs. Employing post hoc analysis, the insertion of a Johnson administration dummy variable (column 4) eliminated the anomalous war coefficients. Consistent with the results in Table 1, the last column of Table 2 suggests that total spending in the president's budget is a function of both bottom-up and top-down spending pressures.

The signs of the coefficients for the economic variables throughout the specifications in Table 2 are generally consistent with those in Table 1. The absolute values of the coefficients, however, are substantially smaller. The impact on spending of inflation, relative to unemployment, also appears stronger within the executive branch.¹⁹ The weak direct influence of projected economic conditions on executive branch spending is surprising and may reflect the long lead times between the compilation of such forecasts in the executive branch and the disbursement of funds (as much as 24 months in

some cases). These results do not mean that fiscal policy pressures exert no influence on the president's budget. Both uncontrollable and defense outlays are sensitive to projected revenues. Moreover, unemployment and inflation affect projected revenues and therefore indirectly influence presidentially proposed uncontrollable and defense budgets. Nonetheless, the results suggest that discretionary fine-tuning of fiscal policy within the executive branch did not greatly affect spending in the president's budget during this period. The impact of presidential elections on presidential budgets is similar to that in Table 1, as the dummy variable for the fourth year after a presidential election acquires a negative coefficient in all of the equations but that for uncontrollable spending. Presidents seemingly wish to appear fiscally prudent in the budget that is debated during the presidential campaign, more so in defense and controllable domestic spending than in uncontrollable expenditures.

Table 3 presents the results for congressional actions on presidential budget requests, employing the same specifications as those reported in Table 2. The results for defense and controllable domestic spending are broadly consistent with Tables 1 and 2. Defense spending exhibits interdependence, while controllable domestic spending appears to be independent. In contrast to the executive branch results, however, congressional budgeting displays no interdependence in uncontrollable spending decisions. One cannot reject the hypothesis that total spending within Congress is independent of revenues. Congressional budgeting thus displays less interdependence than the executive branch, and such interdependence is confined solely to defense spending.²⁰

Similarly to the results in Tables 1 and 2, congressional spending decisions in all three categories respond countercyclically to unemployment. Only congressional

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defense spending, however, exhibits a significant countercyclical response to inflation. Total spending responds in a countercyclical fashion to both unemployment and inflation, but the disparity between the impact of unemployment and inflation on spending is larger within Congress than in the executive branch. The variable for the fourth year after a presidential election again acquires a negative and significant coefficient, indicating that Congress is more stringent in its treatment of the president's budget during presiden-

tial election years, particularly in defense spending.

Presidential Political Variables

Presidential political ideology represents a potentially important influence on budgetary behavior and has received considerable attention in recent work.²¹ Table 4 reports the results of adding both administration-specific dummy variables and, separately, a dummy variable for presidential party affiliation to a version

Table 3. Congressional Outcomes Regressed on Presidential Budget Figures

Independent Variables	Dependent Variable			
	Defense Outlays	Uncontrollable Outlays	Controllable Outlays	Total Outlays
Constant term	-.01 (.02)	-.01 (.01)	.06 ^a (.03)	.04 (.03)
Presidential uncontrollable outlays	-.14 (.09)	.88 ^b (.08)	.18 (.16)	.89 ^b (.16)
Presidential defense outlays	.57 ^b (.13)	-.15 (.11)	-.10 (.22)	.32 (.23)
Level of armed conflict	.54 (.35)	.07 (.31)	-.69 (.64)	1.30 ^a (.65)
Perceived Soviet threat measure	.91 ^a (.35)	-.25 (.30)	.17 (.64)	.83 (.64)
Presidential controllable outlays	-.04 (.02)	.30 ^a (.13)	.21 (.29)	.47 (.28)
Congressional revenues	.29 ^b (.10)	.09 (.09)	-.10 (.18)	.27 (.18)
Congressional unemployment measure	.23 ^a (.10)	.13 (.09)	.18 (.17)	.54 ^b (.17)
Congressional inflation measure	-.12 ^a (.06)	.02 (.05)	-.11 (.11)	-.22 ^a (.11)
Fourth year after presidential election	-.28 ^a (.16)	-.06 (.13)	-.19 (.28)	-.53 ^a (.28)
R ²	.97	.99	.30	.84
R ²	.96	.99	-.07	.76
Durbin-Watson	1.96	2.70	2.07	1.89

Note: Standard errors in parentheses.

^aSignificant at a 95% confidence level, one-tailed test.

^bSignificant at a 99% confidence level, one-tailed test.

Table 4. Coefficients for Administration and Presidential Party Dummy Variables When Added to Modified Benchmark Specifications

	Uncontrollable	Defense	Controllable
Kennedy	.12 (.27)	-.02 (.32)	.09 (.34)
Johnson	-.07 (.18)	.08 (.23)	.05 (.30)
Nixon	.22 (.30)	-1.10 (.68)	-.41 (.27)
Ford	-.22 (.53)	-1.10 (.97)	.49 (.40)
Carter	-.39 (.52)	-1.50 (1.00)	.06 (.32)
Democratic President	-.16 (.11)	.08 (.16)	.15 (.21)

Note: Standard errors in parentheses. Reported coefficients and standard errors multiplied by 100.

of the specification in Table 2.²² The coefficients for the remaining variables in each regression were not substantially altered from their respective values in Table 2 and, in the interests of space, are not reported.

The administration-specific variables and the presidential party variable are small in magnitude and statistically insignificant in all of the equations in Table 4, although the results contain weak evidence of reductions in defense spending during the Nixon, Ford, and Carter administrations. Similar results are obtained when administration-specific variables are introduced into the specifications in Tables 2 and 3. The only exception to this pattern concerns the effect (discussed above) of the Johnson administration on controllable spending in the president's budget.²³ Interacting inflation and unemployment with presidential political affiliation in these specifications also failed to reveal any significant party-based differences in the responsiveness of presidential budgets to economic conditions.

Summary and Conclusions

Previous quantitative studies of budgeting and political economy within the U.S. have been hampered by unrealistic assumptions concerning institutional structure and policy maker omnipotence, as well as a narrow focus on a single dimension of budgetary or programmatic policy to the exclusion of other factors. In this paper, we provide a framework for the integrated analysis and comparison of congressional and executive budgetary priorities, the role of top-down and bottom-up influences on spending, and the impact of political and economic variables on budgetary priorities.

The results of the tests for budgetary interdependence within Congress and the executive branch contain important implications for incrementalist theories of budgeting. The incrementalist hypothesis that individual agency allowances are independent, heretofore tested largely on the basis of the goodness of fit of non-nested models, receives little support in these results. Instead, the outcomes of the

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combined congressional and executive budgetary processes reflect both top-down and bottom-up spending pressures.

Budgetary interdependence appears greater within the executive branch than in Congress, judging from the results for total spending and uncontrollable spending. Budgetary outcomes within the executive branch are interdependent for both defense expenditures and uncontrollable expenditures, categories that accounted for over 80% of total noninterest outlays by 1981. Such a finding is consistent with prior studies of executive branch budgeting and contrasts with the assertions of Hecko (1985) and Schick (1985) that top-down budgeting within the executive branch is a novel phenomenon associated with the Reagan administration. Executive branch budgets have been heavily influenced by top-down pressures and policy targets during much of the postwar period.

Consistent with the critiques mentioned above, congressional budgeting displays substantially less interdependence than do executive branch budgets. Even within the congressional budget process, however, there are indications of interdependence within defense spending, as defense budgets respond to available revenues and pressures for expenditures in other programs. Why does congressional defense budgeting differ from congressional budgeting for other agencies and programs? Defense was the largest controllable budget category and represented the largest single appropriations bill during this period. An appropriations (and, implicitly, outlays) decision of such prominence may have been subject to a greater degree of reconciliation with fiscal and domestic budgetary policies than other, smaller appropriations bills. Alternatively, the size and saliency of this spending bill may have given presidents more influence on congressional decision making in defense budgeting than in other areas. Regardless of the precise explana-

tion, this result underlies the importance of examining different spending categories separately in the analysis of budgetary decision making.

The incrementalist analysis of budgetary decision making emerges from this analysis somewhat bruised for Congress and requires considerable qualification and revision for the executive branch. The incrementalist paradigm has been tested more extensively within Congress, generally for domestic controllable programs. It is exactly here, within Congress and in domestic controllable spending in both Congress and the executive branch, that we find budgetary independence to be most significant and the incrementalist description most accurate. Our analysis, therefore, does not completely refute the incrementalist conclusion concerning budgetary independence but suggests that such independence is far from pervasive within the federal budget. Moreover, the share of total federal spending within which budgetary independence is significant in both the executive branch and Congress has declined considerably during the post-war period.

has declined considerably during the post-war period.

This study also supports a partial revision of the role and significance of uncontrollable spending. Their name notwithstanding, uncontrollable programs display substantial interdependence within executive branch budgeting, responding to the revenue environment and pressures for expenditures in other programs. Executive branch policy makers exercise some control over proposed spending for uncontrollable programs, either through legislative changes proposed with the release of the presidential budget or through the manipulation by these policymakers of economic and demographic estimates. Indeed, uncontrollable programs appear more interdependent than controllable domestic programs, consistent with the arguments

of Schick (1977) and others that putatively controllable spending may be more difficult to reduce than uncontrollable spending.²⁴ While provocative, these results leave open the possibility that the actual level of uncontrollable expenditures may in fact be difficult to control; the *projected* level of such expenditures in the president's budget may be malleable, however, through the strategic employment of assumptions.²⁵

Our empirical results also indicate that economic factors exert a major influence on budget outcomes, although the nature of the influence varies considerably across spending categories. Inflation produces countercyclical responses in defense and controllable programs but not in uncontrollable spending. Total spending responds in a weakly countercyclical fashion to inflation. Unemployment provokes a countercyclical spending response in all three spending categories. Total spending appears to respond to fluctuations in inflation and unemployment in an asymmetric fashion.

In contrast to the strong impacts of economic factors on budgetary priorities, the influence of political variables, such as the party composition of the House, party affiliation of the White House, and even administration-specific effects is modest at best. At this level of aggregation, budgetary allocations appear to be determined primarily by structural factors and interdependencies that exhibit considerable stability during this period.²⁶ These stable, relatively nonideological spending priorities contrast sharply with the portrayals in the political economy literature. In addition, the sensitivity of budgetary behavior to the proximity of presidential elections cited by the political business cycle literature does not exist in these data.

This analysis thus provides little support for a strictly ideological interpretation of the wellsprings of presidential budgetary and economic policy priorities

within the postwar U.S. economy. The differences between Democratic and Republican budgetary policies or between administrations generally appear modest by comparison with the differences between executive and congressional policies during the postwar period.²⁷ Indeed, these results suggest that separating the analysis of executive budgetary behavior from that of Congress is extremely important.

As in any exercise of this sort, our findings are subject to important qualifications. This modelling framework cannot control thoroughly for strategic behavior in presidential and congressional budgeting. Budgetary priorities are measured at a high level of aggregation. This analysis cannot address the competing influence of top-down and bottom-up influences *within* such categories as domestic controllable spending. Similarly, many of the conclusions concerning the role of political influences may be sensitive to aggregation. The empirical analyses are constrained by the small number of observations and the potentially unlimited set of independent explanatory variables. Nonetheless, our empirical results appear robust to different formulations of the predetermined variables. The specifications assume a high degree of stability in budgetary priorities over the 1955-81 period. This assumption is plausible, given the stable structure of the budgetary process during this period and the significant and reasonable coefficient estimates and the high goodness of fit statistics, but it is difficult to test due to the small number of degrees of freedom.

Despite these qualifications, the empirical analysis we have presented allows the various influences on budgetary priorities in the executive branch and Congress to interact in a fashion not previously considered. Moreover, the solution to the problems we have noted is not continued reliance on the assumptions that executive and congressional budgetary priorities are

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the same, that observed macroeconomic outcomes are intended outcomes, or that all spending categories respond identically to fiscal policy and to political, economic, and other influences on budgetary outcomes. Instead, these difficulties underscore the importance of further comparative and disaggregated empirical research and suggest directions for future inquiry.

Appendix

The following provides a listing and definitions of the variables employed in the empirical analysis:

Variable Definitions

Congressional uncontrollable. Uncontrollable domestic outlays for fiscal year t following congressional action on fiscal year t 's budget, divided by GNP for calendar year t .

Presidential uncontrollable. Uncontrollable domestic outlays for fiscal year t as proposed in the president's budget, divided by GNP for calendar year t .

Actual uncontrollable. Actual uncontrollable domestic outlays for fiscal year t , divided by GNP for calendar year t .

Congressional Defense. Department of Defense (DoD) outlays for fiscal year t following congressional action on fiscal year t 's budget, divided by GNP for calendar year t .

Presidential Defense. DoD outlays for fiscal year t as proposed in the president's budget, divided by GNP for calendar year t .

Actual defense. Actual DoD outlays for fiscal year t , divided by GNP for calendar year t .

Congressional controllable. Controllable domestic outlays for fiscal year t following congressional action on fiscal year t 's budget, divided by GNP for calendar year t .

Presidential controllable. Controllable domestic outlays for fiscal year t as

proposed in the president's budget, divided by GNP for calendar year t .

Actual controllable. Actual controllable domestic outlays for fiscal year t , divided by GNP for calendar year t .

Congressional revenues. Revenues (as defined in text) for fiscal year t following congressional action on fiscal year t 's budget, divided by GNP for calendar year t .

Presidential revenues. Revenues (as defined in text) for fiscal year t as proposed in the president's budget, divided by GNP for calendar year t .

Actual revenues. Actual revenues (as defined in text) for fiscal year t , divided by GNP for calendar year t .

Congressional inflation measure. The annual inflation rate in the government price deflator for the first six months of calendar year $t-1$.

Presidential inflation measure. The annual inflation rate projected by the executive branch for calendar year $t-1$.

Congressional unemployment measure. Change in the civilian unemployment rate from the third quarter of calendar year $t-2$ to the third quarter of calendar year $t-1$.

Presidential unemployment measure. Change in the civilian unemployment rate from the fourth quarter of calendar year $t-2$ to the level projected by the executive branch for calendar year $t-1$.

Level of armed conflict. Change in the number of U.S. servicemen killed in action between calendar year $t-2$ and calendar year $t-1$. Reported coefficients and standard errors are multiplied by 1 million.

Perceived Soviet threat. Average conflict score of messages sent from the Soviet Union to the U.S. for calendar year $t-2$ as coded in the Conflict and Peace Data Bank (COPDAB) data set. Reported coefficients and standard errors are multiplied by 1,000.

Northern House Democrats. Number of nonsouthern Democrats in the House

of Representatives during calendar year $t-1$. Alabama, Arkansas, Florida, Georgia, Kentucky, Mississippi, North Carolina, South Carolina, Tennessee, Texas, Virginia, and Louisiana are coded as southern states. Reported coefficients and standard errors are multiplied by 10,000.

n^{th} year after presidential election. A dummy variable assuming a value of 1 during the n^{th} year of an administration. Reported coefficients and standard errors are multiplied by 100.

Budget figures were coded directly from the *Budget of the United States Government*. Trust fund receipts (e.g., FICA) and trust fund outlays (e.g., Social Security) were included for all years. (Prior to fiscal year 1968, trust fund activities were not included in the annual presidential budget document.) The congressional figures for fiscal year t were obtained from the president's budget for fiscal year $t+1$, released in January of calendar year t . Revenues include total projected revenues (including payroll taxes) plus receipts for oil leases on the outer continental shelf, interest paid to trust funds, and employee contributions to federal retirement programs (currently treated in the presidential budget as "offsetting receipts") minus gross interest payments. (Exclusion of interest payments from revenues is necessary for symmetry with our exclusion of interest payments on the expenditure side.) Uncontrollable programs include Medicare, Medicaid, Old Age Survivors and Disability Insurance (OASDI), Civil Service retirement benefits, railroad retirement benefits, veterans' pensions, unemployment compensation, Aid for Families with Dependent Children (AFDC), food stamps, and school lunch programs.

The congressional measure of inflation for fiscal year t is the rate of inflation (based on the GNP deflator) during the second and third quarters of calendar year $t-1$, a period during which Congress gen-

erally considers fiscal year t 's budget. The Congressional measure of unemployment for fiscal year t is the change in the civilian unemployment rate between December of calendar year $t-1$ and the previous December. The presidential measure of inflation for fiscal year t is the executive branch's projected inflation for calendar year $t-1$. The presidential measure of unemployment for fiscal year t is the difference between the executive branch projection for unemployment during calendar year $t-1$ and the unemployment rate during January of calendar year $t-1$. Economic forecasts for the executive branch were taken from the president's budget after fiscal 1976 and, for previous years, from internal executive branch memoranda obtained from the Office of Management and Budget and several presidential libraries. In most cases, these projections were made during November or December of calendar year $t-2$, in conjunction with the final phase of the executive budgetary process for fiscal year t . These variables and their collection are described in more detail in Crecine, Kamlet, Mowery, and Winer (1981). Approximately 15% of our series were missing observations. Actual magnitudes were substituted for these missing data in the reported results. Elimination of these observations entirely did not meaningfully alter our coefficient estimates. In the absence of comparable forecasts for congressional budgeting, we used for fiscal year t the inflation and unemployment rates for the second and third quarters of calendar year $t-1$.

The level of armed conflict measure is defined as $W_t = KIA_{t-1} - KIA_{t-2}$, where KIA_t denotes the number of U.S. servicemen killed in action in calendar year t . For a detailed discussion of the rationale for the use of this variable, see Fischer and Crecine (1981) and Fischer and Kamlet (1984). The perceived Soviet threat variable is drawn from the Conflict and Peace Data Bank (COPDAB) data set. This data

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set, compiled under the direction of Professor Edward Azar, codes occurrences between nations that are covered in any of approximately 70 major public sources, primarily newspapers. Interactions are coded in terms of the countries involved (distinguished between a sending nation and a receiving nation) and the type of issue involved (e.g., economic, military, cultural, etc.) using a fifteen-point conflict scale. The measure of perceived Soviet threat is based on the average conflict-scale ranking of all of the communications from the USSR to the U.S. coded under the military category for each calendar year. For a review of the COPDAB data set see Azar and Havener (1976), Howell (1983), Vincent (1983), and McClelland (1983).

An alternative budgetary outcome measure to that used here is the "full employment budget" (or the associated deficit or surplus), which measures total spending at some notional level of full employment in the national economy. This measure is employed in Pack (1985) and Lowery (1985). Smyth (1970) and others, however, raise a number of theoretical objections to the appropriateness of the full employment surplus as an indicator of short-term fiscal policy. In addition, there exist no disaggregated formulations of the full employment budget across budgetary spending categories, and existing estimates of full employment spending for longitudinal analysis are quite crude.

Our spending revenue variables are divided by GNP. Among recent works, Price (1978), Cameron (1978), Knott (1981), Alt and Chrystal (1983), Lowery and Berry (1983) and Hicks (1984) measure expenditures as a share of GNP. Measuring expenditure variables relative to GNP reduces heteroskedasticity problems. Other techniques, of course, can also be used to correct heteroskedasticity, for instance, a three-step estimation procedure proposed by Glejser (1969). The

Glejser procedure was performed on the basic specifications reported in Table 1. Reestimating the basic specifications using this alternative correction procedure did not significantly alter the magnitude of any of the coefficients.

Estimation of the reduced-form equations is based on the assumption that the error terms in equation system (2) are normally distributed and independent of one another across time (this latter assumption is tested, see below). Identity (2e) makes one of the equations (2a) through (2d) superfluous in the sense that its parameters are completely identified, given the estimates of the other equations. Ordinary least squares on each equation individually provides an efficient estimation procedure for the equation system, given the absence of a specific structural system (Judge, Griffiths, Hill, Lutkophol, and Lee 1985, 619). The modeling approach in this paper is substantially more general than the CALM framework of Fischer and Kamlet (1984), although this increased generality of the model proposed here comes at a cost: no longer can one infer the structural parameters of equation system (1) from the reduced-form system (2).

In testing for the independence of the error term for each equation over time, the Durbin-Watson statistic is calculated and reported for each regression. It is never in the range indicating the presence of first-order serial correlation. Strictly speaking, the lagged expenditure variables in our specifications are not lagged endogenous variables, because they measure past levels of actual spending rather than past values of the dependent variable, which is the budget allowance as proposed by the president or as acted on by Congress. Nonetheless, these variables share important statistical features with "true" lagged endogenous variables. In the presence of lagged endogenous variables, the Durbin-Watson statistic is generally biased toward 2 and is of diminished

power. In the presence of lagged endogenous variables, the Durbin-h statistic, when calculable, provides the basis for an asymptotically valid test for first-order serial correlation. The Durbin-h statistic is calculable for all our regressions and in none of the regressions is the statistic greater than .64, well below the two-tailed 95% significance level of 1.96.

Our estimation of the reduced-form equations assumes that revenues are determined independently of concurrent budgetary and fiscal policy pressures. Support for this assumption is provided in Kamlet and Mowery (1986). The specifications, however, can also be estimated on the assumption that the revenue variable is endogenous, employing prior-year revenues as an instrument in two-stage least squares. This alternative procedure was employed for each of the specifications reported below, without substantially affecting our conclusions in any instance.

Notes

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1. For a recent survey of this work, see Alt and Chrysal (1983).

2. This statement by Chappell and Keech is typical of the literature: "We will assume that the president, through fiscal and monetary policies, controls real output" (1985, 13).

3. Examples of such work include models of the "political business cycle," for example, Nordhaus (1975), MacRae (1977), and Tufte (1978); models of government growth, for example, Niskanen (1971,

1975), and policy "reaction function" models, including Hibbs (1977), Beck (1982), Frey and Schneider (1978a and b, 1979), Russett (1982), Domke et al. (1983), Hicks (1984), and Lowery (1985). Exceptions are Kiewiet and McCubbins (1985a, 1985b).

4. These works focus on the atomistic structure of congressional budgetary decision making, differences between legislative and bureaucratic decision processes, differences in budgetary incentives between bureaucrats and congressional politicians, and differences in budgetary incentives between congressional and executive politicians. In a complementary vein, Ferejohn (1974), Weingast, Shepsle, and Johnsen (1982), and others argue that the decentralized and fragmented congressional decision process promotes excessive pork barrel spending.

5. The use in journalistic and scholarly analysis of the term *uncontrollable* to characterize many domestic entitlement programs also implies a lack of responsiveness of the budgets of such programs to fiscal, defense, and other domestic priorities.

6. This procedure contrasts with the bulk of the literature (including virtually all studies of congressional budgeting) that focuses on budget authority. See Mowery et al. (1980) or Kamlet and Mowery (1983, 1985a) for a more detailed discussion of budget authority and outlays in congressional and executive budgeting.

7. Estimation using ordinary least squares of equations with lagged endogenous variables yields coefficient and standard error estimates that are consistent (rather than unbiased) and the estimation procedure is only efficient asymptotically. Moreover, problems of multicollinearity often are exacerbated when lagged endogenous variables are employed as explanatory variables and lagged endogenous variables complicate testing and correcting for serial correlation (see the discussion in the appendix).

8. Lagged endogenous variables allow for the specification equations to be interpreted in the context of a partial adjustment model (see, e.g., Pindyck and Rubinfeld 1982). Nonetheless, the issue of the speed of adjustment of policies to changes in exogenous influences underlines the importance of avoiding "structural" interpretations of the coefficients of the reduced-form model. If responses to political and economic variables are consistent with a partial adjustment framework, the reduced-form coefficients will represent, among other things, the rate of adjustment of budgetary expenditure to their "desired" levels, rather than capturing the full long-term impact of exogenous variables on spending priorities.

9. Although this is a very simple model of expectation formation, Mullineaux (1980) suggests that expectations for inflation based on such extrapolation may be justified from a "rational expectations" perspective.

10. As noted above, the specific policy instru-

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ments employed in such macroeconomic manipulations are typically ignored. The conclusions of these works have been disputed in other studies, e.g., Golden and Poterba (1980), Alt and Chrystal (1983), Thompson and Zuk (1983), and Lowery (1985). Apart from assuming a president capable and willing to manipulate the economy with a high degree of precision and timing, political business cycle theories typically assume, implicitly or explicitly, some form of retrospective theory of voting. The purely retrospective theory of voting has also been disputed by several recent works, for example, Chappell (1983), Chappell and Keach (1985), Lewis-Beck (1986).

11. These interdependence effects are not artifacts of dividing the spending and revenue variables by GNP, undertaken to correct for heteroskedasticity. Running ordinary least squares (OLS) on the raw spending variables (with suitably transformed non-spending, nonrevenue variables) produces comparable results; "significant" estimates of the same sign are obtained for the coefficients whose values determine the presence or absence of budgetary independence. Significance in such a context must, of course, be qualified by the fact that, given the presence of heteroskedasticity, the standard errors reported in running OLS are generally inconsistent—one of the reasons for dividing spending and revenue variables by GNP in the first place. Similar results are obtained when the concurrent GNP value is replaced by GNP lagged by one year.

12. See, for example, McCubbins (1983), Cusack and Ward (1981), and Moll and Luebbert (1980). Domke et al. (1983) report positive responsiveness of defense spending to a measure of East-West tension constructed by Goldmann and Lagerkranz (1977). This variable was not significant in our analyses; nor was the measure of Soviet military expenditures employed by Fischer and Kamlet (1984).

13. Again, the division of our dependent and explanatory variables by GNP raises the possibility that the apparent responsiveness of the dependent variable (outlays as a share of GNP) to inflation and unemployment reflects fluctuations in GNP, rather than in nominal budget outlays. As Berry (1985) has noted, either fluctuations in nominal outlays, nominal GNP, or some combination of the two may be responsible for these findings. In order to examine this issue further, we ran three specifications employing alternative formulations of explanatory and dependent variables. The first specification divided both the dependent and lagged endogenous variables by GNP_{t-1} ; the second, in addition, divided inflation and unemployment by GNP_{t-1} ; and the third used raw outlay figures (undivided by any GNP figure), multiplying the economic variables by GNP. In all cases, the nature of the impact of unemployment and inflation on budgetary outcomes

is comparable to that reported in Table 1. See Kamlet and Mowery (1985b) for a more detailed discussion of this issue.

14. See Kamlet and Mowery (1985b) for the derivation of this and other figures relating to relative impacts of inflation and unemployment fluctuations on total spending.

15. Kiewiet and McCubbins (1985b) find that congressional elections exercise a positive influence on budget authority allowances for controllable, particularistic programs, which are not examined separately in our aggregate analysis. If this congressional election-based spending cycle were true, we would anticipate the coefficients for the variables representing two and four years after a presidential election to be negative and of comparable magnitude and that for three years after a presidential election to be zero in the regressions of congressional outcomes on prior-year variables (Table 1) and in the regressions of congressional outcomes on presidential budget figures. In neither case is the coefficient for the third year after a presidential election significantly less than that for the second year after a presidential election for any of the individual spending categories or for total spending. It may be that the effect of congressional election years on controllable, particularistic programs washes out when they are combined with other programs. Or, there may be different election year behavior for outlays (employed here) and budget authority (which Kiewiet and McCubbins use), implying a political business cycle that operates through greater generosity in budget authority, possibly in response to more effective interest group pressure during an election year, while outlays are restrained. The possibility of such budget authority-outlays side payments requires further research, but it suggests a very different model of the political business cycle in which interest group pressures, rather than the desire for economic stimulus, drive spending fluctuations.

16. Kiewiet and McCubbins (1985b) obtain a significant effect for House Democrats, although only for non-public works agencies in an analysis that employs budget authority rather than outlays.

17. The specification differs from that in Table 1 by the omission of northern House Democrats and two of the election year proximity variables as independent variables, due to their lack of statistical significance in Table 2. A simple F-test, analyzing the joint effect of the exclusion of these variables on the explained variance, suggests that their exclusion does not significantly affect the goodness of fit of the equations. This exclusion also did not affect the magnitude of any of the estimated coefficients in the equations.

18. For several reasons, the influence of uncontrollable spending pressures and the revenue environment on uncontrollable spending are not simply artifacts of the positive growth trends of uncontrollable spending and revenues and the

negative time trend in defense spending (measured as shares of GNP). The level of armed conflict (WAR) variable exhibits no time trend yet acquires a negative and significant coefficient in the uncontrollable regression. Similarly, the revenue variable has a positive coefficient in the defense spending regression, despite the opposite direction of the growth trends for revenues and defense spending. It is true that substantial collinearity is present between some of the independent variables, a problem that plagues many budgetary models. However, such collinearity does not produce biased coefficient estimates, only larger standard errors. That variables in this analysis have statistically significant coefficients implies that their separate impacts on the dependent variable are determined with a high degree of confidence, despite the presence of collinearity.

19. Similar results were obtained when contemporaneous inflation and unemployment measures rather than projections are used and when absolute, rather than first difference, measures of unemployment are used. These results are similar to those in Kiewiet and McCubbins (1985b), who also found Congress to be more responsive than the executive branch to economic fluctuations in budgetary formulation.

20. Two tests were undertaken to determine whether the Congressional Budget Reform and Impoundment Control Act of 1974, a key goal of which was the enforcement of greater top-down fiscal pressure and interdependence within congressional budgeting, had any impact on congressional budgetary outcomes. The specifications in Table 3 were reestimated for the pre-1974 period; no significant changes in the sign or magnitude of any coefficients were observed. In a second test, a dummy variable assuming a value of 1 for the post-1973 period and 0 elsewhere was interacted with the revenues and other spending terms in each specification. Once again, no significant results were obtained. This lack of impact is consistent with the conclusions of other works that have argued that the Congressional Budget Act has had a modest impact at best on congressional budgetary outcomes. Kamlet and Mowery (1985a) review this literature.

21. Hibbs (1977, 1979), for instance, argues that Democratic presidents place greater weight on reducing unemployment (and less on reducing inflation) than Republicans, while Beck (1982) contends that differences among presidents in their commitment to reducing unemployment are administration-specific, rather than a function of presidential political affiliation. Kiewiet and McCubbins (1985b) report a positive effect of Democratic presidents on controllable spending in the president's budget. Browning (1984), however, suggests that entitlement spending has risen most dramatically under Republican presidents.

22. The specification from Table 2 was modified by dropping insignificant variables. The F-test for

the joint significance of the excluded variables suggested that this exclusion did not affect the explained variance. In addition, the estimated coefficients for the variables that were retained were not affected substantially by dropping these variables. Exclusion of these nonsignificant variables is likely, if anything, to bias our results in favor of accepting the hypothesis that administration effects are significant.

23. The overall pattern of insignificance also remains when administration dummy variables are constructed as spikes, with a 1 in the first year of the administration and a 0 elsewhere, to allow for a one-time impact of a new administration on spending priorities. Placement of the spike in the second or third year of the administration also yields insignificant results.

24. 'The label "uncontrollable" is simply a short-hand way of identifying items which cannot be controlled without changes in the law. But the label veils a significant paradox of budgeting: most of the "uncontrollables" can be controlled, but most of the "controllables" really cannot" (Schick 1977).

25. The period examined is one during which uncontrollable expenditures increased relative to both the rest of the budget and GNP. As such, these results do not speak to the possibility of reductions in uncontrollable expenditures in response to other budgetary and fiscal spending pressures, although in fact every president since Nixon has proposed reductions in some uncontrollable programs (see LeLoup 1978).

26. An interesting question for future investigation concerns the extent to which the Reagan administration represents a departure from this consensus on budgetary priorities during the 1955-1981 period.

27. Similar conclusions have been reached by students of another central element of the U.S. political economy, trade policy. Baldwin and Richardson note that

the general consistency of U.S. trade policy over time is all the more remarkable given the frequent change of political party in power, especially in the Executive branch, but also in the Congress. Party affiliation, in fact, seems no longer to be a useful predictor of U.S. trade initiatives. A more useful predictor appears to be some measure of Executive versus Congressional control. The two branches of U.S. government have different outlooks on trade policy due to differences in constituencies. Conflict has punctuated relations between branches of government much more often than between political parties. (1984, 4)

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AGENDAS AND THE CONTROL OF POLITICAL OUTCOMES

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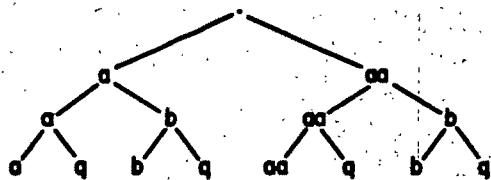
A considerable theoretical literature argues that if everyone votes sincerely, then an agenda setter has near dictatorial influence on final outcomes, whereas if everyone votes strategically, then an agenda setter's power is considerably reduced. This literature assumes that all feasible agendas are of a special type called amendment agendas. But actual legislative and committee agendas—notably those found in Congress—often are not of this type. Once we expand the domain of feasible agendas to include all types allowed by common parliamentary practice, the influence of agendas on legislative outcomes expands, even with strategic voting. Besides showing with counterexamples that previous results do not extend to a more realistic domain of agendas, we prove some theorems that specify the limits (such as they are) of an agenda setter's power.

Beginning with Farquharson (1969), a growing literature has investigated the interplay between agenda structure and voting strategy in legislatures and committees by examining an agenda setter's ability to manipulate outcomes and the ability of strategic voters to offset an agenda setter's power. The results of these investigations appear to support the following picture. If voters act *sincerely* rather than *strategically*—if at every vote they express their true preferences between alternatives instead of looking ahead to final consequences—an agenda setter is a near dictator: the set of outcomes achievable by different agenda forms is comparatively large (McKelvey 1979, Miller 1980, Schwartz 1986). But if voters act strategically, an agenda setter's power is not so great: the set of achievable outcomes is generally smaller (Banks 1985, Miller 1980, Shepsle and Weingast 1984).

Some have taken such results to measure the extent to which legislative decisions are at the mercy of elites who control agendas (Riker 1982). But even if no individual or small group controls agendas—even if the agenda-setting process is open and democratic—the power of a *hypothetical* agenda setter is important for political analysis. It is important because it reflects the degree to which outcomes depend on procedural structure rather than votes, hence the degree to which the real legislative game occurs at the agenda-setting stage rather than the voting stage.

The utility of these results is impaired, however, by the unrealistic assumption that feasible agendas are all of a special type called *amendment agendas*. Such agendas work as follows: a sequence of alternatives is given, and a vote is taken between the first two, after which the winner is pitted against the third alternative, then the winner in this second

Figure 1. A Four-Alternative
Amendment Agenda



ballot against the fourth alternative, and so on. Suppose, for example, that just three motions are on the floor of the U.S. House or Senate: a bill, an amendment to the bill, and an amendment to the amendment. Then there are four possible outcomes:

- q*: the status quo ante
- b*: the bill unchanged
- a*: the bill changed by the original amendment
- aa*: the bill changed by the amended amendment

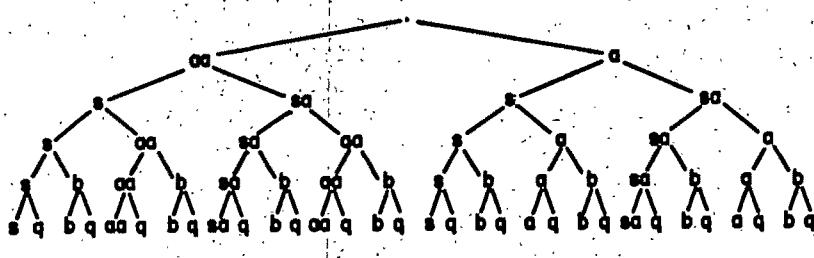
For these alternatives, congressional procedure requires an amendment agenda in which the first vote is between *a* and *aa* (whether to amend the amendment), the second between the winner and *b* (whether to amend the bill), and the third between the winner and *q* (whether to pass the final form of the bill). We represent this agenda by the tree in Figure 1.

Not all realistic agendas are of this sort, however. Suppose, as in Riker's (1958) account of the 1953 Agricultural Appropria-

priations Act, that the following motions are introduced in the U.S. House: a bill, an amendment to the bill, an amendment to the amendment, a substitute amendment, and an amendment to the substitute. Rule XIV of the House requires that members perfect the amendment and the substitute amendment before deciding whether to substitute and then whether to amend the bill. Hence, the agenda, shown in Figure 2, first pairs the bill perfected by the original amendment (*a*) with the bill perfected by the amended amendment (*aa*). But then, unlike an amendment agenda, this one does not pair the winner of the first ballot with anything in the second stage. Instead, the bill perfected by the substitute amendment (*s*) is paired with the bill perfected by the amended substitute amendment (*sa*). The next vote pairs the winners in these first two votes, and the winner in this vote is then paired with *b*. The final vote pits the status quo (*q*) against the survivor of all previous votes—or this vote is postponed until other sections of the bill have been considered (see Bach 1981, Sullivan 1984).

The difference between the agendas in Figures 1 and 2 is subtle. In Figure 1 the winner at one stage enters the balloting in the next, but in Figure 2 *s* and *sa* are paired in the second ballot regardless of which alternative wins in the first. Are such differences in form merely stylistic, or do they affect the power of agendas and agenda setters and the efficacy of strategic voting?

Figure 2. A Congressional Agenda with Substitutes



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Our principal conclusion is that by focusing on amendment agendas, the literature conveys an inaccurate picture. Quite a variety of agendas is available in Congress and other institutions, and few of the theorems about amendment agendas apply generally. The critical assumption of previous results concerning the sensitivity of outcomes to agendas is not that voters are strategic or sincere but that all feasible agendas are amendment agendas. One might suppose that results about amendment agendas fill in part of the map, applying to an important class of procedures. The assumption commonly made, however, is not that the actual agenda is of the amendment type nor that the set of feasible agendas contains amendment agendas but that this set contains *only* amendment agendas, and that assumption is rarely true.

After surveying common agenda forms in the first section, we draw some structural distinctions among agendas in the second section, then define strategic and sincere voting in the third section. The fourth section is a review of previous results based on the amendment-agenda assumption. In the fifth section we show with counterexamples that these results no longer hold when the amendment-agenda assumption is relaxed to allow for other agenda forms. In the sixth section we state several theorems (proved in the Appendix) about the actual limits of a hypothetical agenda setter's power under sincere and strategic voting.

Examples of Alternative Agenda Forms

Figure 2 depicts a common type of congressional agenda, but even more complex agendas are possible (see Bach 1981). Suppose the following eight motions are introduced: a bill, an amendment to perfect the bill, an amendment to perfect the amendment, a substitute amendment, a substitute bill, an amendment to perfect

the substitute bill, a substitute for the substitute bill, and an amendment to perfect the latter substitute. These motions give rise to nine outcomes:

- q*: the status quo
- b*: the bill unchanged
- a*: the bill perfected by the amendment
- aa*: the bill perfected by the amended amendment
- sa*: the bill perfected by the substitute amendment
- sb*: the substitute bill
- asb*: the amended substitute bill
- ssb*: the substitute for the substitute bill
- assb*: the amended substitute for the substitute bill

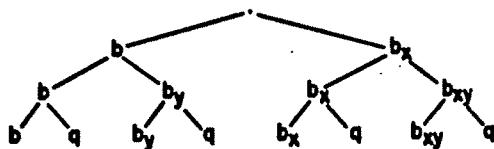
The voting order is as follows (Sullivan 1984):

1. *a* vs. *aa*
2. winner of 1 vs. *sa*
3. winner of 2 vs. *b*
4. *sb* vs. *asb*
5. *ssb* vs. *assb*
6. winner of 4 vs. winner of 5
7. winner of 3 vs. winner of 6
8. winner of 7 vs. *q*

We recoil from drawing the tree for this agenda, but note that this and the previous example both fail to qualify as amendment agendas because of *discontinuities*—steps at which the winner of the previous vote does not enter the balloting but is set aside for later consideration. In the last example, the alternatives paired in the fourth and fifth ballots are independent of all previous winners.

The existence of discontinuities is not the only way agendas in Congress and other institutions can differ from amendment agendas. Notice that the agendas in Figures 1 and 2 share two properties: First, they are *symmetric*—that is, each tree splits into two subtrees that are alike except for the interchange of alternatives (*a* and *aa*) occupying their top nodes;

Figure 3. A Two-Period Congressional Agenda



each of these subtrees splits in the same way, and so on down the agenda. Second, each is a *one-period* agenda—that is, all motions are on the floor before any are voted on. In a multiperiod agenda, by contrast, some motions are voted on before others are recognized. For example, suppose that, in addition to b and q , we also have

- b_x : the bill perfected by amendment x
- b_y : the bill perfected by amendment y
- b_{xy} : the bill perfected by both amendments

In this case standard parliamentary procedure requires a two-period agenda in which x is voted up or down before y is recognized. Depicted in Figure 3, this agenda requires three comparisons between alternatives:

1. b vs. b_x
2. { b vs. b_y if b wins at 1
 b_x vs. b_{xy} if b_x wins at 1
3. winner of 2 vs. q

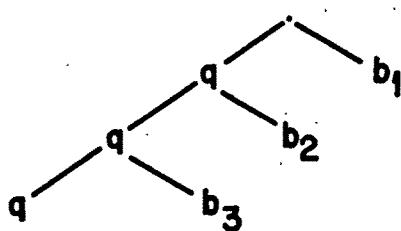
Besides being multiperiod, the agenda in Figure 3 differs from those in Figures 1 and 2 in that it is not symmetric. The tree branches into two subtrees that are dissimilar even apart from the interchange of b and b_x : one contains b_y where the other contains b_{xy} . This is because either b_y or b_{xy} enters the balloting at the second round, but *which* of them does so depends on which alternative— b or b_x —wins at the first round. We call the agenda in Figure 3 a *first-order* agenda because it involves only first-order amendments (amendments to the original motion) and

because these amendments are compatible (one outcome can contain all of them). Every first-order agenda is continuous, but it is not symmetric unless it involves just one amendment.

The agendas examined thus far are *uniform*, by which we mean that all branches in their trees have the same length. But even uniform agendas do not exhaust the realistic types. Consider a *sequential-elimination* agenda, in which a new motion is introduced and voting proceeds to the next stage only if the current motion fails. Illustrated by Figure 4, such agendas are neither uniform nor symmetric. In Farquharson's celebrated narrative (1969), Pliny sought to discourage a sequential-elimination agenda in favor of the plurality vote. Millennia later, such agendas are observed when parliamentary bodies such as the U.S. Senate vote on personnel questions or when members are obliged to take some action (to adopt a budget, say) and so must keep voting on proposed actions until one passes.

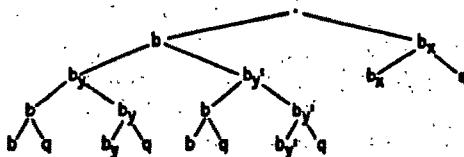
Other examples of nonuniform and hence nonsymmetric agendas include those with a succession of "killer amendments," as in Eneelow and Koehler's discussion of the Panama Canal Treaty (1980). Opponents of the treaty introduced a series of seemingly innocuous amendments solely to make the treaty's wording differ in some way from the text that had been negotiated with Panama. If any one of these motions had passed, the

Figure 4. A Sequential-Elimination Agenda



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Figure 5. A Hybrid Agenda



others would not have been introduced. We can illustrate this situation with two first-order amendments and, for good measure, a second-order amendment. The agenda, depicted in Figure 5, comprises five alternative outcomes:

- q : the status quo
- b : the bill
- b_x : the bill amended by x
- b_y : the bill amended by y in its original form
- b_{xy} : the bill amended by the amended version of y

Neither symmetric nor uniform nor continuous, this hybrid agenda suggests that our examples only begin to uncover the various forms of agendas that should interest political scientists. At any rate, they show that amendment agendas do not exhaust the possibilities.

Our ability to formulate nonamendment agendas raises the question, how prevalent are amendment agendas in practice? Although two-motion agendas can be of the sequential-elimination type, the most commonly observed two-motion agendas involve an initial motion and one amendment, and they are of the amendment variety. But consider agendas based on three motions: a bill, b , and two amendments, x and y , where x is recognized before y . If the moving of y depends on whether x passes, we cannot have an amendment agenda. So suppose y is moved regardless of the action taken on x . Then if y is a second-order amendment (Figure 6a) or a complete substitute for b (Figure 6b), the agenda is of the amend-

ment type. But if y is a first-order perfecting amendment (Figure 6c), the agenda is not an amendment agenda. Hence, an agenda based on three motions is an amendment agenda only if (1) the third motion is made and recognized regardless of whether the second passes and (2) the second motion is either a second-order amendment or a complete substitute.

First-order agendas, like that of Figure 6c, are arguably the most common in most committees. The impression, however, that amendment agendas are especially prevalent might arise from a confusion between *motions* and *alternatives*. For example, suppose there is an initial motion, to pass bill b , and two first-order perfecting amendments, x and y . If the second motion is recognized regardless of the action taken on the first, then the usual language of committee deliberations, which makes reference to motions rather than final outcomes, leads to the impression that the agenda is that of Figure 6d, an amendment agenda. However, because x and y are compatible motions rather than alternative outcomes, the correct agenda is the one shown in Figure 6c, which is not an amendment agenda.

A Classification of Agendas

In this section we formally define several important categories of agendas, most of them informally introduced in the

Figure 6. Contrasting Agendas

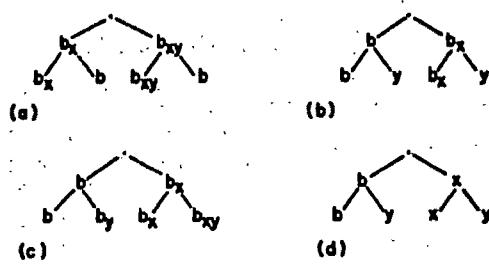
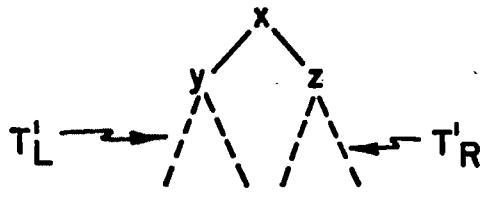


Figure 7. Agenda T'



previous section. We begin by letting X be a set of alternative outcomes, letting x, y, z, a, b , and so forth denote elements of X , and letting α, β , and so forth denote finite, nonempty subsets of X . We represent agendas by binary trees:

DEFINITION. An agenda on α is a finite binary tree T such that (1) every node (vertex) of T is occupied by some member of α , (2) every member of α occupies some bottom node of T , and (3) $y \neq z$ for every subtree T' of T of the form shown in Figure 7.

Condition (1) requires that every fork in the tree represent a choice between two alternatives and (as a mathematical convenience, which we disregard in most figures) that even the top node be occupied by some alternative (an arbitrary one will do). Condition (2) ensures that there is some path to every outcome in α , and (3) rules out "nonchoices" of the form y vs. y .

If an agenda T' has more than one node, then it has at least three subtrees: T' itself and its left and right principal subtrees, T'_L and T'_R , shown in Figure 7. Note that a principal subtree with more than one node splits into its own principal subtrees.

Now consider the two agendas in Figure 8. They are the same except for the left-right order of subtrees and alternatives at various nodes. Because they embody the same sequence of comparisons and must yield the same outcome, we call them *equivalent*. Here is a recursive definition of this relation:

DEFINITION. If the Agenda T_1 has but one node, then T_1 is equivalent to T_2 if and only if $T_1 = T_2$. If T_1 has more than one node, then T_1 is equivalent to T_2 if and only if one principal subtree of T_1 is equivalent to one principal subtree of T_2 , while the other principal subtree of T_1 is equivalent to the remaining principal subtree of T_2 .

To develop our classification of agendas, we begin with a type exemplified by the agendas in Figures 1 and 2:

DEFINITION. A symmetric agenda on α is an agenda T on α such that, for every subtree T' of T of the form shown in Figure 7, T'_L is equivalent to the result of substituting y for z in T'_R , and T'_R is equivalent to the result of substituting z for y in T'_L .

All symmetric agendas have three other important properties. First, they are nonrepetitive, a property commonly required by legislative rules. This means that they prevent an alternative once rejected from coming up again for a vote.

DEFINITION. A nonrepetitive agenda on α is an agenda T on α such that, for every subtree T' of T of the form shown in Figure 7, y does not occur in T'_R and z does not occur in T'_L .

Second, symmetric agendas on α are complete in the sense that they guarantee every alternative in α its day in court: whether an alternative gets to be voted on at a given stage does not depend on the choice made at the previous stage.

DEFINITION. A complete agenda on α is an agenda on α such that, for every subtree T' of the form shown in Figure

Figure 8. Equivalent Agendas



7, T'_L contains every alternative occurring in T_R with the possible exception of z , and T'_R contains every alternative occurring in T_L with the possible exception of y .

Figures 1 and 2 depict complete agendas; Figures 3–5, incomplete ones. Although all symmetric agendas are complete, not all complete agendas are symmetric. For example, the following agenda is complete but not symmetric: x and y are paired; if x wins, it is paired with z and the winner with w ; but if y wins, z and w are paired and the winner paired with y .

The third property that symmetric agendas satisfy is uniformity:

DEFINITION. A uniform agenda on α is one whose branches are all of the same length.

Figures 1 and 2 depict symmetric, hence uniform, agendas; Figure 3, a uniform, nonsymmetric agenda; and Figures 4 and 5, nonuniform agendas.

The following theorem (proved in the Appendix) summarizes the properties of symmetric agendas:

THEOREM 1. All symmetric agendas are nonrepetitive, complete, and uniform.

A fifth type of agenda, introduced in the previous section, does not encompass all symmetric agendas:

DEFINITION. A continuous agenda on α is an agenda T on α such that, for every subtree T' of T of the form shown in Figure 7, $x = y$ or $x = z$.

Figure 1 depicts an agenda that is symmetric and continuous; Figure 2, symmetric but not continuous; Figures 3 and 4, continuous but not symmetric; and Figure 5, neither.

A final category, included in all five preceding ones, is that of amendment agendas:

DEFINITION. An amendment agenda is

an agenda that is equivalent to $A(x_1, \dots, x_n)$ for some x_1, \dots, x_n , where $A(x_1, \dots, x_n)$ is defined by the following recursion: $A(x_1)$ is the 1-node tree consisting of x_1 . $A(x_1, x_2, \dots, x_{k+1})$ is the tree T in which x_1 occupies the top node and $T_L = A(x_1, x_3, \dots, x_{k+1})$ while $T_R = A(x_2, x_3, \dots, x_{k+1})$.

The intuition here is that an amendment agenda is determined by a sequence (x_1, \dots, x_n) such that x_1 is first pitted against x_2 . If x_1 wins, we move into the subtree T'_L , which is determined by the same sequence, less x_2 ; but if x_2 wins, we move into T'_R , which is determined by (x_2, x_3, \dots, x_n) .

Amendment agendas are nonrepetitive, complete, uniform, and continuous. All symmetric agendas have the first three properties, but

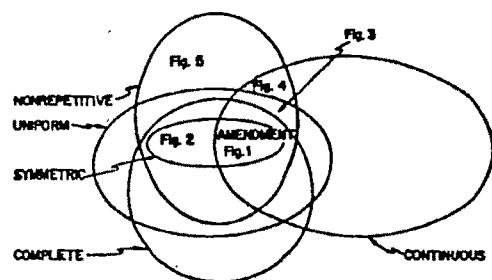
THEOREM 2. All and only amendment agendas are symmetric and continuous.

The Venn diagram in Figure 9 summarizes our classification, Theorems 1 and 2, and the easily proved fact that all complete and nonrepetitive agendas are uniform. (Although we have not supplied examples for every cell, it is easy to do so.)

Sincere and Strategic Voting

To define the choices from a given agenda under sincere and strategic voting,

Figure 9. Classification of Agendas



let P be a "social preference" relation on X . We assume that P is *asymmetric* (if $x P y$ then not $y P x$) and *connected in X* (if $x, y \in X$ and $x \neq y$ then $x P y$ or $y P x$). Under majority rule, $x P y$ if more people prefer x to y than prefer y to x . For the sake of generality, however, we do not require that P be the majority-preference relation. Our one restrictive assumption (common to most of the literature) is *connexity*, which rules out ties. Although some tie-breaking rules, such as the rule letting a chairman cast the deciding vote, can be thought of as built into P , others cannot because they depend on the positions occupied by tied alternatives in an agenda.

We can now define the *sincere* and *strategic choices*, $SIN(T)$ and $STRAT(T)$, from an arbitrary agenda T . If T contains just one alternative, x , then $SIN(T) = STRAT(T) = x$. Otherwise, let x occupy the top node of T_L and y the top node of T_R . Then

$$SIN(T) = \begin{cases} SIN(T_L) & \text{if } x P y \\ SIN(T_R) & \text{if } y P x \end{cases}$$

and

$$STRAT(T) = \begin{cases} STRAT(T_L) & \text{if } STRAT(T_L) \\ P \cdot STRAT(T_R) & \\ STRAT(T_R) & \text{otherwise.} \end{cases}$$

These recursive definitions define the sincere and strategic choices from a given complex tree in terms of choices from its less complex components. The definition of $STRAT(T)$ is equivalent to the definitions of Moulin (1979) and McKelvey and Niemi (1978).

Choice Sets and Theorems about Amendment Agendas

This section reviews what is already known about the sincere and strategic choices from amendment agendas. Since

P is connected, it is obvious that every α must contain either a unique dominant alternative or a unique dominant cycle, where

DEFINITION. x is a dominant alternative in α if and only if $x \in \alpha$ and $x P y$ for all $y \neq x$ in α . β is a dominant cycle in α if and only if, for some x_1, \dots, x_n , $\beta = \{x_1, \dots, x_n\} \subseteq \alpha$, $x_1 P x_2 p \dots P x_n P x_1$, and $x_i P y$ for all $x_i \in \beta$ and all $y \in \alpha - \beta$.

Now let

$$G(\alpha) = \{x \in \alpha \mid x \text{ is a dominant alternative in } \alpha \text{ or } x \text{ belongs to the dominant cycle in } \alpha\}.$$

Miller (1980) proves:

$$G(\alpha) = \{x \mid x = SIN(T) \text{ for some amendment agenda } T \text{ on } \alpha\}. \quad (1)$$

More generally:

$$G(\alpha) = \{x \mid x = SIN(T) \text{ for some symmetric agenda } T \text{ on } \alpha\}. \quad (2)$$

The latter result follows from a still more general result by Schwartz (1986, chap. 6), which allows ties and nonbinary choices. Thus, if a dominant alternative exists among the alternatives appearing on an amendment or other symmetric agenda, that alternative will be chosen. If, on the other hand, a dominant cycle exists, then some outcome in the cycle must be chosen, and any outcome can be chosen with some admissible agenda.

A third result follows from McKelvey's proof (1979) that for majority rule and the usual spatial context (Euclidean or, more generally, differentiable and convex preferences), if there is no undominated point, the entire space is a cycle ($x \in X$ is undominated if $y P x$ for no $y \in X$): If x and y are any two points in X , there is an amendment agenda T on some finite sub-

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set of X such that x is the first alternative in T (x occupies the top nodes of T and T_1) and $y = SIN(T)$. (Although the relation, M , of majority preference is not connected, we can interpret P as the result of adding some tie-breaking rule to M .)

For strategic voting, Miller's definitions (1980) of the covering relation and the uncovered set are crucial. Stated for an arbitrary subset Y of X ,

DEFINITION. x covers y in Y if and only if $x, y \in Y$, $x P y$, and for all $z \in Y$, if $y P z$ then $x P z$. And

$U(Y) = \{x \in Y \mid \text{nothing covers } x \in Y\}$
(the uncovered set of Y).

Miller proves:

$STRAT(T) \in U(\alpha)$ if T is an amendment agenda on α . (3)

The significance of this result derives from these properties of U :

$U(\alpha) \subseteq G(\alpha)$ and $U(\alpha) \subseteq \text{Pareto set of } \alpha$.

Since the sincere choice from an amendment agenda on α can be anything in $G(\alpha)$ —often a large subset of α that includes Pareto inefficient outcomes—and since $U(\alpha)$ and hence any strategic choice from α is necessarily in the Pareto set, strategic voting has been interpreted as a means whereby voters can limit the power of an agenda setter.

Banks (1985) shows that strategic voting can limit outcomes further. His result requires three definitions:

DEFINITION. A vector $x = (x_1, x_2, \dots, x_n)$ is a chain in α if and only if $x_i \in \alpha$ for all i and $x_i P x_j$ whenever $i < j$. A maximal chain in α is a chain x in α such that there is no longer chain in α containing every member of x . And

$B(\alpha) = \{x \mid x = \text{first element of some maximal chain in } \alpha\}$.

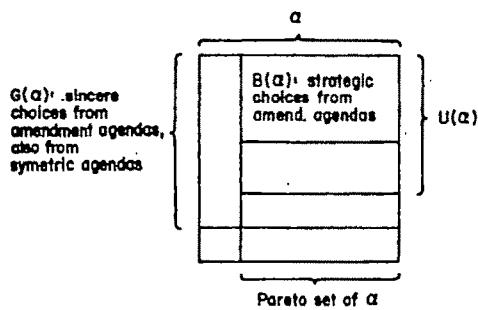
Banks proves:

$$B(\alpha) = \{x \mid x = STRAT(T) \text{ for some amendment agenda } T \text{ on } \alpha\} \subseteq U(\alpha). \quad (4)$$

We summarize results (1)–(4) in Figure 10. Notice that these results relate the final outcome only to the other alternatives on the agenda: they say nothing about alternatives in X but outside α . Hence, they do not say whether McKelvey's theorem holds for strategic voting—whether agendas can “lead anywhere” in a Euclidean space when voters act strategically. Shepsle and Weingast (1984), however, extend (3) for simple majority rule in the spatial context to show that if x_1 is the first alternative in an amendment agenda T on α and if x_j is chosen over x_k whenever $j > k$ and x_j and x_k tie, then x_1 does not cover $STRAT(T)$ in X . That is, the only points that can be reached, under strategic voting and an amendment agenda, from some initial alternative x_1 are points that x_1 does not cover. McKelvey (1986), moreover, offers a model of endogenous amendment-agenda formation in which outcomes outside the uncovered set of the entire space cannot be reached. Strategic voting, then, appears to limit the power of an agenda setter regardless of whether our focus is on a fixed, finite set of alternatives that must enter the agenda or an entire issue space and hence regardless of whether the agenda setter can control only the order of voting or the set of feasible alternatives as well. We say “appears” because the assumption that all permissible agendas are of the amendment type is essential to these results.

Were the set of feasible agendas restricted to amendment agendas, then strategic voting also would limit the length of agendas required to secure reachable outcomes. With sincere voting, if an alternative y is required to enter the voting in the first stage of an amendment agenda T on α , then there exists an amendment agenda on part or all of α by which any given outcome $x \in G(\alpha)$ can be

Figure 10. Summary of Amendment-Agenda Results



reached. With strategic voting, on the other hand, Miller (1980) shows that if $x \in G(\alpha)$ can be reached at all, then there exists a one- or two-stage amendment agenda on a subset of α that begins with y and yields x . Shepsle and Weingast (1984) establish a parallel result for spatial preferences. They show that all the outcomes that can be reached from x_1 (namely, the outcomes that do not cover x_1) can be reached by some one- or two-stage agenda (which may, however, contain an outcome foreign to the original agenda).

Counterexamples to Extensions

What we want to show now is that the results we reviewed in the previous section hold only for institutions in which the feasible agendas are limited to amendment agendas. That is, the apparent limits that strategic voting sets on a hypothetical agenda setter's power depend critically on the set of permitted agendas, and if we properly extend a setter's strategies to include all agendas that we might actually observe in a legislature, strategic voting is not the constraint it was thought to be.

Example 1. To see that things can be different when we turn from amendment agendas to other agenda forms, we note first that *agendas can be found from*

which the sincere choice, $SIN(T)$, need not belong even to $G(\alpha)$. Suppose every voter ranks the five alternatives shown in Figure 3 as follows: b_y, b_{xy}, b_x, b, q . Since this order is unanimous, we may suppose that P orders the alternatives the same way. Hence, if everyone votes sincerely in the agenda—call it T —of Figure 3, then b_x defeats b , after which b_{xy} defeats b_x and then q , so $SIN(T) = b_{xy}$. But b_y is the dominant alternative, so $SIN(T) \notin G(\alpha) = \{b_y\}$.

Example 2. Although Example 1 shows that outcomes outside $G(\alpha)$ can prevail under sincere voting, notice that strategic voting in this example yields b_y , which, because it is dominant, is the sole element of $U(\alpha)$ and $B(\alpha)$. But suppose the following eight alternatives are on the floor of the U.S. House:

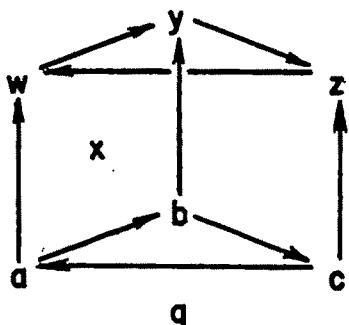
- w: an unamended bill
- x: the bill perfected by an amendment
- y: the bill perfected by an amended amendment
- z: the bill perfected by a substitute amendment
- a: a substitute bill unchanged
- b: a substitute for the substitute bill
- c: the substitute substitute perfected by an amendment
- q: the status quo

Then the agenda is as follows (Sullivan 1984):

1. x vs. y (whether to amend the amendment)
2. the winner at 1 vs. z (whether to substitute for the [perfected] amendment)
3. the winner at 2 vs. w (whether to amend the bill)
4. b vs. c (whether to amend the substitute substitute bill)
5. the winner at 4 vs. a (whether to substitute for the [perfected] substitute bill)
6. the winner at 5 vs. the winner at 3

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Figure 11. Social Preference for Example 2



- (whether to replace the [perfected] bill by the surviving substitute bill)
7. the winner at 6 vs. q (whether to pass the bill's surviving form)

Suppose the social preference under majority rule takes the form shown in Figure 11, where, for those alternatives not paired by an arrow, their height on the page represents P . Because the agenda tree is especially complex, we do not reproduce it here, but note that $STRAT(T) = x$ even though $x \notin B(\alpha)$. That is, chains with x as the first element include (x) , (x, b) , (x, b, c) , (x, b, c, q) , and so forth, but none of these chains is maximal since we can set w , z , or y at the top of any of them and still have a chain. Thus, whereas an amendment agenda over the alternatives in this example must yield a point in $B(\alpha)$ with strategic voting, the agenda prescribed by Rule XIV yields an outcome outside $B(\alpha)$. This shows that Banks's theorem about amendment agendas does not extend to other agenda forms, even realistic symmetric ones.

Example 3. Although in Example 2, $x = STRAT(T)$ is not in $B(\alpha)$, x is in the uncovered set, $U(\alpha)$: there is no alternative that defeats x while also defeating everything x defeats. Nevertheless, we can construct a counterexample to the proposition that the strategic choice from a

realistic agenda—again a symmetric agenda permitted by Rule XIV—must belong to the uncovered set. There are seven alternatives:

- c: an unamended bill
- a: the bill perfected by an amendment
- b: the bill perfected by a substitute amendment
- x: a substitute bill unamended
- y: the substitute bill perfected by an amendment
- z: the substitute bill perfected by a substitute amendment
- q: the status quo

Agendas with both a perfecting amendment to a bill and a substitute bill may be rare in the House. Bach (1981), however, illustrates a similar agenda with the case of Senate Bill 7, the Veterans' Health Care Amendments of 1979. The required congressional agenda is the following:

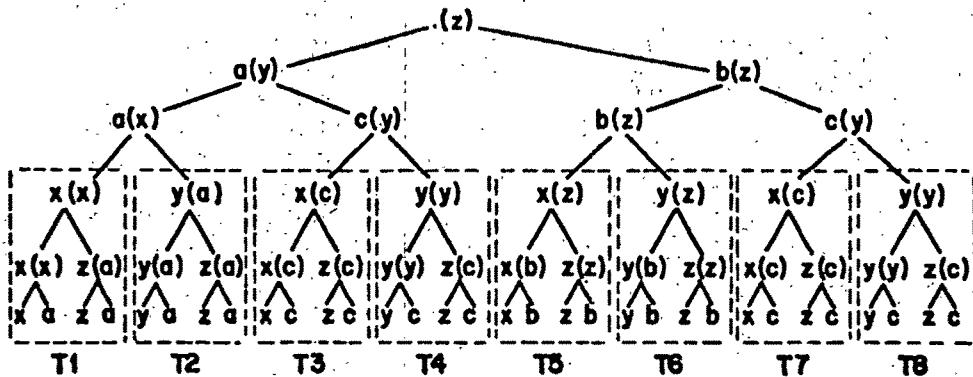
1. a vs. b (whether to replace the amendment with a substitute amendment)
2. winner at 1 vs. c (whether to amend the bill)
3. x vs. y (whether to amend the substitute bill)
4. winner at 3 vs. z (whether to replace the [perfected] substitute by the substitute to the substitute)
5. winner at 2 vs. winner at 4 (whether to replace the [perfected] bill by the surviving substitute)
6. winner at 5 vs. q (whether to pass the surviving motion)

Suppose a three-member committee holds the following preference orders:

- Member 1: $a c z b y x q$
- Member 2: $b y c x a z q$
- Member 3: $x z a y c b q$

The majority-preference relation is as follows, where, aside from the exceptions noted by arrows, the left-to-right order of

Figure 12. Agenda for Example 3



the motions corresponds to the majority preference:

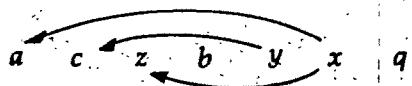


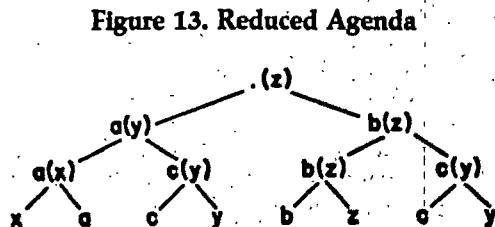
Figure 12 portrays the agenda and, in parentheses, shows the strategic choice from each subtree (to save space we omit q from the tree since it is defeated by everything else). So $STRAT(T) = z$. But $z \notin U(\alpha)$: a covers z because a beats z and also beats the three alternatives (b , y , and q) that z beats.

To see why a covered alternative is chosen, consider the eight subtrees, T_1-T_8 , in Figure 12. Each is equivalent to a mini-amendment agenda (e.g., in T_1 , x is paired with z , then the winner with a), so from result (3), the strategic choice from each is uncovered in that subtree.

Since no information about outcomes is lost if we replace a subtree by its strategic choice, we redrew the agenda in Figure 13 to extend each branch down just to where subtrees become amendment agendas (for T_5 we substitute b for z since z beats b and will beat b in the corresponding comparison with the strategic choice from T_6).

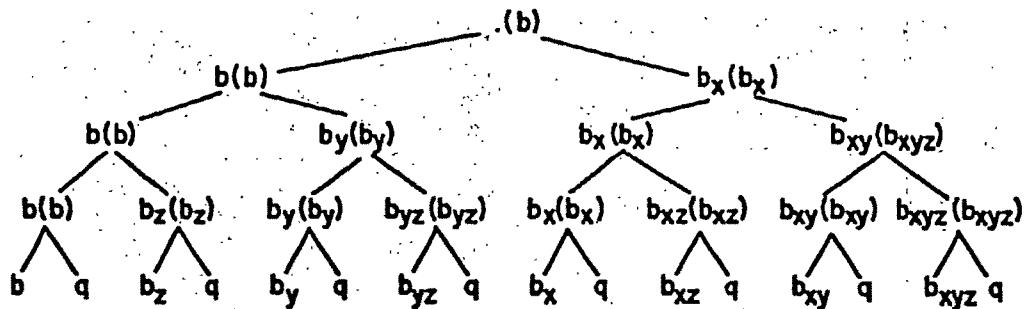
This tree is almost an amendment agenda: after a and b are paired, the winner is paired with c , and the winner in that context with y . The important exceptions are that if a wins in the first two rounds, x and a are paired (rather than a and y), whereas if b wins in those rounds, b is paired with z (rather than y). Thus, x can defeat a and then lose to y without ever being compared to z . The discontinuities permit this asymmetry in the treatment of z and a and account for the survival of a covered alternative. Although the original tree is symmetric, the strategically equivalent reduced tree, owing to the discontinuity, is not symmetric.

Example 4. It would be a mistake to infer from the preceding examples that symmetry or discontinuity is essential to agendas in which strategic voting leads outside the uncovered set. Consider an agenda involving three compatible first-order amendments, x , y , and z . Suppose P



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Figure 14. Agenda for Example 4



orders the nine alternative outcomes, from left to right, thus:

$$b_{yz} \ b_{xy} \ b \ b_x \ b_y \ b_z \ b_{xz} \ b_{yz} \ q$$

with the exception that $b_x P b_{xyz}$. Then b_{xy} covers b . Nevertheless, the appropriate agenda, depicted in Figure 14 with strategic equivalents in parentheses, yields b as the final outcome. Although not symmetric, this agenda is continuous, uniform, and nonrepetitive.

These examples, then, establish the amendment-agenda assumption as essential to Results (1)-(4). They establish the same for the three other propositions that we discuss in the previous. First, *strategic choices need not be Pareto-efficient*. In Example 3, the strategic choice, z , is covered but Pareto efficient. Now switch a and z in Member 3's preference ordering. Then P remains the same, but z becomes Pareto inefficient.

Second, *the first alternative on an agenda can cover the final strategic choice*. In Example 3, a is voted on first, and a covers z , yet z is the outcome under strategic voting.

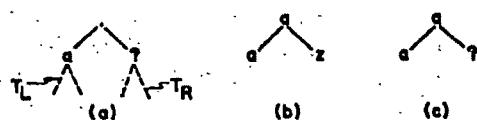
Finally, *not every agenda can be reduced to a < two-step agenda with the same first alternative and the same strategic choice*. Looking again at Example 3, notice that every one- or two-stage agenda T that begins with a and yields z as the strategic choice must have the form

shown in Figure 15a. Since $z = STRAT(T)$, z must also equal $STRAT(T_L)$ or $STRAT(T_R)$. If $z = STRAT(T_L)$, then T_L must have the form shown in Figure 15b since, with two or fewer steps, this must be the last step and only a and z can be paired. But a beats z , so $z \neq STRAT(T_L)$. Suppose, then, that $z = STRAT(T_R)$. Then T_R must have the form shown in Figure 15c, where $?$ is an alternative that beats a but loses to z (so that z survives as the final outcome). But the only alternative that defeats a is x , which z does not defeat. Thus, the agenda in Figure 14 cannot be reduced to one or two stages with alternative a entering the first stage and z being the final outcome.

Theorems

The preceding examples demonstrate that none of the earlier results specifying limits on a hypothetical agenda setter's power under strategic voting applies when that agenda setter's strategy set contains all agendas permitted in real legislatures.

Figure 15. Two-Step Reduction of Example 3



latitudes, such as the U.S. Congress. The results we prove in this section show how wide the true limits are, both for strategic and sincere voting. When compared with the theorems reviewed earlier, these results reveal exactly how special a case the amendment agenda really is. We begin with an unsurprising result:

THEOREM 3. *If T is any agenda on α , then $STRAT(T) \in G(\alpha)$.*

Because $U(\alpha)$ and $B(\alpha)$ are subsets of $G(\alpha)$, this theorem does not exclude the possibility that an agenda setter's power is restricted by strategic voting to these particular subsets of $G(\alpha)$. Strategic voting does restrict a setter's power, but not in the way we might think. First, consider the "kitchen sink" set:

$$K(\alpha) = \{x \in \alpha \mid \alpha = \{x\} \text{ or } x P y \text{ for some } y \in \alpha\}.$$

Thus, x is in $K(\alpha)$ so long as α consists of x alone or x defeats something or other in α . Clearly, $G(\alpha) \subseteq K(\alpha)$, and $K(\alpha)$ comprises all but at most one member of α . Not surprisingly:

THEOREM 4. *If T is an agenda on α , $SIN(T) \in K(\alpha)$.*

Like Theorem 3, this result places weak bounds on final outcomes. What is interesting, however, is that *these bounds are the strongest there are*: the G and K sets are the exact limits of an agenda setter's power under strategic and sincere voting, respectively.

To see what we mean, consider a sequential-elimination agenda, the general form of which is that of Figure 4, but with any number of nodes. Our next theorem says that *any* outcome in $K(\alpha)$ —which is all of α less at most one member—can be the sincere choice from some nonrepetitive sequential-elimination agenda, hence from some nonrepetitive continuous agenda:

THEOREM 5. *If $x \in K(\alpha)$, then for some nonrepetitive sequential-elimination agenda T on α , $x = SIN(T)$.*

When the set of feasible agendas is restricted to the amendment agendas, or even the symmetric ones, sincere voting cannot lead outside the G -set, but as soon as the feasible agendas are allowed to include even nonrepetitive sequential-elimination agendas (hence, more generally, continuous agendas), sincere voting can lead practically anywhere.

We can now see one weak limit, however, that strategic voting places on an agenda setter's power: although large, $G(\alpha)$ is often a proper subset of $K(\alpha)$. But because $G(\alpha)$ might be large and some of its elements of Pareto inefficient, we want to know whether strategic voting places more severe limits on an agenda setter's power. The next five results show that tighter limits do not exist except in special cases.

THEOREM 6. *If $x \in G(\alpha)$, there exists a nonrepetitive sequential-elimination agenda T on α such that $STRAT(T) = x$. (Moulin 1985)*

Figure 16 summarizes the Theorems 3–6.

If we drop the requirement of nonrepetitiveness in Theorem 6, we can ensure that T begins with any given alternative and yields any other given alternative as the strategic choice:

THEOREM 7. *If $x \in G(\alpha)$, then there exists a sequential-elimination agenda T on α such that y is the first alternative in T and $x = STRAT(T)$.*

McKelvey's theorem about agendas leading anywhere under sincere voting can be restated for strategic voting if, instead of amendment agendas, we use sequential-elimination agendas:

THEOREM 8: *For the "standard spatial model," if X is the issue space, if the majority-preference relation M is a*

subrelation of P , and if no dominant point in X exists, then for any two points $x, y \in X$, there is a nonrepetitive sequential-elimination agenda T on some finite subset of X such that (1) x is the first alternative in T , (2) $y = STRAT(T)$, and (3) $STRAT(T'_L) \neq MSTRAT(T'_R)$ or conversely for every > 2 -node subtree T' of T .

Since P is connected but M is not, we might think of P as the result of extending M by the addition of some tie-breaking rule. Clause (3) says that no M -ties actually arise in T . The essential point of this theorem, though, is that it is not strategic voting per se that limits choices to the uncovered set or the Banks set but the assumption that an agenda setter must choose agendas of the amendment type.

Theorem 6 tells us that if all voters are strategic and if all binary agendas are admissible, then there exists some binary agenda on α that will choose any outcome in $G(\alpha)$, whereas from the results reviewed in the Choice Sets and Theorems section, we know that if agendas are limited to the amendment type, then only outcomes in $B(\alpha) \subseteq G(\alpha)$ can prevail. It is interesting, however, to consider separately the class of symmetric agendas since one-period agendas often are required to be symmetric (e.g., by House Rule XIV). Of course, our examples from the previous section tell us that such agendas can lead outside the Banks set (Example 2), outside the uncovered set (Examples 3 and 4), and outside the Pareto set (Example 3 with modified preferences for Member 3). Often, though, the rule assigned to a bill in the House limits the number of alternatives that can be voted on, whence the importance of the following theorem:

THEOREM 9. *If α has 5 or fewer members, then for any symmetric agenda T on α , $STRAT(T) \in U(\alpha)$.*

Thanks to Example 3, the five-

alternative limit cannot be raised. Hence, earlier results do extend to certain non-amendment agendas if the number of alternatives is severely constrained: even under strategic voting, the power of an agenda setter is no greater with a strategy set comprising all symmetric agendas than with a strategy set limited to amendment agendas if the alternatives that can be included in the agenda are five or fewer. But what if they exceed five? Can just any outcome in $G(\alpha)$ be achieved with a symmetric agenda, as it can with a sequential-elimination agenda, or are final outcomes limited to a set lying "between" $U(\alpha)$ and $G(\alpha)$? Here is a partial answer:

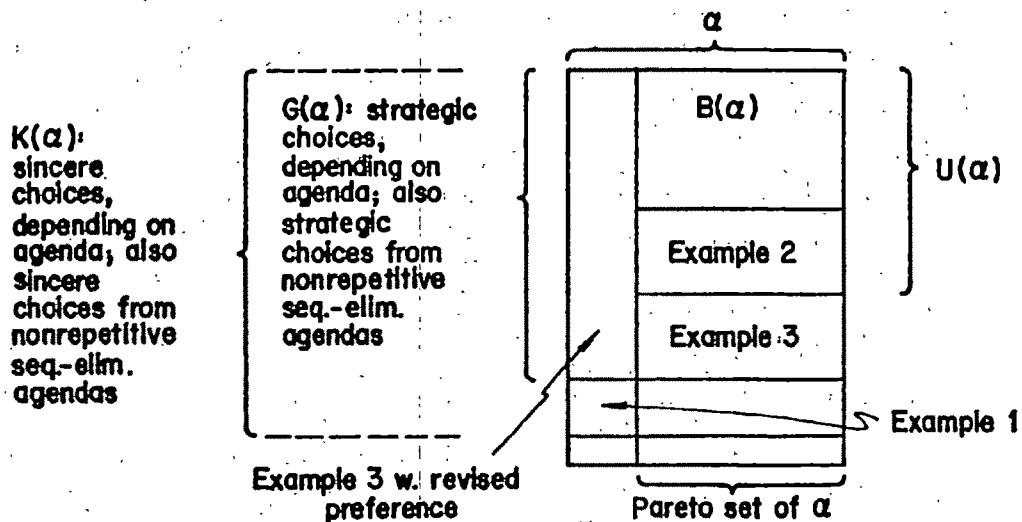
THEOREM 10. *Suppose T is a symmetric agenda on α , α has at least 4 members, and x is covered in α by all but 2 other members of α . Then $x \neq STRAT(T)$.*

Notice that if $|\alpha| \geq 4$ and if $x \in G(\alpha)$, then there must be at least two other members of α that do not cover x , whereas everything else in α can cover x (see the construction used to prove the theorem). Given the assumptions of Theorem 10, although $x \in G(\alpha)$, x cannot be chosen from any symmetric agenda if voters are strategic. Thus, it appears that the set of reachable outcomes when only symmetric agendas are feasible lies somewhere between what is reachable if only amendment agendas are feasible and what is reachable if all binary agendas are feasible.

Conclusions

It is tempting to interpret our analysis as supporting the idea that political outcomes are at the mercy of elites who control agendas. Certainly, our analysis supports the proposition that strategic voting is not always effective for thwarting the intent of an unconstrained agenda setter, but it is naive to interpret our results as implying a reemergence of the power of agenda setters, hypothetical or real. For

Figure 16. Summary of General Agenda Results



one thing, the set of feasible legislative agendas, even with a handful of motions, may be large. And Hammond's (1986) adaptation of agendas to the study of hierarchical decision processes suggests that the set of feasible organizational agendas may be larger still. Hence, power may lie more with those who are best able to ascertain the implications of particular agendas than with those who control procedures and agenda form.

Second, in addition to the formal constraints imposed by such measures as Rule XIV, setters are also subject to those who would upset their calculations by the introduction of new issues and dimensions (Riker 1982). And as the experiences of Plott and Levine (1976) attest, even those unschooled in agenda manipulation soon learn the importance of agenda control and seek the means to thwart that control. Third, our analysis assumes that all preferences are common knowledge. But as Ordeshook and Palfrey (1986) demonstrate, outcomes can be radically different with incomplete information. Although their analysis is limited to amendment agendas, it shows how agen-

das reveal different things about preferences, suggesting that a setter can manipulate the agenda to reveal preferences in particular ways.

We also should consider the ways in which motions are introduced for consideration. With the exception of Theorem 6, which concerns spatial majority-rule games, our analysis focuses on the manipulation of outcomes by deciding how some fixed set of alternatives is considered and disposed of. But agenda power has two sources: the ability to choose a procedure for selecting from some fixed set of alternatives and the ability to choose that set. And we suspect that agenda power, if it exists at all in Congress, has more to do with the second source than the first. The labeling of alternatives as *the bill*, *an amendment*, or *a substitute amendment* is an important strategic process, but once all motions are made and the labels attached, there may be little room for strategic maneuver.

The relevance of agendas to the study of final outcomes, then, may depend on the rules that determine the alternatives that a legislature considers. Here, how-

ever, the possible endogeneity of the process complicates matters. Some progress has been made in the study of endogenous agenda formation (McKelvey 1986), but again under the assumption that all agendas are of the amendment type. The variety of congressional agendas and the limits that might otherwise be placed on the types of agendas that a legislature can use raise new research possibilities. The labeling of motions may be an important strategy available to members because the decision about how to label a motion has more profound consequences than earlier research indicates. The sole strategic consideration in an amendment agenda is the order of the alternatives. But if, for example, two proposed amendments to a committee report are both moved and recognized as perfecting amendments rather than as a perfecting amendment and a substitute bill, the prescribed agenda must be nonsymmetric (as in Figure 2).

Hence, instead of proclaiming the pre-eminence of the agenda setter—hypothetical or otherwise—our analysis warns us that our subject concerns games in which participants' strategies include more than simply deciding the order in which to present motions or whether to vote sincerely on the current ballot. We are only beginning to identify and understand those strategies, but this essay sheds some light on the potential consequences of part of the strategy space of those who participate in the institutions of social choice.

Appendix

Proof of Theorem 1. Because symmetric agendas are obviously complete and uniform, we prove only that they are non-repetitive. If T on α is symmetric and repetitive, then some subtree of T is equivalent to a tree T' in which some $x \in \alpha$ occupies the top node of T'_L , some $y \in \alpha$ occupies the top node of T'_R , but x occurs

in T'_R , say at node n . By symmetry, T'_L is the same as T'_R but for the substitution of x for y . So x must occur at node n in T'_L as well. But because we can get T'_R from T'_L by substituting y for x , y must occur at node n in T'_R , which is impossible since x occurs there. Q.E.D.

Proof of Theorem 2. We first prove, by induction on n , the lemma that if $T = A(x_1, \dots, x_n)$ is an amendment agenda, then (x_1, \dots, x_n) contains no repetitions. This is trivially true if $n = 1$. Otherwise, $T_L = A(x_1, x_3, \dots, x_n)$ and $T_R = A(x_2, x_3, \dots, x_n)$ are agendas. By inductive hypothesis, (x_1, x_3, \dots, x_n) and (x_2, x_3, \dots, x_n) contain no repetitions, so if (x_1, \dots, x_n) contained a repetition, we would have $x_1 = x_2$, in which case $x_1 (= x_2)$ would occupy the top nodes of both T_L and T_R , contrary to the definition of an agenda.

We prove the theorem itself by induction on the number of nodes of T , which we assume exceeds 1 (else the theorem is trivially true). Let T be an amendment agenda, so that T is equivalent to $T' = A(x_1, \dots, x_n)$ for some x_1, \dots, x_n ($n > 2$). Then $T'_L = A(x_1, x_3, \dots, x_n)$ and $T'_R = A(x_2, x_3, \dots, x_n)$, which are continuous and symmetric by inductive hypothesis. Therefore, since x_1 occupies the top node of T' and T'_L while x_2 occupies the top node of T'_R , T' is continuous. But since, by the lemma, (x_1, x_3, \dots, x_n) and (x_2, x_3, \dots, x_n) contain no repetitions, T'_L is the result of substituting x_1 for x_2 in T'_R , while T'_R is the result of substituting x_2 for x_1 in T'_L . Hence, owing to the symmetry of T'_L and T'_R , T' is symmetric.

Conversely, suppose T is continuous and symmetric. Then T is equivalent to an agenda T' in which, for some x and y , x occupies the top nodes of T' and T'_L while y occupies the top node of T'_R . By inductive hypothesis, T'_L is equivalent to $A(x, x_1, \dots, x_n)$ and T'_R to $A(y, y_1, \dots, y_m)$ for some x_1, \dots, x_n

and y_1, \dots, y_m ($n \geq 0, m \geq 0$). But by symmetry, $A(y, y_1, \dots, y_m)$ is the result of substituting y for x in $A(x, x_1, \dots, x_n)$, and by the lemma, (y, y_1, \dots, y_m) and (x, x_1, \dots, x_n) have no repetitions. So $A(y, y_1, \dots, y_m) = A(x, x_1, \dots, x_n)$. Hence, T' is equivalent to $A(x, y, x_1, \dots, x_n)$. Q.E.D.

Proof of Theorem 3. By induction on the number of nodes of T . Trivial if T has one node. Otherwise, let α_L be the set of alternatives in T_L , let α_R be the set of alternatives in T_R , and let $\alpha = \alpha_L \cup \alpha_R$. By inductive hypothesis,

$$STRAT(T_L) \in G(\alpha_L) \quad (A1)$$

$$STRAT(T_R) \in G(\alpha_R) \quad (A2)$$

Without loss of generality, suppose that

$$STRAT(T) = STRAT(T_L) \quad (A3)$$

From the definition of $G(\alpha)$, everything in $G(\alpha)$ bears P to everything in $\alpha - G(\alpha)$, while everything in $G(\alpha_L)$ bears P to everything in $\alpha_L - G(\alpha_L)$. So if there existed a $w \in G(\alpha_L) - G(\alpha)$ and a $v \in G(\alpha) \cap \alpha_L$, we would have both wPv and vPw , contrary to the asymmetry of P . Hence, either $G(\alpha_L) - G(\alpha)$ or $G(\alpha) \cap \alpha_L$ is empty. That is, either $G(\alpha_L) \subseteq G(\alpha)$ or $G(\alpha) \cap \alpha_L = \emptyset$. Similarly, either $G(\alpha_R) \subseteq G(\alpha)$ or $G(\alpha) \cap \alpha_R = \emptyset$. Consequently, if $G(\alpha_L) \not\subseteq G(\alpha)$, then $G(\alpha) \cap \alpha_L = \emptyset$, so that $G(\alpha) \cap \alpha_R \neq \emptyset$ (since otherwise $G(\alpha) = G(\alpha_L \cup \alpha_R)$ would be empty), whence $G(\alpha_R) \subseteq G(\alpha)$, and thus, by (A1) and (A2), $STRAT(T_R) \in G(\alpha)$ (because $G(\alpha) \cap \alpha_L = \emptyset$), but $STRAT(T_L) \notin G(\alpha)$, which implies that $STRAT(T_R) P STRAT(T_L)$, contrary to (A3). Hence, $G(\alpha_L) \subseteq G(\alpha)$ after all, and so by (1) and (3), $STRAT(T) \in G(\alpha)$. Q.E.D.

Proof of Theorem 4. By induction on $|\alpha|$. Trivial if $|\alpha| = 1$. Otherwise, let y occupy the top node of T_L and z the top node of T_R . Without loss of generality, suppose $y P z$. Then $SIN(T) = SIN(T_L)$. If T_L con-

sists just of y , then $SIN(T) = SIN(T_L) = y P z$, so $SIN(T) \in K(\alpha)$. On the other hand, if T_L has more than one node, then by inductive hypothesis, $SIN(T) = SIN(T_L) P w$ for some $w \in \alpha$, and so $SIN(T) \in K(\alpha)$. Q.E.D.

Our next results require a formal definition of sequential-elimination agendas.

A *sequential-elimination agenda* is an agenda that is equivalent to $S(x_1, \dots, x_n)$ for some x_1, \dots, x_n , where $S(x_1, \dots, x_n)$ is defined thus: $S(x_1)$ is the 1-node tree occupied by x_1 ; $S(x_1, \dots, x_n)$ is the binary tree T in which x_n occupies the top node, T_R is the 1-node tree occupied by x_1 , and $T_L = S(x_2, \dots, x_n)$.

Proof of Theorem 5. Trivial if $\alpha = \{x\}$. Otherwise, there exists a $y \in \alpha$ for which $x P y$. So there exist y_1, \dots, y_n such that (1) $\alpha - \{x\} = \{y_1, \dots, y_n\}$, (2) $y_i \neq y_j$ unless $i = j$, and (3) $y_1 = y$. Then for the nonrépetitive sequential-elimination agenda $T = S(x, y_1, \dots, y_n)$, $SIN(T) = SIN(T_R) = x$, because $x P y_1 = y$. Q.E.D.

Two definitions and a lemma. Before proceeding to the remaining theorems we need the following:

DEFINITION. A path on α is a vector (x_1, x_2, \dots, x_n) such that $\alpha = \{x_1, x_2, \dots, x_n\}$ and for each $x_i \neq x_1$, $x_j P x_i$ for some $j < i$. It is nonrépetitive if $x_i \neq x_j$ whenever $i \neq j$.

LEMMA. If $x = (x_1, x_2, \dots, x_n)$ is a path on α , then there exists a sequential-elimination agenda T on α such that (1) $STRAT(T) = x_1$, (2) T begins with x_n , and (3) T is nonrépetitive if x is nonrépetitive.

Proof. Trivial if $n = 1$. Otherwise, let $T = S(x_1, \dots, x_n)$. Then T begins with x_n and is nonrépetitive if x is. For $i = 1, 2, \dots, n-1$, let T_i be the subtree of T in which the first vote is between x_n and x_i . Then it suffices to show, by induction

on $n-i$, that for every $i < n$, either $STRAT(T_i) = x_i$ (so that, in particular, $STRAT(T_1) = x_1$), or $STRAT(T_i) = x_1$, or $x_j P STRAT(T_i)$ for some $j = 1, 2, \dots, i-1$. If $x_n P x_{n-1}$, then $STRAT(T_{n-1}) = x_n$ and, since x is a path, either $n = 1$ or $x_j P STRAT(T_{n-1}) = x_n$ for some $j = 1, 2, \dots, n-2$. Otherwise, $x_{n-1} P x_n$, so $STRAT(T_{n-1}) = x_{n-1}$. Now suppose that $i < n-1$. If $x_j P STRAT(T_{i-1})$, then $x_o = STRAT(T_i)$. On the other hand, suppose $STRAT(T_{i+1}) P x_i$. Then $STRAT(T_i) = STRAT(T_{i+1})$. But by inductive hypothesis, either $STRAT(T_{i+1}) = x_1$, or $x_j P STRAT(T_{i+1})$ for some $j < i+1$, or else $STRAT(T_{i+1}) = x_{i+1}$, in which case, since x is a path, we again have $x_{i+1} = x_1$ or $x_j P x_{i+1} = STRAT(T_{i+1})$ for some $j < i+1$. Hence, since not $x_j P STRAT(T_{i+1})$, there must exist a $j < i$ such that $x_j P STRAT(T_{i+1}) = STRAT(T_i)$ unless $STRAT(T_i) = STRAT(T_{i+1}) = x_1$. Q.E.D.

Proof of Theorem 6. Trivial if $\alpha = \{x\}$. Otherwise, if $\alpha - G(\alpha) \neq \emptyset$, let $\alpha - G(\alpha) = \{y_1, \dots, y_k\}$ where $y_i \neq y_j$ whenever $j \neq i$. We have two cases:

Case 1. $G(\alpha) = \{x\}$. Then $x P y_i$ for all i . So (x, y_1, \dots, y_k) is a nonrepetitive path on α , and the theorem follows from the lemma.

Case 2. $G(\alpha)$ is a dominant cycle in α . Then there exist x_1, \dots, x_m such that (1) $G(\alpha) = \{x_1, \dots, x_m\}$; (2) $x_1 = x$; (3) $x_1 P x_2 P \dots P x_m P x_1$; and (4) $x_i P y_j$, $j = 1, 2, \dots, k$; $i = 1, 2, \dots, m$. Let (x'_1, \dots, x'_r) be the result of deleting from (x_1, \dots, x_n) all but the first occurrence of each x_i that occurs more than once. Then $x'_1 = x$, and $(x'_1, \dots, x'_r, y_1, \dots, y_k)$ is a nonrepetitive path on α , whence the theorem follows by the lemma. Q.E.D.

Proof of Theorem 7. Trivial if α consists of a single alternative. Otherwise, if $\alpha - G(\alpha) \neq \emptyset$, let $\alpha - G(\alpha) = \{z_1, \dots, z_k\}$. We now have the same two cases to consider as before.

Case 1. $G(\alpha) = \{x\}$. Then $x P z_i$ for all

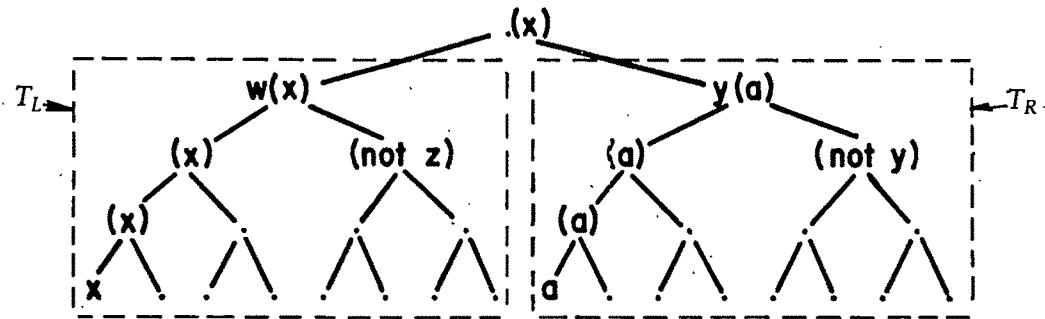
i , and either $x = y$ or $x P y$. So (x, z_1, \dots, z_k, y) is a path, and the theorem follows by the lemma.

Case 2. $G(\alpha)$ is a dominant cycle in α . Then the same four conditions hold as in the proof of Theorem 6 except that the last condition (4) reads: $x_i P z_j$ for all i and j . Consequently, either $y = x = x_1$ or $y = x_j$ for some $i > 1$ or $y = z_j$ for some j . So $(x_1, \dots, x_m, z_1, \dots, z_k, y)$ is a path in which $x_1 = x$, and the theorem follows from the lemma. Q.E.D.

Proof of Theorem 8. There exist x_1, \dots, x_n such that (1) $x_1 = x$, (2) $x_n = y$, and (3) $x_1 P x_2 P \dots P x_n$, where P is a connected extension of the majority-preference relation (McKelvey 1979). Since (x_1, \dots, x_n) is a path, the theorem follows from the lemma. Q.E.D.

Proof of Theorem 9. Since q is not critical to Example 3 in Section 5, as few as six alternatives are required to generate a symmetric agenda that leads outside of $U(\alpha)$. To see that we cannot construct an example with fewer alternatives, let some alternative x be covered by y (so $y P x$). Then for x to prevail, we must eliminate y , say with z . But if $z P y$, then $z P x$ (since anything that defeats y defeats x), so z also must be eliminated by an alternative, say w , for which $x P w$. Otherwise, if $\alpha = \{x, y, z\}$ or if $\alpha = \{x, y, z, w\}$ with $w P x$, then $x \notin G(\alpha)$ and x cannot prevail. So we must have $\alpha = \{x, y, z, w\}$ with $x P w$ (and since y covers x , with $y P w$). But even this is not enough. For x to be the outcome, at least one principal subtree, say T_L , must have $x = STRAT(T_L)$, and x or w must equal $STRAT(T_R)$. If T is symmetric, then T_L must look like an amendment agenda over three members of α , and the only such agenda that yields x is " z vs. x , the winner against w ." So the first ballot of T involves y , which enters T_R , and T_R yields w only if it is the agenda " y vs. w , the winner against z ." But if the first node of T_L pits z against x and if the first node of T_R pits y against w , then T is

Figure A-1. Agenda Beginning with w and y



not symmetric. This shows that if x prevails, then α cannot have four or fewer elements.

Now let $\alpha = \{a, x, y, z, w\}$ and, as before, let $x = \text{STRAT}(T_L)$ and let a or w equal $\text{STRAT}(T_R)$. Since $\text{STRAT}(T_L) = x$, then, from the previous argument that no covered alternative can prevail in any four-alternative symmetric agenda, y cannot enter T_L . Thus, y must be voted on in the first ballot of T and enter T_R . But x cannot be paired with y in this first ballot. Since y must be defeated by something in T_R for x to prevail in T , pairing x and y first means that T_L is identical to T_R except for the interchange of x and y , in which case, whatever defeats y also defeats x . Hence, we have three cases, depending on what, besides y , is voted on in the first ballot.

Case 1. If the first ballot pairs y and w , then the agenda must look like the one shown in Figure A-1, where the outcomes in parentheses denote strategic choices. Alternative a must be the strategic choice of T_R since it is the only alternative in this subtree that does not necessarily defeat x . But if $x P a$, then $y P a$, in which case we must have $a P z$, else $a \notin G(\{x, y, z, a\})$ and a would not equal $\text{STRAT}(T_R)$ as assumed. If, without loss of generality, $a = \text{STRAT}(T_{R_L})$ as indicated, then T_{R_L} must correspond to the agenda " y vs. a , winner against z " or to " x vs. a , winner against

z ." But if z enters the agenda only in the last ballot, it must do so also in T_L , owing to the symmetry of T , and since $z P x$, x cannot rise in T_L to become $\text{STRAT}(T_L)$.

Case 2. If the first ballot pairs a and y , then the agenda looks like the preceding picture except that w and a reverse roles. But by the same argument, $w = \text{STRAT}(T_R)$ only if the left subtree of T_R is the agenda " y vs. w , the winner against z " or " x vs. w , the winner against z ," so again z enters the agenda only in the last ballot and $x \neq \text{STRAT}(T_L)$.

Case 3. If the first ballot pairs z and y , then we must have $w = \text{STRAT}(T_R)$, since, to ensure that $y \neq \text{STRAT}(T_R)$, we must have $a P y$ and hence $a P x$. Also, to ensure that $w \in G(\{a, x, y, z\})$, we must have $w P a$. Substituting w for a in T_R in the previous figure, $w = \text{STRAT}(T_R)$ only if the left subtree of T_R is the agenda " x vs. w , the winner against a " or " y vs. w , the winner against a ." But now, as in Case 1, the symmetry of T requires that a also enter the agenda in the final ballot of T_L , and since $a P x$, $x \neq \text{STRAT}(T_L)$.

Q.E.D.

Proof of Theorem 10. By induction on $|\alpha|$. Assume that $x = \text{STRAT}(T)$. Then $x \in G(\alpha)$ by Theorem 3. Since x is covered, $G(\alpha)$ is a cycle, so $x P y P w$ for some y , $w \in G(\alpha)$. Let $\{z_1, \dots, z_n\} = \alpha - \{x, y, w\}$. Since $x P y$, y does not cover x ,

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and since $y P w$ and anything that covers x bears P to y , w does not cover x . So z_1, \dots, z_n cover x by hypothesis of the theorem. Hence, $z_i P x$ and $z_i P y$ for all i , and thus, since $G(\alpha)$ is a dominant cycle in α , w bears P to some z_i , say $w P z_1$. Because $x = STRAT(T)$, x equals $STRAT(T_L)$ or $STRAT(T_R)$; say $x = STRAT(T_L)$. By Theorem 3, $x \in G(\alpha_L)$, where α_L is the set of alternatives in T_L . By the symmetry of T , α_L lacks just one member of α . Hence, $y, w \in \alpha_L$ (else $x \notin G(\alpha)$). So if any z_i belonged to α_L , we would have $x \neq STRAT(T_L)$ by inductive hypothesis. Thus, no z_i belongs to α_L . Consequently, $n = 1$ and $\alpha = \{x, y, w, z_1\}$. Since $|\alpha| = 5$, the theorem follows from Theorem 9.

Q.E.D.

Note

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PRISONERS' FINANCIAL DILEMMAS: A CONSOCIATIONAL FUTURE FOR LEBANON?

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Lebanon's banking system is viewed as an "international regime" that held the country together in the face of all pervasive warfare. The system illustrates the underlying logic of such regimes, in that the banks' credit policies operationalized the rational choice model of an iterated prisoner's dilemma game. And just as international regimes are supposed to reflect the interests of state actors, analysis of the banks' balance sheets and income statements reveals changes in capital structure that reflect changes in Lebanon's political balance. While sustaining elite cohesion, the banks serviced political clienteles and might, if a political settlement were reached, support the restructuring of a consociational system more in line with Lebanon's demographic balance. If, on the other hand, no settlement is reached, the poor country's suicide may highlight certain aspects of international reality that seem recalcitrant to the theory of international regimes.

Whereas Plato sought ethical principles by "writing large" the travail of the human soul into a well-ordered political macrocosm, contemporary students of international relations write small. They derive theories of cooperation from microeconomics and prisoners' dilemmas. In a world without enforceable centralized authority or the proxy of a hegemonic state, the Hobbesian sovereign gives way to intermediaries who, like used car dealers, are supposed to prevent "market failure" (Akerlof 1970; Keohane 1984). This fresh, albeit shadowy, "grey in grey" (Hegel 1953, 13) perspective on international regimes may also shed light on domestic situations. In Lebanon the level of conflict possibly exceeds even Hobbes's visions of brutality and nastiness. In this paper I will view

Lebanon's banking system as the principal "regime" that supported the country's political elite as well as its economy in the face of uncivil, multidimensional warfare.

The new perspective of international cooperation rests squarely on concepts of transaction and information costs borrowed from financial rather than political theory. As is well-known, however, "economics proceeds in a context framed by law and order, established by politics. Economic life continues in wartime and in periods of revolution and anarchy, but under conditions that must be regarded as pathological" (Kindleberger 1970, 15). How then can financial concepts apply to a world of anarchy, in which property rights and contracts are unenforceable? Lebanon offers a practical illustration in

which to ground these concepts and recognize their limits.

Since 1975 the country has replicated in microcosm the condition of international anarchy posited by the structural realists, yet its "pathological" economy survived and in some sectors prospered until the Israeli invasion of 1982. Its banking system continued for a time to finance industry, illustrating the new logic of international cooperation and its limits. The bankers cooperated by rolling over their shares of loans, unwittingly operationalizing the rational choice model of an iterated Prisoner's Dilemma game. They also unintentionally adapted to shifts in political power. The banking regime, in fact, supported a potentially functioning polity. In this light, financial intermediation can be understood as the elite adhesive required to make consociational democracy work. It will be shown that the banks' capital structure changed—and could change further—in ways that better reflect the country's changing political and demographic balance.

The argument of this paper will be developed in four stages. First, the theoretical rationale for international regimes is translated back into its financial nexus. A theory of financial intermediation can shed light on the Lebanese banking system and explain why a "regime" was needed. Secondly, the system will illustrate the functioning of the regime under conditions of anarchy. Thirdly, the conditions for its demise or transformation will be analyzed in light of the Prisoner's Dilemma. And, finally, Lebanon's banking regime will be related to conventional political analyses of the country's past and its potential futures.

International Regimes and Theories of Financial Intermediation

In systems lacking centralized enforcement, *regimes* can be generally defined as

"sets of implicit or explicit principles, norms, rules, and decision-making procedures around which the actors' expectations converge" (Krasner 1983, 2). Keohane (1984) compares them to credit markets (p. 129) and explains in his seminal work why actors' expectations might continue to converge around them even when the balance of power changes: regimes can be understood "as information-producing and transaction cost-reducing entities rather than as quasi-governmental rule-makers" (p. 101). They are supposed to enable the actors—typically state actors—to stave off market failure in some sense. The concepts, however, were extracted from the world of finance, not politics.

The spectre of market failure may be easier to locate in purely economic activities, however political they may also be, than in conventional power relationships among states. Most intended regimes, of course, comprise interrelationships between state actors, but the actors need not be states. The international banking system may display a regime, for instance, insofar as banking operations across national lines escape the jurisdictions of the banks' parent countries. To the extent that relationships between Less Developed Countries (LDC) debtors and creditors remain viable, it makes sense to speak of an international banking regime of nonstate actors (Lipson 1985). Our theoretical problem is to discover the sources of Pareto-deficient equilibria—or market failure—that a regime of financial intermediation might remedy.

The financial literature offers a variety of models to explain the underlying rationale for financial intermediation in advanced capitalist systems. The multiplicity of explanations is due, of course, to the variety of possible market imperfections financial theorists may model (Campbell and Kracaw 1980; Chan 1983). If the present analysis were intended to offer historical clues about the rise of

banking systems, an eclectic approach would be in order (Kindelberger 1983). Transaction costs are obviously fundamental to explanations of financial systems in trading countries like Lebanon. So, too, a developmental perspective would stress transaction costs and asset transformation because the focus is on mobilizing savings for industrial investment (Abdi 1977). But most of the market failure literature is really about information (Chan 1983), and some of it analyzes conditions in advanced capitalist societies under which banks may substitute for capital markets. This analysis is the most useful for Lebanon because it was the banks' medium and long-term lending, not their principal business of trade financing, that was the greatest anomaly in the wartime situation.

The source of the problem is quality uncertainty offered in markets characterized by informational asymmetries (Akerlof 1970). Without reliable used car dealers, for example, people will only offer lemons for sale because buyers will be unable to distinguish them from cream puffs. Buyers will not have adequate incentive to acquire the information needed to make the necessary distinctions, nor will anyone else, given the public goods nature of information, unless perhaps a used car dealer can trade on (lower-cost) information and acquire a good reputation. Financial theorists have developed this line of argument into a convincing rationale for financial intermediation in societies where contracts can be enforced. It is useful to summarize the argument and its underlying assumptions before extending it to other societies where contracts cannot be enforced.

Like Akerlof's used cars, investment projects vary in quality. Assume that each entrepreneur knows the value of the project but is unable reliably to communicate it to potential investors. In the investment market, bad projects will drive out good ones, and potential investors will be

unwilling to invest their funds, assuming that alternative placements (e.g., in other markets, outside Lebanon) are available. As Leland and Pyle (1977, 383) point out,

two problems . . . hamper firms which might try to sell information directly to investors. The first is the appropriability of returns by the firm—the well known 'public good' aspect of information. . . . The second . . . is related to the credibility of that information. It may be difficult or impossible for potential users to distinguish good information from bad. If so, the price of information will reflect its average quality. And this can lead to market failure if entry is easy for firms offering poor quality information.

This as may safely be assumed in unregulated settings. In the absence of intermediaries, unreliable information would have resulted in a suboptimal level of investment in Lebanon.

Financial intermediaries "will be the preferred form of information producer" (Campbell and Kracaw 1980) if entrepreneurs desire confidentiality (as modeled by Campbell 1979). A Pareto-preferred rational expectations equilibrium will emerge as long as it can be assumed that the intermediaries are producing reliable information. Without assuming "pathologically honest" information brokers, the moral hazard question may be resolved if there are capital barriers to entry into the information market or at least a context of law and order for enforcing contracts. Otherwise, however, a rational expectations equilibrium between investors and financial intermediaries will not ordinarily be achieved through market forces. Some sort of regime will be needed to make the market work. In Lebanon, as we shall see, the government had to impose order on the banking system in the late 1960s. Subsequently, when the government could no longer enforce contracts, much less the banking regulations, the bankers themselves had to keep up their own regime.

Iterated Prisoners' Dilemmas and Discount Rates

Regimes of this kind arise, or at least may be sustained, in response to dilemmas of common interest or common aversions (Stein 1983). The theoretically more interesting dilemmas, requiring collaboration rather than mere coordination, are those of common interests, as expressed in the prisoner's dilemma.

The two prisoners, it will be recalled, can either stick to their alibi when the district attorney separately interrogates them or confess to the crime. The sentence for sticking to one's alibi (cooperating) will be longer than that for confessing (defecting) if the other prisoner gives in to the DA's bait of confessing and going free. The payoffs may be summarized: $DC > CC > DD > CD$. The prisoners have a common interest in cooperating ($CC > DD$), but each is better off defecting, whether the other cooperates ($DC > CC$) or defects ($DD > CD$). Their rational expectation, given the DA's incentive scheme, is mutual defection. The actors' dominant strategies produce, in microeconomic language, a Pareto-deficient equilibrium.

Though the prisoners' rational expectation is mutual defection in a one-play game, mutual cooperation becomes possible if they can expect an indefinite future of interactions. In the iterated version of the game proposed by Axelrod (1984), the two players know their past "prison" of interactions, anticipate an indefinite future of interactions, and value present payoffs over uncertain future ones. They can escape the dilemma if they can rationally expect each other's continued cooperation. Axelrod proposes that they may reasonably cooperate rather than defect if, in addition to expecting an indefinite future of interactions, each player does not discount expected future values of the increments of collaboration as compared to defection enough to make the present benefits of the latter offset the former.

The critical variable is each player's discount rate.

The Lebanese bankers operated, of course, with real discount rates, or opportunity costs of capital, and these may serve to flesh out Axelrod's model. The following exposition about Lebanese banking will lead into the model of a prisoner's dilemma among bankers who roll over doubtful loans. By such "thick" analysis of an international regime, throwing financial metaphors back into context (Sartori 1970), I do not intend to criticize Axelrod or Keohane, much less to deter others from letting their metaphors fly further afield—rather, a firmer grounding may lead to higher rebounds.

The Lebanese Banking Regime

In the 1950s and 1960s, Beirut was the principal trading entrepôt for the Middle East, including the oil states of the Persian Gulf. Its physical and political conveniences also made Beirut the region's principal information center for exchanging commercial as well as other sorts of intelligence, but the banks were primarily engaged in reducing transaction costs associated with domestic and regional trade. Letters of credit and short-term commercial loans, often triangular deals connecting Lebanese traders with their clients around the world, continue to be the banks' bread-and-butter business.

The commercial banks became involved, however, in financing industry with medium- and long-term credit. Formal capital markets never functioned properly (Hoss 1974, 38), for investors had little reason to believe that stock prices, manipulated by inside dealers, reflected publicly available information. Moreover, very little reliable information was available to the public: published financial statements could not reflect generally accepted accounting procedures because the Lebanese accounting profession was unregulated, its practitioners at

the mercy of their wealthy clients. Most entrepreneurs, too, preferred to keep ownership within closed circles of family and friends rather than to sell equity on an open market.

The situation of the typical Lebanese entrepreneur was analogous to that of the manager in more complex financial markets who prefers inside debt to equity or bond issues so as to hide strategic plans or marketing secrets from competitors (Campbell 1979). The banks were attractive to the entrepreneur because banks "do not always know the final purpose for which loans have been granted. This is mainly due to the multipurpose activities undertaken by a large number of major bank clients" (Saliba 1981, 152). Not always knowingly (though better informed than other potential investors), the commercial banks, by rolling over short-term commercial loans—in effect translating them into medium- or long-term financial instruments—sustained optimal levels of investment in Lebanese industry.

The Origins of the Banking Regime

The Lebanese government undertook its first serious efforts to regulate the banking industry only after it was rocked by a major financial crisis. In October 1966 the Palestinian-owned Intra Bank, by far the largest of Lebanon's banks, ceased to meet its payments after a run on its deposits. Whether, as its defenders argued, the bank merely faced a temporary liquidity problem or, as its critics insisted, it had been mismanaged to the point of insolvency, the immediate cause of the crisis was that Intra had engaged in too much long-term lending of short-term deposits. In reality this bank was only the tallest tree in a forest of overextended and mismanaged banks (Moore 1983, 33–34). The crisis enabled the government to implement the Code of Money and Credit enacted in 1963 over the opposition of Lebanon's freewheeling bankers, repre-

sented by their Association des Banques du Liban (Asseily 1967, 16–22).

The new regime sponsored by the government consisted of a Supreme Banking Council headed by the governor of the Bank of Lebanon (as the new central bank was named) and performing "the function of a banking court with authority to impose controls and various degrees of penalties on banks, including the withdrawal of the banking license." An independent Banking Control Commission was set up at the central bank to supervise the commercial banks and recommend to the Supreme Council any action to be "taken over banks facing special difficulties or banks violating the rules and regulations prescribed by the Code of Money and Credit and the directives issued by the Bank of Lebanon or the Banking Control Commission itself" (Saliba 1981, 149). In the spirit of Lebanon's constitution, it was informally understood that the governor of the central bank would be a Maronite, the chairman of the Banking Control Commission a Sunni Muslim, and so on down the line, replicating the country's system of countervailing minorities.

Authorized to clean up the system, the Banking Control Commission closed down ten of Lebanon's remaining 85 banks, permitted four others to go into voluntary liquidation, and supported a moratorium against issuing new bank licenses (Hoss 1974, 32). The managers of some of the liquidated banks were brought to trial and convicted for lending substantial sums to friends and fellow board members without adequate prospects of repayment, and some twenty members or former members of parliament were implicated (Messara 1977, 151). The Banking Control Commission was not able, however, to impose ratios or other restrictions upon banks in need of "improving their special situations" (Hoss 1974, 26) before the government collapsed in 1975. It had not resolved the basic problem of mismatched maturities

and tacit term-lending by rollovers. Banks were still permitted to make loans of up to 30% of their capital per individual to their shareholders, families, and friends (Banker Research Unit 1980, 116).

The Regime's Survival under Conditions of Anarchy

Despite the collapse of government, however, the banking system survived without a single bank failure between 1975 and 1984. Fluctuations in international interest rates were far more volatile and threatening to Lebanese liquidity than those upon which Intra had founded (Ghattas 1971). When, as in 1979-80, Eurodollar rates skyrocketed, local banks were tempted to speculate in foreign exchange transactions at the expense of liquidity in pounds. Another factor that had contributed to the problem of Intra and other banks was the scarcity of short-term commercial lending opportunities in Lebanon relative to the supply of short-term deposits. The warfare naturally accentuated the disproportion, for the deposits kept coming in—remittances from abroad, salaries from militias, and high hashish sales—partially offsetting the general slowdown in economic activity, whereas the slowdown restricted short-term lending, tempting some banks into excessive long-term lending and real estate speculation. In response to these pressures the central bank tried on occasion to restrict credit and reduce the ceiling on the net foreign exchange positions banks were in theory allowed to hold.

The regime survived in no small measure because of the commitment, and indeed heroism, of the Lebanese central and commercial bankers and their employees, who dodged bullets, paid off rival militias, and managed to keep their doors open for all but a few months of a 10-year nightmare. But economic factors were also at work. The central bank's ample gold cover, safely stored (the writer

suspects) in the vaults of the New York Federal Reserve, increased sufficiently in market value to keep the Lebanese pound more amply covered than any other freely convertible currency in the world. And, of greatest import, the underlying Lebanese economy was sufficiently resilient to support a free banking system. The value of Lebanese industrial exports (mainly to Saudi Arabia and Iraq) increased from \$64 million in 1970 to \$528 million in 1980 (Rivier 1982, 123). But this economic miracle begs the question of why bankers remained willing to roll over the loans needed to finance it—even after the Israeli invasion of 1982 destroyed much of Lebanon's industrial plant.

Bankers became entrapped in ever-growing mountains of debt, much of it in the form of nonperforming commercial loans. By the end of 1983, bad debts "rolled over on a yearly basis . . . were conservatively estimated at around 15 percent of total loans outstanding" (Iskander and Baroudi 1984, 207). The Lebanese bankers had become the prisoners of their clientele but had to keep pumping and priming them, as do the multinationals with their LDC debtors for lack of better alternatives. Writing off even that conservative 15% of their loan assets, amounting to 169% of their capital accounts (Banque du Liban 1984, 41), would have entailed collective bankruptcy. Instead they cooperated, resolving their dilemma of common interests by rolling over the loans rather than individually defecting. Like blind soccer players they continued to exchange unreliable information about their clients in a stadium where, as one leading politician observed, not even the law of the jungle prevailed; so dense and overgrown was its underbrush of class, regional, and international, as well as sectarian, conflicts.

The very failure of the Banking Control Commission to rationalize the system and foster specialized medium- and long-term credit institutions may have helped the banker-prisoners to adapt to the new war-

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time situation. Credit analysis remained, for the most part, as refractory to modern techniques as when Yusif Sayigh had interviewed Lebanese bankers (1962, 94). Despite professional credit analysis seminars introduced by a dynamic chairman of the Bank Control Commission in 1981 and 1982, with the help of leading U.S. banks and continued by the American University of Beirut's Business School in 1983, the Lebanese paid only lip service. As one leading banker explained, credit was a function of trust and friendship, not of projected cash flows based on tenuous, primarily political assumptions. The high number of Lebanese banks meant that only trust and friendship could prevent market failure and throw bankers who were not necessarily trusting friends into each other's arms. The logic of the system rested on a classic creditors' dilemma of common interests.

The regime survived anarchy because it did not need to rely on the authorities to enforce regulations and directives. The central bank exercised authority over weaker banks that had lesser stakes in the system—even so, without operative legal sanctions, its tactics smacked more of carrots than sticks. Authority over the larger banks seems to have been minimal. When, for instance, the Bank Control Commission once confronted a major bank head-on, insisting that it remedy its woeful capital inadequacy, the dynamic chairman of the Control Commission lost his job despite full support from the Governor of the central bank (himself the son of a Lebanese president and a potential presidential contender). The Christian who managed the bank in question, a former finance minister, successfully negotiated the support of the incumbent Sunni prime minister, who disposed of this bit of Sunni patronage and was anticipating future electoral expenditures—high in Lebanon by international standards, as Hudson noted (1968, 255). The successor to the Control Commission was equally

unable to impose prudent liquidity and solvency ratios upon the banks.

Most borrowers eligible for government subsidies to cover damage incurred in the 1975–76 fighting had preferred privately to renegotiate their loans with the banks, thereby preserving mutual honor and confidentiality, rather than be listed as receiving handouts. To assess the prospects for continued renegotiations, it is useful to try to reconstruct plausible parameters for their prisoners' dilemmas.

The Prisoners at Work: The Model

Suppose, not unreasonably in the conditions prevailing in Lebanon between 1976 and 1982, that an entrepreneur enjoys a close, possibly family relationship with the owner-manager of a bank that enjoys a good reputation with other banks. The bank may exchange shares of the entrepreneur's loans with other reputable banks for shares of their loans to other equally well-placed entrepreneurs—normal portfolio diversification to spread the risks—or the entrepreneur may patronize several banks. Wishing each year to roll over the loan, the entrepreneur who can credibly threaten his banks with default yet promise ultimate repayment "when the security conditions in the country improve" then enjoys a position of strength analogous to the district attorney's in a prisoner's dilemma. In practice, more than two banks will be renegotiating their loan with the entrepreneur, but the flavor of the situation can be conveyed by imagining just two banks, locked into a prisoner's dilemma by the shared prospect of having to refinance the entire loan or lose its share if the other defects. Assume in this simplest symmetrical case that each bank's share, k , is equal and that the perceptions of the probability of ultimate repayment, p , communicated by the original lending bank, are believed by the

other banks. Then the expected identical payoffs are as follows:

$$DC = k(\text{loan})$$

$CC = pk(\text{loan} + \text{interest})$, where p is the probability that the loan and interest will be repaid in a year.

$$DD = 0$$

$CD = p(\text{loan} + \text{interest}) - (1 - k)(1 + r)$ (loan), where r is the discount rate, or opportunity cost, of the additional funds required to finance the debtor if the other bank refuses to roll over the loan.

Assuming perfect symmetry, the banks will have the same opportunity cost of capital. The problem, following Axelrod (1984, 207-08), whose w can be transformed into a real discount rate $r = (1 - w)/w$, is to determine the maximum value of r , or the minimum of p , the probability of repayment, for which the payoff, DC , for unilateral defection will be offset by the stream of future benefits from mutual cooperation. If the players employ Axelrod's tit-for-tat strategy, the defector can expect a total stream of payments $DC + DD/(1 + r) + DD/(1 + r)^2 + \dots$, whereas the bank that rolls over the loan can expect a total stream of CC/r . Since $DD = 0$, cooperation on a tit-for-tat basis is defensible as long as

$$DC < CC/r$$

$$k(\text{loan}) < pk(\text{loan})(1 + i)/r$$

$$r < p(1 + i), \text{ or } p > r/(1 + i)$$

In other words, if $r = .25$ and $i = .15$ (plausible in 1981), future expectations of cooperation could offset the temptation unilaterally to defect as long as the chances of repayment were at least one out of five. Banks where opportunity costs of capital were a bit higher—up to 45% return on equity—could still reasonably roll over loans if they perceived the chances of repayment to be better than two out of five. As long as the bankers could believe there were some prospects

of an eventual recovery of the Lebanese economy, there seemed little reason for not continuing to lend to one's kin or friends or, by sharing in other banks' loans, to friends of other intermediaries.

Strategies to Avoid Playing Chicken

The model helps to explain banks' strategies as well as why they might keep cooperating in rolling over commercial and industrial loans under conditions of general anarchy. The assumption of perfect symmetry can be relaxed to reflect some of the variances in the luxuriance of strategies, types of ownership, styles of management, and clienteles displayed by the 80-odd commercial banks operating in Lebanon. After 1977, when the moratorium on issuing new bank licenses was lifted, 10 new banks squeezed into the crowded market place in search of prime clients while a number of established banks were sold to aggressive new owners, enabling potential prisoners to escape.

The banks can be situated in strategic niches defined by their opportunity costs of capital and perceptions of credit risks. Banks with high opportunity costs of capital and "conservative" habits of estimating risk would prefer to reduce their portfolio of doubtful debts and escape playing iterated prisoner's dilemma games. But it was better to qualify as a prisoner than be perceived as a less credible chicken ($DC > CC > CD > DD$) (Oye 1985, 8). Chickens had less bargaining power than real prisoners because their threat of defection was less credible. If $CD > DD = 0$, then

$$CD = pL(1 + i) - (1 - k)(1 + r)\text{loan}$$

$$> 0$$

$$p > (1 - k)(1 + r)/(1 + i)$$

A bank could be perceived as playing chicken if p (the banker's estimate of the probability that the loan would even-

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Table 1. Profiles of Owner Nationalities and Loan Portfolio Growth Rates of the Top 15 Creditor Banks by Rank: 1974-1982

Rank				Annual Growth of Loan Portfolio 1974-82 (%)
1974	1982	Bank	Ownership	
1	15	G. Trad-Crédit Lyonnais	French, Lebanese	11
2	9	Société Nlle. de la Banque de Syrie et du Liban	French, Lebanese	17
3	1	Banque Libano-Française	French, Lebanese	26
4		Citibank	U.S.A.	-8
5		Banque Nationale de Paris	French	9
6		Chase Manhattan	U.S.A.	-9
7		Moscow Narodny ^a	U.S.S.R.	4
8		Société Générale Libano-Européenne	French, Lebanese	14
9		Arab African International Bank	Egyptian	-12
10		Bank Almashrek	U.S.A., Lebanese	14
11	3	Fransabank ^b	Lebanese	22
12	4	Banque Audi	Lebanese	29
13		Banco di Roma	Italian	3
14	8	Banque Libanaise pour le Commerce	Lebanese	25
15		Bank of America	U.S.A.	-11
	2	Banque du Liban et d'outre-Mer	Lebanese	36
	5	Banque Byblos	Lebanese	31
	6	Banque de la Méditerranée	Lebanese	31
	7	Beirut Riad Bank	Lebanese	29
	10	Banque Sarader	Lebanese	44
	11	Bank of Beirut and the Arab Countries	Lebanese	35
	12	Crédit Libanais	Lebanese	27
	13	Banque de l'Industrie et du Travail	Lebanese	35
	14	Crédit Populaire	Lebanese	40

Sources: Association des banques du Liban 1974; Baz 1984.

^aGrowth of loans is calculated until 1981.

^bThe Banque Française Pour le moyen Orient was merged with Banque Sabbagh and renamed Fransabank.

tually be repaid) were sufficiently high, k sufficiently high, and/or r sufficiently low. The profile of the chicken player was that of an unprofitable family bank saddled with high shares of loans on which its outlook remained bullish.

Foreign banks, especially the big U.S. multinationals, were among the first to cut their loan portfolios, while Lebanese-owned banks took over many of the former's prime Lebanese customers' loans (and corresponding deposits). Although most of the big foreign banks retained a presence in Lebanon after the first rounds of fighting in 1975-76, their position relative to Lebanese banks altered dramatically. Table 1 lists by rank the top 15

creditor banks in 1974 and in 1982, along with profiles of their principal owners' nationalities and the average annual rates of increase in the size of their loan portfolios between 1974 and 1982. Lending remained as concentrated in 1982 as in 1974—the top 15 accounting in both years for slightly over half of the banking system's credit to the private sector—but foreign banks and Lebanese banks owned in large part by foreign interests removed themselves from the competition. The fully Lebanese-owned banks, seeking market share and having greater confidence, perhaps, in their borrowers, were more willing to become prisoners.

The larger aggressive Lebanese banks

could divide up shares of prime customers' loans no longer wanted by Citibank or its peers, whereas family banks that elected to remain small risked becoming ever more burdened by undiversified chunks of debt of friends or relatives. If a bank's share of a doubtful debt were high, however, it would not qualify as a player in prisoner's dilemma because it would be better-off rolling over the loan even if other banks defected. Always concerned to prevent any bank failures that would adversely reflect upon the system, the central bank kept ailing family banks in business and encouraged their sale, wherever possible, to outside investors or their merger with healthier banks. In one case it even permitted the new investors (headed by a former finance minister) to acquire the bank *without* its portfolio of doubtful debts.

Most of the aggressive banks, in turn, seem to have regained relatively healthy returns on their capital after a period, between 1975 and 1978, of plowing their revenues back into reserves ("provisions") for doubtful debts. Among the 10 of the 15 top credit banks that had published income statements since 1973, only 3 were not declaring consistent profits—averaging 27% return on equity—during those golden years of banking under conditions of anarchy, 1980 and 1981. Their go-go growth strategies, however, generally sacrificed profits for market share and resulted in steady decreases in capital adequacy (i.e., higher leverage and risk).¹ Staying small and nursing a less diversified portfolio, on the other hand, could be riskier because other banks would have less incentive to cooperate in rolling over any problematic loans. The high-growth, undercapitalized banks could at least be each other's prisoners rather than succumb to the largesse of the central bank.

To qualify as a prisoner rather than a chicken, a bank needed size and corresponding market power (and possibly political power) as well as profitability to

keep its individual shares of dubious loans to a minimum. No prisoner's dilemma model fully captures the process of loan renegotiations between unequal partners (Telhami, n.d.), but occasionally, as with Sinno and Jabbour in 1982, the negotiations became public knowledge. The banks broke out of their prisoners' dilemmas and delegated the head of a leading Lebanese bank, a former finance minister, to renegotiate the loans. The Sinno and Jabbour Group in the end agreed to sell off some real estate (including interests in a nightclub in Rio de Janeiro) and convert its short-term debt into a medium-term loan. The exposure of the bank that renegotiated the package was reduced by more than two-thirds, to a mere 5% of the term loan; another prominent Lebanese bank, enjoying especially close relationships with both the lead bank and their common French banking connections, also had its exposure whittled down at the expense of other Lebanese and Canadian banks (Abu Zaki 1982; Restructuring 1982).

The Apparent Breakdown of the Regime

Banks could reasonably continue to roll over dubious loans until 1984, when the political and economic consequences of the Israeli invasion and its aftermath, including the bombing of the U.S. and French peacekeeping forces, became fully apparent. Much of Lebanon's industrial plant had been destroyed, and exports to the Gulf were adversely affected also by the uncertainties of overland transportation through Syria and reduced demand in Iraq and Saudi Arabia. Because taxes could not be collected, the government experienced alarming deficits and a burgeoning public debt. For lack of exports or foreign investment, there was a record balance of payments deficit in 1984. The central bank insisted, however, that "exchange controls cannot be a solution to the balance-of-payments problem

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because it would finally sweep away confidence in the Lebanese economy" (Banque du Liban 1985, 20).

The new phase in Lebanese banking was anticipated by a swindle. The manager of First Phoenician (purchased from First Chicago in 1982) made fraudulent loans (Summary 1984, 31-34) financed in part by interbank deposits at high interest rates and departed for Brazil in March 1984 (Iskander 1984). The central bank rescued the depositors and put the bank under new management, but might not other banks finance borrowers speculating in foreign exchange with similar results? Assuming that most banks still acted "legally" and restrained their financing of foreign exchange speculation, what were their new parameters for rolling over old loans as expectations hardened that the bottom had fallen out of the foreign exchange market for the Lebanese pound?

An expected depreciation rate, d , of the Lebanese pound must be included in the model. The temptation of cashing in a loan might then be offset by the present value of the rewards of future repayment if

$$r < p(1 + i)(1 - d), \text{ or}$$

$$p > r(1 + i)(1 - d).$$

By late 1984 there seemed little reason not to expect a continuing annual slide of the pound against the dollar (or a basket of foreign currencies) of 50% or more. Consequently, the probability of repaying a loan had to be at least $1/(1 + i)$ for banks to fit the prisoners' model of cooperation and at least to break even ($r = d$). Yet interest rates were being kept well below the rate of expected depreciation; and peace seemed ever less likely, diminishing the probability of repayment of doubtful debts.

Why banks continued to roll over loans is not altogether evident. For customers who could never repay or who had per-

haps run off to Brazil with no intention of repaying, Lebanese banks still had no alternative but to keep the loans on their books. There was no legal way of writing them off unless the customer were declared bankrupt, but without functioning law courts, bankruptcy could not be declared (Saliba 1981, 154). Solvent clients, however, had their cash "outside" Lebanon. Any bank's advantage in collecting loans in depreciating Lebanese currency could still be offset by the rewards of other ongoing business relationships with the client. Because banks representing 60% of the banking system's total assets (Iskander 1984, 3) had established branches in Paris, Geneva, London, and Brussels, they might still be rolling over some of the nonperforming loans under the shadow of a shared future.

Dilemmas of common interest had meanwhile supported a bankers' regime inside Lebanon during a period of anarchy lasting from 1975 until 1984 or so, independent of continuing relationships abroad. The banking system was also one of Lebanon's few institutions to have preserved the values of mutual, intraelite accommodation enshrined in its prewar consociational "democracy" (defined by Kerr not as "the rule of the *demos* but simply the distribution of guarantees to the recognized factions coexisting in the country of the means to defend their minimum interests" [1986, 188]). The banks prevented market failure despite asymmetric information segmented along sectarian as well as along family lines. They often cooperated across sectarian lines—a big syndicated loan for a cement works at Sibline, near the Shouf mountain strongholds of the Druze, being a good political example. Lebanon's leading Christian-and Sunni-owned banks participated in Druze chieftain Walid Jumblatt's project, whereas the major Druze-owned bank did not.

The banks were almost as much political as financial intermediaries. They help

explain not only financial cooperation during Lebanon's war but also a political anomaly, namely why the antediluvian political chiefs—despite a revolutionary situation in 1975 (Johnson 1983)—could return to power or at least fight over its absence. "Writing large" the banking interests back to their political nexus, Lebanon's multibank system effectively performed a function of elite integration, whether for consociational democracy or for the more authoritarian regime attempted by the Gemayels after the Israeli invasion.

"Writing Large": Capital Adequacy and Political Change

Unlike Austria or Holland, Lebanon never evolved mass parties of social integration, even among lower-middle class Maronites (Entelis 1974, 101–23), that would make the consociational formula work under modern conditions. Instead, clientage networks not dissimilar to the ones described by Albert Hourani (1968, 41–68) in eighteenth century "Syria" persisted in the Lebanon of the 1970s. In part, the civil war signified the breakdown of the patrons' control over socially mobilized and politically awakened masses. One astonishing feature of Lebanon's superstructure by the late 1970s, however, was the resurrection of many of the political patrons whose grips over their respective communities had been weakened in the heat of the battles of 1975–76. The status of these putative warlords was apparently hereditary—as attested by the careers of Walid Jumblatt, Bechar and Amin Gemayel, and many others (Khalaf 1980, 254–58; Messara 1977, 183–201).

Durable patron-client networks may be presumed to have permitted these families to retain their grip, even as their locus of economic influence shifted after the

Second World War from rural landholdings to urban commerce (Dekmejian 1975; Harik 1975). Banking then became so central to Lebanese politics that, in alliance with other commercial interests, it prevented the building of an interventionist Lebanese state (Dubar and Nasr 1976, 67–75; Owen 1976, 28–31). The country was to be a safe haven for senior French and then U.S. multinational partners in the 1950s and 1960s. Clientelist practices usefully insulated the commercial center against sectarian pressures (Lemarchand 1981, 23) even as the specter of "confessionalism" paralyzed efforts to change the political system. But the commercial machinery that materially sustained the political networks also contributed to the elite's cohesion. Without denying its possible basis in a shared class interest, this cohesion was most visibly displayed in concrete interconfessional commercial ties centered in the banking system.

The banks contributed to intersectorial cooperation by sharing in the financing of entrepreneurs, thereby directly or indirectly supporting the clienteles of the political chiefs. By exchanging information and sustaining capital markets, they provided those tangible "first-order" resources needed to preserve durable patron-client structures (Moore 1977; Scott 1972). The greater the number of "competing" banks, in fact, the better the chances of keeping the elite mutually accommodating despite acerbic cleavages that confessional violence further embittered after 1975. The system, however, also eliminated some elites who appeared too powerful (anticipating the Palestinian Liberation Organization), notably those Palestinians who, through Intra Bank, had controlled a host of strategic industries such as the Middle East Airlines, the Port of Beirut, and the Casino du Liban. Portfolio diversification mitigated other cleavages among Lebanese in the sectors that counted most: the commercial and industrial sources of political revenues

inside the country. Indeed the leading political families shared just as many dilemmas of common interest as the bankers, but the bankers were the necessary mediators. Dilemmas of cooperation are less amenable to rational solutions among actors who, unlike bankers, are too envious and "too clever" (Axelrod 1984, 23).

Obviously bankers without guns could not control warlords (though a couple of prominent bankers were considered to be the principal financial promoters behind the elections of two presidents in 1982). With or without guns, the political chiefs hardly seemed capable even of primitive sociability, much less of reshaping a consociational formula, which requires an accommodating political elite. Salman (1984) compared their discussions at Geneva and Lausanne in 1983 and 1984 to those of hashish addicts living out Egyptian illusions and realities after 1967 on a houseboat in Naguib Mahfouz's *Chatter on the Nile*. Unfortunately, political Lebanon cannot survive unless they can agree on a new form of consociation (Messara 1983), yet most moderate non-Christians view the open recognition and legitimization of confessional cleavages required to make a consociation work (Lijphart 1977, 103) as a step toward the partition favored only by some Maronite and Israeli interests.

The alternatives are bleaker: partition or the unwilling assimilation of remaining Christians in a Muslim majority. A dominating Khomeini-style Shia plurality would be as distasteful to other Muslims and Druze, not to mention most educated Shiites, as to the Christian minorities. As Ajami (1985) explains, "the fight in Lebanon now is not about the establishment of a state of the zealous; it is about the apportioning of power among the country's principal sects. . . . A Shia bid for power that tries to outrun the sectarian compact can succeed no better than the Maronite dream of a state cut off from

its Arab environment" (pp. 792-93). The other alternative, partition, may be the outcome of another decade of turmoil and terror unmixing the confessions, but only the Maronite and Druze quarter of the population have safe mountain havens on this gory reading of the future. Partition cannot be the outcome of a deliberate political settlement because the conditions rendering its implementation possible would have already permitted a new consociation.

Toward a Redistribution of Financial Power?

It may still not be too late for the warlords to patch together some settlement. Were the political miracle to occur, the banking system would indirectly contribute to the new equilibrium. The "hell of the old politics resurgent" might yet be reconciled with a "benign utopia of finance" (Ajami 1981, 2) if hard financial logic prevails and the warlords stop playing their perverse games.

Most of the leading Lebanese creditor banks could not sustain their prodigious growth through retained earnings. An examination of their balance sheets and income statements shows that by 1982 capital adequacy (measured by [capital + reserves]/total assets) was woefully inadequate by international standards (Chaib 1983, 13, 17, 18). Most of the banks keeping adequate provisions for their loans, in addition to their capital and other reserves, had required substantial injections of new capital. The new sources of capital were predominantly Muslim and might therefore in time tend to equilibrate the banking system in ways that would support incremental political change within a consociational context.

Most Lebanese freely admit that the old consociational framework's allocations no longer reflect demographic or political realities. It is only possible to suggest the discrepancies between the official alloca-

Table 2. Constitutional, Demographic, and Financial Indicators of Power by Religious Sect

Religious Sect	Seats in Parliament	Percent of Total Population ^a (est. 1985)	Percent of Banking Capital in Top 15 Banks ^b as to		
			lending 1974	lending 1982	net worth 1982
Maronite	30	21			
Greek Orthodox	11	6			
Other Christian	13	8			
Total Christian	54	35	62	47	32
Sunni	20	25			
Shiite	19	36			
Druze	6	4			
Total non-Christian	45	65	18	36	33
Confessionally indeterminate			20	17	35
Totals	99	100	100	100	100

Sources: Chamie 1981, 25, 85; Association des Banques du Liban 1974; Baz 1984, 215, 220.

^aEstimated growth rates for each sect were projected from the 1932 census. Palestinian immigrants, some of whom became Lebanese citizens, are not included in these estimates of the Lebanese population.

^bThe allocations of capital among the top 15 Lebanese creditor banks of 1974 are calculated from capital book values, whereas the capital of the 1982 creditors includes other reserves and loan provisions as well. Only capital and reserves, not provisions, are counted for the 15 banks having the greatest net worth.

tions, indicated by parliamentary seats, and the demographic realities, estimated impressionistically (the last Lebanese census was undertaken in 1932, and subsequent administrative projections make political but no demographic sense)—without statistically valid margins of error—in light of recent research into the fertility of the respective communities.² The results are summarized in Table 2. Comparisons between the first and second columns suggest the extent to which the Maronites, Greek Orthodox, and other Christian communities are overrepresented in the Lebanese parliament at the expense of the Sunni community and especially at the expense of the Shiite community.

The top fifteen Lebanese-owned creditor banks can also be scrutinized, still more tentatively, with respect to their sectarian ownership in 1974 and in 1982. The third column of Table 2 indicates that

Christians controlled 62% of the total capital of these 15 banks in 1974, whereas the Muslims and Druze had only 18%, the rest belonging to banks that seemed confessionally indeterminate.³ With only three exceptions, the same banks occupied the top 15 positions in 1982 (fourth column), but their ownership had changed. New capital injections tended to be Sunni Muslim, mainly from the Arabian Gulf, but many of the large lenders were still inadequately capitalized. A fifth column in Table 2 represents a different, only partially overlapping, set of leading banks, the 15 in 1982 that were the most highly capitalized. In this set the share of Christian equity is further diminished, yet these are the banks that may be waiting in the wings to rescue overextended ones. In the event of peace, a competitive shake-out of Lebanon's overcrowded banking system is to be expected. Comparisons between columns

four and five suggest that the predominantly Christian banks would be the most vulnerable to takeovers. The resulting capital structure might then support a predominantly Muslim allocation of political power, just as the pre-1974 regime supported a Christian one.

Such a shift in financial power would be discreet and subtle: bank owners and managers have never supported the political clienteles of leading politicians along exclusively confessional lines. The logic of loan portfolio diversification encouraged intraconfessional collusion among elites. The most prominent banks were impeccably rooted in other communities as well as their own. One senior banker scolded the present writer for writing of political services Lebanese banks might be performing (Moore 1983)—until a very prominent young politician breezed unannounced into his office, elegantly upholding the author by his sporting presence, to our mutual merriment. Newer banks sometimes lacked subtlety by factoring confessional segmentation into their marketing strategy, putting a token Druze on the board of directors, for instance, to penetrate markets in the Shouf, where the Druze were concentrated. Shared confessional roots might indeed give entrepreneurs better chances to borrow, expand, and then diversify their credit base, especially since so much violence has restricted people and information to ever-tighter communal circuits. Much of any renewed economic activity would in turn reinforce political leaders' clienteles.

Conclusion

Such visions of incremental change may seem utopian, but the most plausible alternative is an equally incremental, albeit violent, grinding down of geographical units into confessionally homogeneous subunits over another decade or two. If Lebanon became too

bloody a laboratory to offer further illustrations of the theory of international regimes, the poor country would, having committed suicide, highlight certain aspects of international reality that seem recalcitrant to theory.

Iterated prisoners' dilemmas assume a pale world, hardly Mediterranean or Middle Eastern, in which economic interest has castrated the passions. A prisoner's dilemma cannot encompass actors who "seek to minimize the difference between their own returns and those of others" (Stein 1983, 134). Its payoff matrices cannot quite do justice to passions like envy (where perhaps $DD > CC$) or to sheer bloody-mindedness (where the other's cooperation may be a source of pathological anxiety: $DD > CD > DC > CC$) —nor even to the uncalculating spontaneity expressed in Rousseau's more famous parable of the stag hunt (1962, 171: $CC > DC > DD = CD$). Bankers may feel more comfortable with Axelrod's tit-for-tat strategies than most politicians or warlords, for status calculations in economic relationships are less likely to transform "a variable-sum game like the Prisoner's Dilemma into a strictly competitive struggle with no possibility for joint gains" than they are in security relationships (Lipson 1984, 15). At least the bankers understand discount rates.

In a real world of anarchy, one that may be all too close to international reality, Lebanese bankers indeed kept faltering capital markets from failing. Their dilemmas of common interests also helped to keep the country together and service the business clienteles of its warring elites. Their banking regime supports Keohane's contention that such regimes may survive the hegemony of their creator, in this case a consociational democracy of sorts, at least for a time. The banking system's adaptation to anarchy also supports Krasner's hypothesis (1983, 361) that international regimes, while depending upon the allocation of power among the state actors that promoted them, may

feed back changes in these relationships.

But meanwhile, Lebanon is committing suicide. Refounding a consociation on which to rebuild its shattered economy and society may require a genie as well as more Syrian military commitment. Most bankers, while prepared to shore up consociational democracy, would favor any Napoleon to protect themselves from the political jungle. Their regime, like the politicians they befriended, is better viewed as the residue of a disintegrated political order than as the proxy for a Hobbesian sovereign. Collusion among financial intermediaries cannot reshape political reality; it can only recall it briefly and perhaps explain it, "grey in grey," after polity and economy have both broken down.

Lebanon may be a plague that should not infect the study of international forms of disorder. But if its internal turmoil is viewed instead as a microcosm of our maladies, then international regimes based on the financial logic illustrated by the Lebanese bankers may be equally pale and unsatisfactory substitutes for more political forms of cooperation.

Notes

I offer this essay in memory of Malcolm Kerr, who supported my work at the American University of Beirut, and of Jim Coleman, who gave me refuge at UCLA. I wish to thank my colleagues in UCLA's Political Economy Colloquia, Jeff Frieden, Mike Wallerstein, and Ahmed Enany for their stimulation. This essay was originally presented at the 1985 convention of the American Political Science Association, New Orleans.

1. Among the 19 Lebanese banks for which income statements as well as balance sheets were available over the years 1980-82, capital adequacy was regressed on the log of the growth of total assets and the ratio in 1982 of the spreads between interest received and interest earned to total assets—a basic measure of profitability. The two independent variables accounted for a corrected r square of .57 of the variance in capital adequacy. The T statistic for growth was -2.1, for spreads, 2.8.

2. Chamie (1981, 85) estimated the net annual percentage growth rates of the respective religious communities as follows: Shiites, 3.8; Sunnis, 2.8;

Catholics (of whom 80% are Maronite), 2.0; Druze, 1.8; and non-Catholic Christian, 1.7. Projecting these rates from the 1932 census, the Lebanese population (excluding all Palestinian immigrants, some of whom became Lebanese citizens) was just over 3 million in 1985, distributed as indicated in Table 2.

3. The ownership of each bank was judged with respect to the confessional affiliations of its directors and its suspected sources of capital to be either Christian, non-Christian, or confessionally indeterminate; some had changed between 1974 and 1982. The judgments are subjective; one or two notable Muslim members of the board did not make an otherwise Christian or Franco-Maronite bank confessionally indeterminate if the notables seemed to be tokens without much capital behind them. Each bank's capital was totally allocated to one of the three categories.

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Forthcoming in June

The following articles, controversies, and research notes have been scheduled to appear in the June 1987 issue:

Paul R. Abramson, "Measuring the Southern Contribution to the Democratic Coalition."

Vinod K. Aggarwal, Robert O. Keohane, and David B. Yoffie, "The Dynamics of Negotiated Protectionism."

Jonathan Bendor, Serge Taylor, and Roland van Gaalen, "Stacking the Deck: Bureaucratic Mission and the Search for Alternatives."

Darrel Dobbs, "Rationalism and Heroic Reverence in Homer's *Odyssey*."

Bernard Grofman, Guillermo Owen, Nicholas Noviello, Amihai Glazer, "The Copeland Winner and Reasons for Stability and Centrality of Legislative Choice in the Spatial Context."

Robert W. Jackman, "Political Institutions and Voter Turnout in the Industrial Democracies."

M. Kent Jennings, "Residues of a Movement: The Aging of the American Protestant Movement."

George Klosko, Edward N. Muller, and Karl-Dieter Opp, "The Rationality of Collective Action."

Michael MacKuen and Courtney Brown, "On Political Context and Attitude Change."

Douglas Madsen, "Political Self-Efficacy Tested."

Samuel Merrill, III and Jack Nagel, "The Effect of Approval Balloting on Strategic Voting Under Alternative Decision Rules."

Edward N. Müller and Mitchell A. Seligson, "Inequality and Insurgency."

Richard Niemi and Joel D. Barkan, "Age and Turnout in New Electorates and Peasant Societies."

Alwyn R. Rouyer, "Governmental Performance and the Decline of Fertility in India."

CONTROVERSY

MODELING WAR AND PEACE

The appropriate model of international conflict and war has been much discussed in recent years, and considerable progress has been made in modeling. Nevertheless the assumptions, conditions, and specifications for international conflict models remain eminently debatable. In the December 1986 issue of this Review, Bruce Bueno de Mesquita and David Lalman presented a generalized expected utility model of international conflict. Here Seif Hussein takes issue with their model in various particulars, aiming to perfect and strengthen the model. Bueno de Mesquita and Lalman respond.

In their recent paper, "Reason and War," Bruce Bueno de Mesquita and David Lalman (1986) present a generalized version of the former's well-known expected utility model of conflict and war. In the following paragraphs I offer modifications of some of their arguments, qualifications and extensions of others. I am only interested in the theoretical arguments, and those discussed here appear in the same order as they do in "Reason and War." Moreover, these arguments are presented in order of increasing importance, thus the more significant points come later.

Monotonicity and Maximization

The two authors start by assuming that decision makers are expected utility maximizers. Thus, when confronted with two options *a* and *b* such that the expected utility from *a* is greater than that from *b*, the decision maker rationally chooses *a*. However, the authors soon introduce another assumption describing the manner in which choice is made. This second assumption is that "the probability of a nation [in a conflict situation] choosing to escalate a dispute is monotonic with its expected utility."

I argue here that the first of these two assumptions is superfluous. The mono-

tonic probability of escalation assumption is meant in the authors' analysis to reflect the continuity of probability assessment as a function of the decision makers' expected utility. Thus, with monotonicity every increment in expected utility increases the probability of escalation.¹ Expected utility maximization, on the other hand, does not allow for continuous changes in the probability of escalation. In fact, under the maximization assumption the probability of escalation can take no more than the following three values:

$$\begin{aligned}P^i(Esc_i) &= 1 \text{ if } E^iU_{ij}(Esc) > E^iU_{ij}(Esc) \\P^i(Esc_i) &= .5 \text{ if } E^iU_{ij}(Esc) = E^iU_{ij}(Esc) \\P^i(Esc_i) &= 0 \text{ if } E^iU_{ij}(Esc) < E^iU_{ij}(Esc)\end{aligned}$$

where

$P^i(Esc_i)$ = probability that decision maker *i* decides to escalate a dispute with *j*

$E^iU_{ij}(Esc)$ = *i*'s expected utility from escalation

$E^iU_{ij}(Esc)$ = *i*'s expected utility from not escalating

The following picture illustrates the differences in the behavior of the probability of escalation under the expected utility maximization assumption (Figure 1-a) and under the monotonic probability assumption (Figure 1-b).

Figure 1a. Probability Distribution Under Maximization

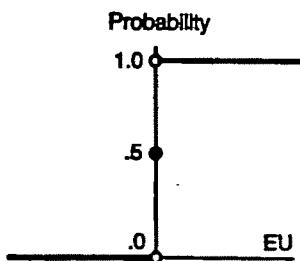
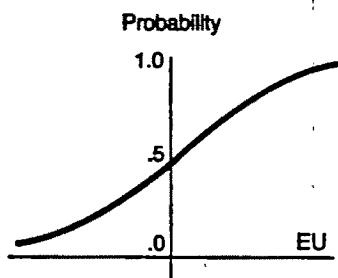


Figure 1b. Probability Distribution Under Monotonicity



Having established the differences in behavior implied by the two rationality assumptions, we note that the analysis in "Reason and War" can only be carried out under the monotonic probability assumption, making the maximization assumption altogether unnecessary.² Nonetheless, I shall make use of the latter assumption to illustrate a point.

The Necessary and Sufficient Conditions Reexamined

The authors make and prove the following most interesting proposition: "When the probabilities (of escalating a dispute) are monotonic with expected utilities the necessary and sufficient conditions for war/peace exist only in the limit." If no bounds are placed on the

values of the utility of the acts, the necessary and sufficient conditions are satisfied when the numerical value of expected utility approaches *plus infinity* for war or *minus infinity* for peace.³

The above proposition is indeed true, the reason being that the necessary and sufficient conditions for war/peace obtain only if probabilities of escalation equal one/zero, respectively. Under the monotonicity assumption, where no bounds are placed on the values of expected utilities, these probabilities are reached only in the limit when decision makers gain their maximum expected utilities.

In the absence of the above proposition, one might be tempted to conclude that in an environment close to that of certainty, that is, when winning and losing are known with probabilities almost equal to one and zero, respectively, the necessary and sufficient conditions for war and peace will tend to be satisfied. Such a conclusion, however, will be wrong. This can easily be verified by noting that expected utility is computed by multiplying probabilities and utilities, and since the probabilities are bounded between zero and one, the only way for expected utility to approach the limit—assumed to be large enough and possibly infinite—is to allow the utility parts of the computation to increase or decrease freely. For example, if

$$EU = \sum_{t=1}^k p_t u_t$$

then $\lim EU \rightarrow \infty$ [$-\infty$] only if

$u_p \rightarrow \infty$ ($-\infty$) for some u_p

and for all $u_s = u_p$, $u_s < M < \infty$.

k is finite.

The expression above holds true regardless of the values of p_t as long as these values are different from zero. Thus, if decision maker i is computing the expected utility from going to war with j solely on the basis of the probabilities and

utilities of winning and losing, then the expected utility is evaluated as follows:

$$EU(\text{war}) = \text{Prob}(\text{win}) \cdot U(\text{win}) + \text{Prob}(\text{lose}) \cdot U(\text{lose}).$$

Now, even if the probability of winning is very tiny, as long as the utility from winning approaches infinity and that of losing is bounded by some finite number, the decision maker will escalate with a probability equal to one. If the same conditions apply for all relevant decision makers, then the necessary and sufficient conditions for war are satisfied. Similarly, if the utility of losing approaches minus infinity and that of winning remains bounded by a finite number, the necessary and sufficient conditions for peace obtain even if each decision maker is almost certain of winning the war (i.e., with probability close to one). In general, it can be shown that for any probability distribution $p = (p_1, \dots, p_i, \dots, p_m)$ where $0 < p_i < 1$, and any real number L , there exists an appropriate utility vector $u = (u_1, \dots, u_i, \dots, u_m)$ such that $p \cdot u = L$. If the probabilities of winning and losing are viewed as a reflection of the power distribution in the system, then this result implies the generic nonexistence of power-based explanations of the necessary and sufficient conditions of war and peace.⁴

Now recall from Monotonicity and Maximization above that the one/zero probability is a feature of decision making under the expected utility maximization rule. Hence, in the limit, and only in the limit, do decision makers with monotonic probabilities of escalation act as if they are expected utility maximizers. Hence, the maximization assumption is also overly restrictive and of limited relevance to the analysis undertaken in "Reason and War." As is mentioned above, this assumption can be disposed of at no cost, and the entire analysis can be performed under the monotonic probability assumption.

The Opponent's Likely Behavior and the Decision to Escalate

Bueno de Mesquita and Lalman define $P^i(Esc_j)$ as nation i 's probability assessment of escalating a dispute against nation j , and $P^j(Esc_i)$ as nation j 's estimate of the probability that nation i will escalate. Then they give the following representation (under the assumption of monotonic probability):

$$P^i(Esc_j) = f(r \cdot \cos\theta) = f[E^i(U_{ij})] \quad (1)$$

$$P^j(Esc_i) = g(r \cdot \sin\theta) = g[E^j(U_{ji})] \quad (2)$$

$$\text{where } r = \{\{E^i(U_{ij})\}^2 + [E^j(U_{ji})]^2\}^{1/2} \quad (3)$$

The multiplication sign is \cdot , and θ is the slope of r .

Notice that since both equations are estimates made by i , the shape of the function g must be known to i . If i then contemplates escalating a dispute against j , i must not fail to use this information to make the final assessment on whether or not to escalate. At least, that must be the reason why i should take the trouble and bear the cost of computing j 's expected utility and guessing on the shape of j 's reaction function, namely g . Thus, a final (probabilistic) assessment by i to escalate—we denote it as $d^i(Esc)$ —should depend on i 's welfare as well as its estimate of j 's likely behavior, for example,

$$d^i(Esc) = d^i[E^iU_{ij}, g[E^jU_{ji}]] \quad (4)$$

Hence, the basic four equations that the authors of "Reason and War" constructed and tested may be rewritten as follows:⁵

$$P(\text{war}) = d^i(Esc) \cdot d^j(Esc) \quad (5)$$

$$\begin{aligned} P(\text{intervention}) \\ = & \{d^i(Esc) \cdot [1 - d^j(Esc)]\} \\ & + \{d^j(Esc) \cdot [1 - d^i(Esc)]\} \end{aligned} \quad (6)$$

$$\begin{aligned} P(\text{peace}) \\ = & [1 - d^i(Esc)] \cdot [1 - d^j(Esc)] \end{aligned} \quad (7)$$

$$\begin{aligned} P(\text{violence}) &= 1 - P(\text{peace}) \\ &= P(\text{war}) + P(\text{intervention}) \end{aligned} \quad (8)$$

A rather peculiar feature of Bueno de Mesquita and Lalman's representation of Equations (1)–(3) above is that it does not allow for the kind of formulation we present in Equations (5)–(8). Indeed, if we try to rewrite i 's probability of escalation—Equation (1)—as a function of i 's estimate of the opponent's likely behavior—Equation (2)—using the information in Equation (3), we end up with Equation (1) again. So the formulation of the first three equations leaves no chance for i to benefit from already available vital information about its adversary's likely behavior, and changes along lines similar to the ones suggested are needed. I should also note that in the empirical analysis of "Reason and War," $E^i U_{ji}$, which is simply $g^{-1}[P(Esc_j)]$, is treated as an indicator of expected costs. This usage is certainly defensible, but it also invites the question, How can i have an estimate of expected costs and yet not use this information to revise and update the decision?

Escalation in Extensive Form

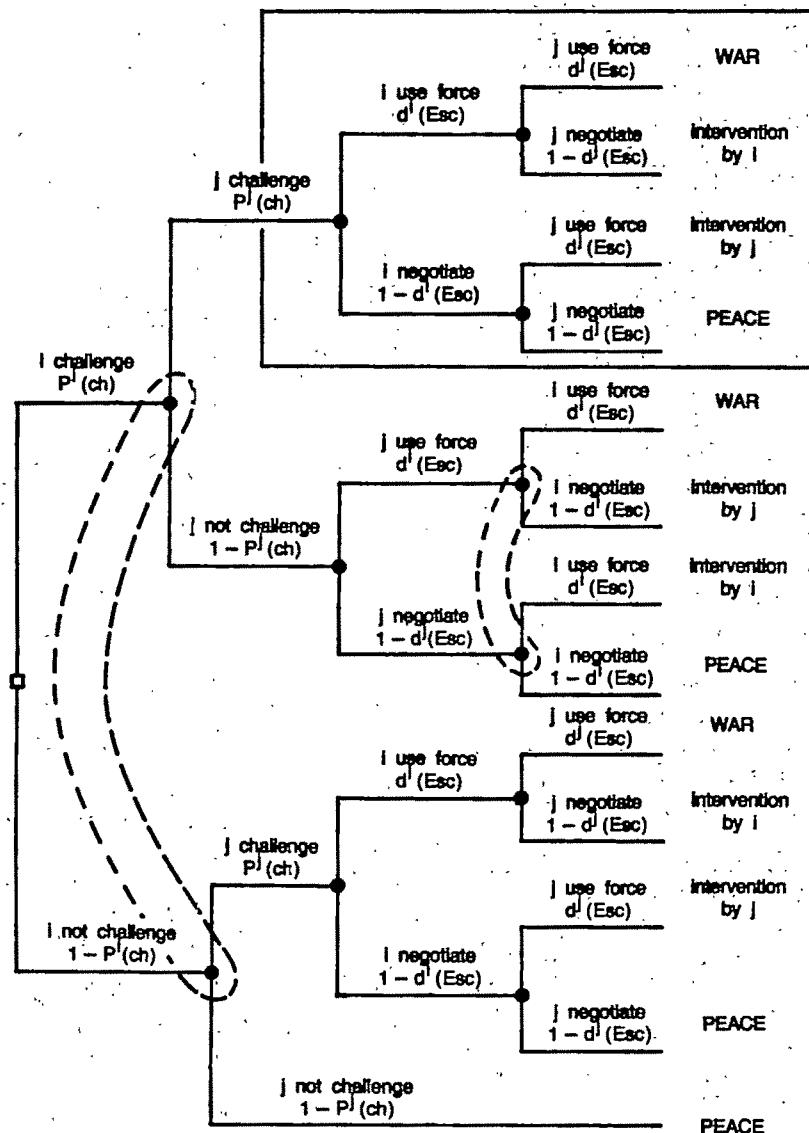
Now we turn to the last four equations, (5)–(8), and ask the following question: Are these equations acceptable after $P(Esc)$ is replaced with $d(Esc)$? The answer is mixed. On the one hand, our modified formulation does not go beyond a change of variables, leaving the original shape of the equations intact. Moreover, the original formulation, as well as the modified version seem to carry the right messages. On the other hand, these messages are incomplete. To illustrate, let us take Equation (7) and examine it along with Figure 3 in "Reason and War," which is the source of all four equations.⁶ In Equation (7), probability of peace is estimated as the result of both nations' choosing not to escalate the dispute in question. But, as

is clear from Figure 3, this assessment takes place only after i has decided to make a demand on j and a threatening one indeed. It is not unreasonable, however, to believe that peace may also prevail whenever i (and of course j) evaluates the expected utility from challenging the status quo and does not find it advantageous to do so. Hence, no demands are made. Therefore, the probability of peace should reflect not only the failure of a dispute to escalate into violence, which is what the authors of "Reason and War" offer, but also the probably more common situation in which both parties simultaneously decide that it is not in the interest of either of them to disturb the status quo.

To embody the above discussion in a probabilistic assessment of occurrence of peace as well as of war, intervention, and violence, we start by representing the problem of conflict escalation as a game in extensive form (Figure 2). This representation is most suitable for sequential decision making processes, when one of the players moves first and the other players move afterward. But in our case, we want to compute probabilities of outcomes generated by a sequence of events arising from either i or j challenging the status quo. This means that i and j must be allowed to move simultaneously. In a game in extensive form, such a situation is accounted for by allowing one of the players, say i , to move first and then not permit j to know the actual course of action i has taken. Hence the dotted information set around j 's initial move in Figure 2, which reflects j 's uncertainty about i 's decision. However, after j makes a move, all information is disclosed and the game proceeds from there with the appropriate actor making the third move. Thus if i challenges the status quo and j does not, then after the information is disclosed, it is j who must react to i 's attempt to upset the status quo. At this stage, j is allowed to use one of two strategies: the first is to use force to thwart i 's challenge,

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Figure 2. Escalation in Extensive Form.
The square in the upper right hand is equivalent to Figure 3 in "Reason and War."



and the second is to negotiate a way out. To this i responds either by using force to effect its desire or by negotiating an agreement with j . The probability of each action is depicted underneath that action in the decision tree. No payoffs are shown, only final outcomes: war, peace, or military intervention.

Two important points are to be emphasized. The first is that there is no reason to restrict the strategy set of each player to the binary choice of *either use force or negotiate*. Giving in to the opponent's demands is definitely an admissible strategy and so is a *blend of use of force and negotiate*. Outcomes corresponding to these alternative strategies can be different levels of violence and/or peace. I do not wish to complicate this brief note further by accounting for all admissible strategies. Nonetheless, a detailed study of war and peace must take them into consideration. The second point is that the final stage of the game can arguably take place in ignorance of the outcome of the stage that preceded it in that i and j may be making simultaneous decisions. To accommodate such possibilities, I depicted only one information set at the middle of Figure 2. The reader must decide for himself or herself if more are needed.

Now with the above discussion in mind and guided by Figure 2, Equations (5)–(8) can be rewritten, allowing the possibility of new interpretations for the probabilities of war, intervention, and peace. These are given below as equations (5')–(8'). To simplify notations I let p^i , d^i , and $(1 - d^i)$ denote the probability that i will challenge, use force, and negotiate, respectively. Similar expressions for j are needed.

$$\begin{aligned} P(\text{war}) &= [P^i \cdot P^j \cdot d^i \cdot d^j \\ &\quad + (1 - P^i) \cdot P^j \cdot d^i \cdot d^j \\ &\quad + (1 - P^i) \cdot P^j \cdot d^i \cdot d^j] \\ &= d^i \cdot d^j \cdot (P^i + P^j - P^i \cdot P^j) \end{aligned} \quad (5')$$

$$\begin{aligned} P(\text{intervention}) &= P(\text{intervention by } i) \\ &\quad + P(\text{intervention by } j) \\ &= [P^i \cdot P^j \cdot d^i \cdot (1 - d^j) \\ &\quad + P^i \cdot (1 - P^j) \cdot d^i \cdot (1 - d^j) \\ &\quad + (1 - P^i) \cdot P^j \cdot d^i \cdot (1 - d^j)] \\ &\quad + (P^i \cdot P^j \cdot d^j \cdot (1 - d^i) \\ &\quad + P^i \cdot (1 - P^j) \cdot d^j \cdot (1 - d^i) \\ &\quad + (1 - P^i) \cdot P^j \cdot d^j \cdot (1 - d^i)] \\ &= [d^i \cdot (1 - d^j) \\ &\quad + d^j \cdot (1 - d^i)] \cdot (P^i + P^j - P^i \cdot P^j) \\ &= (d^i + d^j - 2d^i \cdot d^j) \\ &\quad \cdot (P^i + P^j - P^i \cdot P^j) \end{aligned} \quad (6')$$

$$\begin{aligned} P(\text{peace}) &= [P^i \cdot P^j \cdot (1 - d^i) \cdot (1 - d^j) \\ &\quad + (1 - P^i) \cdot P^j \cdot (1 - d^i) \cdot (1 - d^j) \\ &\quad + (1 - P^i) \cdot P^j \cdot (1 - d^i) \cdot (1 - d^j) \\ &\quad + (1 - P^i) \cdot (1 - P^j)] \\ &= (1 - d^i) \cdot (1 - d^j) \\ &\quad \cdot (P^i + P^j - P^i \cdot P^j) \\ &\quad + (1 - P^i) \cdot (1 - P^j) \end{aligned} \quad (7')$$

$$\begin{aligned} P(\text{violence}) &= 1 - P(\text{peace}) \\ &= P(\text{war}) + P(\text{Intervention}) \end{aligned} \quad (8')$$

The following final comments are in order:

a. The *challenge* and *no challenge* options are usually treated as primitive alternatives, that is, expected utilities are computed over them and a choice is made to follow one or the other. Generally, no probabilities are placed on these alternatives, but it is no difficult matter to construct such probabilities.⁷

b. Equations (5')–(7') above are clearly more general than the corresponding ones in "Reason and War." In fact, with the proper specification, the latter equations can all be derived from Equations (5')–(7') above.

c. Although Equations (8) and (8') retain the same shape they had in the formulation in "Reason and War," the com-

ponents of each carry information quite different from that in the original one.

d. It must also be emphasized that Equation (7) computes the probability of peace between the pair i and j only. To find the probability of peace for the whole system, the computation of Equation (7) has to be performed on all pairs in the system; then all these computations have to be compressed in one final probability assessment. Similar expressions developed along the same lines generate systemic probabilities of war, intervention, and violence.

SEIF M. HUSSEIN

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Seif Hussein suggests modifications and expansions of the expected utility approach to international conflict. These enhance the generality and the interpretation of our propositions. He shows that weaker assumptions consistent with our approach yield the same predictions. Hussein notes that his points "are presented in order of increasing importance, thus the more significant points come later." Our response, then, begins with his more important statements and proceeds to those he considers of lesser consequence.

His four major arguments, in descending importance, are

a. *Escalation in extensive form* can account for all conditions leading to peace. "Reason and War" accounts only for alternative resolutions of conflicts, given that a crisis has occurred (Bueno de Mesquita and Lalman 1986; Lalman 1985).

b. The extensive form that he suggests can be used to define systematic relationships that underlie the opponent's response behavior.

c. The propositions in "Reason and War" provide an important additional

critique of power-based theories not drawn out in the article.

d. The *assumption of expected utility maximization* is superfluous for the main results in "Reason and War."

Escalation in Extensive Form

Seif Hussein offers several interesting extensions of the argument in "Reason and War." His Equations (5')-(8') are equivalent to Expressions (1)-(4) in "Reason and War" if one assumes, as we have in our paper, that leaders in nation i have already chosen to challenge j . Thus, Hussein's p^i (the probability of a challenge by i) is given as 1 in "Reason and War" since we examine only instances in which a conflict already exists. To show the equivalences, substitute the value 1 for p^i in Hussein's Equations (5')-(8'). These reduce directly to our equations (1)-(4). Equations (5')-(8') are more general than the equations that describe the same conditions in "Reason and War," permitting analysis of the likelihood of no crisis arising in the first place. That is, they include those instances of peace that occur in non-conflict cases.

Hussein's contribution is a direct theoretical extension of our model and as such is readily testable. By our strict monotonicity assumption, as Equation (1) from "Reason and War" approaches its minimum value, *not challenging* is increasingly preferred to *challenging* the putative opponent. Therefore, as the overall expected utility gets smaller for any nation i , the probability that no crisis will arise (i.e., that there will be the peace that follows the continuation of the status quo) increases. To test this extension, we create a random set of 240 ordered pairs of nations (i, j) such that i is a prospective challenger and j is the prospective opponent of i . The selection criterion is that i and j must not have been engaged in a threat, intervention, or war with one

another. Our expectation is that the mean $E^i(U_{ij})$ score for the randomly chosen i will be less than for those nations i engaged in any level of dispute (i.e., our data set of 133 conflict events used in "Reason and War"). A comparison of the mean expected utility scores for i in the two data sets reveals that the values in the nonconflict cases are smaller than those in the conflict cases. The t-statistic is 5.5, indicating that the difference in means is extremely unlikely to have occurred by chance. Thus, Hussein's extension is an application of the logic in "Reason and War" to the problem of conflict initiation, a subject not addressed in our paper. The result reported above highlights, in a preliminary way, the potential of our model to account for initiation as well as escalation of disputes. In fact, initiation is examined in *The War Trap* (Bueno de Mesquita 1981) and is the subject of work in progress by us. Hussein has anticipated our extension of the model to nonconflict cases.

Opponent's Response Behavior

Hussein's criticism that i inadequately takes into account the expected behavior of j is correct as far as it goes. His reasoning is appropriate to a sequential game, while our paper is about the initial decision by i or j to escalate. It is not about subsequent rounds of negotiation or escalation. In our paper we allow some updating of i 's decision through the mechanism of controlling for i 's belief about j 's expected utility relative to i 's. Thus we introduce dummy variables to identify in which octant the ordered pair of expected utilities $[E^i(U_{ij}), E^j(U_{ji})]$ fall and use that information to estimate the costs i expects j will inflict on i . We agree that j 's position is important in the unfolding of a conflict but disagree over its significance for i 's initial decision to escalate. Consider the payoff matrix below:

Table 1. i 's Approximation of j 's Initial Choice

i 's Initial Choice	Use Force ($Prob = P$)	Negotiate ($Prob = 1 - P$)
Use force	$P(F_i - C_i)$	$(1 - P)F_i$
Negotiate	$P(N_i - C_i)$	$(1 - P)N_i$

where F_i is i 's expected utility from choosing the *force* strategy, P is the a priori probability i attaches to j 's selecting its force strategy, C_i is i 's estimate of the cost to i if j chooses the *force* strategy— $g[E^i(U_{ij})]$ from "Reason and War"—and N_i is i 's expected utility from choosing the *negotiate* strategy. A similar set of payoffs could be specified from j 's perspective. Now if i chooses *force*, the expected value will be

$$PF_i - PC_i + F_i - PF_i = F_i - PC_i. \quad (1)$$

If i chooses the *negotiate* strategy, the expected value will be

$$\begin{aligned} PN_i - PC_i + N_i - PN_i \\ = N_i - PC_i. \end{aligned} \quad (2)$$

i 's choice of strategy is based on

$$\begin{aligned} (F_i - PC_i) - (N_i - PC_i) \\ = F_i - N_i. \end{aligned} \quad (3)$$

Therefore, taking i 's perception of j 's expected utility into account, we see that in the *initial* choice of strategies by i , only i 's expected utility and not i 's view of j 's expected utility affect i 's decision to use force or negotiate. This is as it should be since the expected utility estimates are constructed as net changes in welfare, given that the potential opponent is willing to use all of its capabilities to inflict costs on i (as indicated in the specification of the value of P in "Reason and War"). We treat i as updating its estimate of likely costs away from the worst case scenario through the use of $E^i(U_{ij})$ to determine in which octant the dispute lies,

given $E^i(U_j)$. The logit coefficients for the cost variables in each octant are empirical approximations of the function g .

In later research we plan to move toward a sequential decision analysis to develop theoretically determined estimates of g . We share Hussein's view that this would be superior. The form of g and how i evaluates the information provided by g are not yet determined either by us or by Hussein.

Critique of Power-based Theories

Hussein provides a demonstration that Propositions (1) and (1a) of "Reason and War" indicate why power-based theories of international conflict are inadequate. This is a useful elaboration that reinforces arguments we have made elsewhere (Bueno de Mesquita 1981a; Lalman 1985). Indeed, in "Reason and War" we note that "the simple theoretical structure set out here and elaborated more fully in Lalman (1985), encompasses structural theories of power distributions, arms races, deterrence, negotiations, bluff, and escalation." In subsequent work, we plan to pursue more thoroughly the implications of the expected utility approach for power-based and other theories of conflict.

Maximization Assumption

Hussein has very cleverly demonstrated that the assumption of expected utility maximization is a proper subset of the assumption of monotonicity. Therefore, our assumption of strict monotonicity—accompanied by our implied assumption that the monotonic relationship between expected utility and the probability of escalation is differentiable—is sufficient to deduce the results in "Reason and War." However, when investigating conflict initiation and other dimensions of conflict decision making—all of which are

part of our larger research program—the maximization assumption is still useful.

The monotonicity assumption is much less restrictive than the assumption of expected utility maximization, while still being consistent with rationality under the following conditions:

a. If i is rational in the expected utility sense, then when faced with discrete strategic choices, i selects the better (i.e., larger expected utility) option for sure. However, i does not know the precise shape of j 's utility function and so can only guess probabilistically at what strategy j will choose (see Table 1). That probabilistic calculation is assumed to be monotonic and differentiable with i 's estimate of j 's expected utility (and with j 's estimate of i 's expected utility).

b. We, as researchers, can only estimate i 's and j 's expected utility values. Therefore, our predictions of their behavior are probabilistic (monotonic and differentiable) with our estimates of their expected utilities.

What is gained with the monotonicity assumption is the ability to explain variations in choices within the ordinal categories identified in Proposition 2 in "Reason and War." Hussein's comments highlight the fact that stronger theoretical and empirical results have been obtained while employing the weaker assumption that the choice of a strategy is a monotonic and differentiable function of the utility expected from that choice.

The thrust of Hussein's comment, our response, and the body of literature using expected utility theory to account for international conflict is that such an approach is moving toward an axiomatic understanding of the subject. This being the case, we can evaluate the impact of individual assumptions on subsequent deductions. In this way we can weaken assumptions, helping to make the results even more general. Hussein has pointed the way toward making improvements

along this line without altering the fundamental aspects of our approach.

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Notes

1. To be more precise, the term *monotonicity* is used to mean that the probability of escalation is strictly increasing and twice continuously differentiable in its argument. See Lalman 1985.
2. From a technical point of view a weaker form of the monotonicity axiom can be satisfied by the maximization assumption. But that would be at the expense of the continuity of probability estimates. Continuity and monotonicity are indeed the driving forces of "Reason and War."
3. One can indeed place bounds on the upper and lower limits and the proposition will still hold true.

4. Similar results can be found in Bueno de Mesquita 1981 and Lalman 1985.
5. These formula also appear in Lalman 1985.
6. Figure 3 from "Reason and War" is depicted as part of Figure 2 below.
7. See Hussein 1986 for such a construction.

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RESEARCH NOTE

RECRUITMENT CONTRASTS IN A DIVIDED CHARISMATIC MOVEMENT

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This research report analyzes the evolution of the bond between a charismatic leader and his mass following. Its empirical focus is the case of Juan Perón and the Peronist movement in Argentina. After discussing stages in the evolution of charisma, we review our earlier findings showing that by 1965 there was clear separation between those Peronists devoted to the movement and those Peronists devoted to the man. We note distinctive features in the social and attitudinal profiles of each and then turn to our principal concern, the recruitment of new blood into each Peronist camp. Personalist youth turn out to be much like their older counterparts, but organizationalist youth demonstrate social and attitudinal features that set them dramatically apart from the personalists and apart from the older organizationalists as well. We discuss the implications of this ever widening rift.

Many fascinating and important phenomena in political life, though in principle available for empirical study, in practice present intractable problems to the would-be systematic investigator. As a result they are left in a condition of scientific limbo sometimes described as theory-rich but data-poor. Political charisma is one such phenomenon. The difficulties its study presents to the empiricist are formidable and doubly so if the charisma in question involves mass publics rather than small groups. How can one collect crucial attitudinal data in such an intensely emotional setting? How can one systematically observe the evolving bond between leader and mass? In this light it is not surprising that empirical work on political charisma is so very meager. This research note comes from a larger study that will provide data-based—though necessarily still preliminary—answers to some impor-

tant questions about that phenomenon. The study has as its empirical focus Argentina's Peronists and their leader, Juan Domingo Perón.

In an earlier report, we laid out our theoretical perspectives, data sources, analytic methods, and three key findings: (1) two variants of Peronism had emerged after Juan Perón's initial period of power, one faithful to the man, the other more devoted to the movement; (2) these two wings of Peronism had different social and attitudinal bases; and (3) to a significant extent the social base of a wing predicted its attitudinal base (Madsen and Snow 1983).

In this note we take up recruitment. Where does each of the two variants of Peronism find its new blood? What factors drew new members into each of the two wings? Casting these questions into stark relief is the fact that by the time of Perón's return to power in 1973, the two

wings of Peronism were in full-scale combat. Indeed, the airport fighting between them at the time of Perón's intendedly triumphant return to Argentina has been popularly labeled the Ezeiza Massacre (Page 1983, 579). What had happened?

Some Background

Juan Perón was a little known member of the military circle that seized the Argentine government in 1943. This coup came after a period of economic crisis marked by major decline in the all-important agricultural sector, by massive migration to crowded and job-short urban centers, and by poverty and despair among urban workers. Perón became secretary of labor in the new government and for two years, with wide publicity and great drama, used his powers to transform the lives of workers and their families. Not surprisingly, he acquired a passionate following. Threatened by Perón's ascendancy, the army arrested him in October 1945; an act that brought tens of thousands of his supporters marching in massive columns from the outlying slums and industrial suburbs to the center of Buenos Aires. They demanded and obtained his release.

For the next decade, as master of Argentina, Perón led the government. He also transformed the structural bases of Argentine politics, creating and nurturing Peronist party and trade union apparatuses and bringing many previously unrepresented elements (including women) into those organizations. In 1955, growing disputes with the Church (from which he was excommunicated) and with the armed forces, coupled with economic reversals, led to a coup that forced Perón into exile.

Even while in exile, Perón continued to be the major factor in Argentine politics. Leaders of Peronist organizations streamed back and forth across the Atlantic, seeking guidance and bringing back

policies and taped speeches for the faithful. Although the Peronist party had been declared illegal, Peronist trade unions continued to function. Eventually, in the congressional elections of 1962 and 1965, Peronist candidates were allowed, but they could not be identified as such on the ballot. The results of both these elections showed Peronism still to be the nation's largest party, and both led to military coups. In 1973 the Peronists were allowed to run a candidate for the presidency, as long as that candidate was not Perón himself. A stand-in named Héctor Cámpora ran with the slogan "Cámpora to the government, Perón to power." He won, and three months later resigned, necessitating new elections. This time Perón was nominated, and he won with 63% of the vote. He returned to Buenos Aires on 23 June 1973 to the sound of gunfire. His followers were killing one another.

There can be no surprise in a discovery that in the 30 years Perón held center stage in Argentine political life, his bond with his followers had changed. After all, the Peronists, from an inchoate though passionate agglomeration had become a structured movement. Perón himself, in government and even more in exile, had to rely on lieutenants and agents—intermediaries who carried their version of his Word to the faithful and who put their version of his policies into effect. Argentina's economy, though having ups and downs, had left behind the dismal conditions under which Perón's charismatic leadership began. And, of course, the very personnel of Peronism had changed as old followers dropped out or died and new ones were recruited.

Edward Shils (1958) has argued that a charismatic bond inevitably evolves from a direct, concentrated, and emotionally intense form at its birth to an indirect, dispersed, and less passionate one as it ages. If this is true in general, what would the particulars be in the Peronist case? What sorts of followers would still have a direct

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and emotional tie to Perón as a man? What sorts would be more attached to Peronism as a movement?

In seeking to answer those questions, we were fortunate to obtain data from a national survey taken almost exactly midway between Perón's expulsion from Argentina and his triumphant return. (The methods of the survey are described in Kirkpatrick 1971.) In such a serendipitously timed—though, it turned out, still far from ideal—set of observations, we had a unique resource within which to examine the status of Peronism twenty years after its inception.

The survey data made it clear that the Argentine public was making a distinction between Perón and Peronism. Those items dealing explicitly with Perón himself exhibited one pattern and those dealing with the movement quite another. Factor analysis (with an orthogonal rotation) was ultimately used to construct measures of personalist and of organizationalist tendencies. And then 100 respondents were drawn from the uppermost location on each factor, yielding two pure groups for further analysis. Our pure personalists were unwaveringly committed to Perón but typically rather lukewarm to the movement. Our pure organizationalists were strongly committed to the movement but on average somewhat negative in their feelings for the man. But remember that in both cases these attachments were (by definition) affective and symbolic; they do not involve formal membership of any kind.

The socioeconomic status (SES) differences between personalists and organizationalists are striking. The former were far more likely to be residents of small towns or recent migrants to cities. Most interesting, they were, in SES terms, very much worse off than were the organizationalists.

Temperamentally, the personalists were more convinced of the value of strong leaders, more acquiescent to

authority, and more confused about politics and government. This profile surely holds no surprise for students of authoritarian politics. And recall, this is a profile that correlates highly with the socioeconomic profile—that is, the latter substantially predicts the former in a statistical sense. These results leave the strong impression that in our personalists we have people much like those who in the 1940s found their savior in Juan Perón. Moreover, they seem most unready to give up their attachment to their leader. The forces behind the dispersion of Peronist charisma would seem most unlikely to flourish here.

The organizationalists were not only better off in SES terms but were also distinctive attitudinally; notable was their more conventionally "leftist" response to worker and communist symbols.

Recruitment Contrasts

The contribution of this note is to add consideration of recruitment processes to our analysis of the evolution of charisma. What sorts of youth were coming into each wing of Peronism? Was the social and psychological makeup of the recruits like that of their older counterparts, suggesting a stable gap between the two groups, or was that makeup different?

We assumed that the conditions leading large numbers of people to seek a personal savior in political life would be much the same, whether they were young or old—or, for that matter, whether they were 1965 seekers or 1945 seekers. We were more uncertain about the organizational young. No a priori reason led us to expect them to differ greatly from their older counterparts, one way or the other.

Before turning to the evidence, we must insert an important caveat. The data speaking to our recruitment questions are much less firm than we would have liked. Using an age division point of 30 years, we have one-fifth of our 100 cases in each

youth component. Thus, we are here examining 20 cases of personalist youth and about the same number of organizationalist youth. While these figures do not seem unrealistic in terms of population parameters, they do illustrate the problem with using general population samples in analyses of subgroups. The evidence presented below must be considered in that light. Our own view is that the persuasiveness sacrificed by the data in purely statistical terms is substantially recovered by two other tests of data adequacy: strong empirical patterns and a clear fit with other sources of evidence (see di Tella 1983, chap. 2; Gillespie 1982; Guimaraes 1982; Page 1983, chap. 47; and Smith 1982).

Socioeconomic Differences

In our youth groups we have individuals who were no more than 10 years old and in some cases yet unborn when Perón's initial bond with his following was formed in 1945. They were between 8

and 20 when he went into exile. How does their socioeconomic makeup compare with that of their older counterparts?

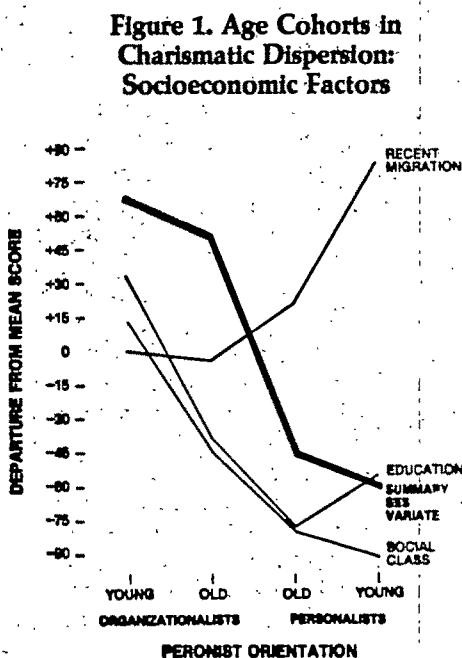
The personalist young and old were, as expected, much the same. It is true that the young were a bit poorer and a bit better educated, but the only striking difference had to do with recent migration, which was much higher for the young than for the old (who were themselves well above the population average).

With respect to the organizationalists, in most of the social and economic conditions of young and old, we found a reasonably close match. But there was one astonishing exception: the young were of much higher social status, scoring even above the average for the Argentine population as a whole, while their older counterparts scored well below average! In class terms, then, these young organizationalists were very different indeed.

In Figure 1 we present the key findings. Given in bold relief is the line of position for each group on a summary socioeconomic measure (standardized across the 200 Peronist cases to produce a mean of zero and a standard deviation of one). This measure, called a *canonical variate* in technical language, was derived from an earlier discriminant function analysis in which weights were assigned to social and economic variables on the basis of their capacity to discriminate between personalist and organizationalist Peronists (full details are given in Madsen and Snow 1983). Note that in this weighting process, age was not involved.

On this summary measure, we can see in Figure 1 that the two youth groups are even farther apart than are their older counterparts—a finding that portends not stability or convergence but rather a growing gap between the two wings of Peronism. In their social foundations they apparently will have less and less in common.

The particulars of this growing gap are also to be found in Figure 1. The three factors most powerful in differentiating the



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newer recruits from the older members are recency of migration, social class, and education. (In the figure, each of the three has been standardized across the entire sample, which permits group scores to be compared with population averages, as well as with each other.)

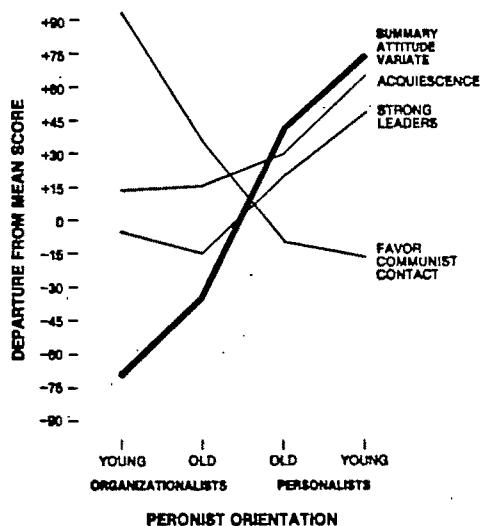
Psychological Differences

The temperamental differences between young and old personalists can be quickly summarized. The young felt slightly less able to understand politics (in spite of greater education); they were much less likely to promise support to political candidates from the working class; they were much more supportive of a strong leader in politics; and they scored much higher on a measure of acquiescence to authority. If personalists in general found happiness with a *leader*, these young personalists must have found rapture!

A comparison of young and old organizationalists on the attitudinal items revealed one remarkable difference: the young gave solid support to a policy of friendly Argentine relations with Cuba and with unspecified "communist countries." As noted earlier, the organizationalists had already been found to be more leftist than the personalists, but here we find the young going far beyond the position of the typical older organizationalist. Such a result is all the more striking when one realizes that this leftward drift could find no sanction in Perón's own rhetoric; indeed, quite the opposite.

In Figure 2 the attitudinal findings are given. Again presented in bold relief is the line of position for each group on a summary canonical variate, the one built from the attitudinal items that differentiate personalists from organizationalists. Plainly, the positions of the two youth groups here reinforce the impression given by the socioeconomic comparisons: if these two groups represented the future of Peronism, a deep schism in the movement was assured.

Figure 2. Age Cohorts in Charismatic Dispersion: Attitudinal Factors



Discussion

The evidence presented here suggests the possibility of a development in the evolution of Peronism that in fact occurred seven years later: Peronists warred with Peronists. The character of the warring camps is quite well predicted from the results given above. This remarkable fit with subsequent events adds a good deal of interest (and additional strength) to our findings.

One might ask, however, what the Peronist case teaches us about charismatic movements in general? If we can say that the nature of the original Peronism was certain to change, that inevitably there would come greater differentiation within the movement, can we also say why the change took the shape it did? Was simple disintegration equally likely? Are there general lessons about the role of new blood in the dispersion of charisma that can be drawn on the basis of these findings?

We believe our findings on the personalists to be general. The main point is simple: people in crisis are ready for—and sometimes actively seek—a personal savior; once they have found one, they do not easily let go. Of course, crises take many shapes, sometimes being more microscopic in their character and other times more macroscopic. Social scientists have a tendency to notice only the latter.

Personalists found in Juan Perón the answer to their own crises (which were triggered by macroscopic events and hence had macroscopic scope). Organizationalists, on the other hand, having escaped those circumstances—or, in the case of the young organizationalists, never having faced them—saw Perón himself in a more dispassionate light. For them, the charisma had been dispersed.

If this is correct, it suggests that all charismatic movements must founder in the face of success. When the crisis has passed, for whatever reason, the followers regain their sense of control, their sense of personal efficacy, and the leader recedes into the psychic shadows. However, any structural residue from the charismatic period may by that time have gained bureaucratic life of its own and survive.

Of course, none of this tells us why the young organizationalists were moving to the left. Obviously, there is no answer to be found in our data. One might speculate, however, that the problems confronting Argentina in the 1960s (e.g., economic stagnation, disastrous inflation, political states of seige, and military repression) must have fed a disposition to

seek fundamentally new economic and political arrangements. In that context, especially for activist middle-class youth, there must have been special appeal in a revolutionary ideology that combined a powerful world view and an attractive idealism on the one hand and a "scientific" rejection of discredited institutions and practices on the other.

Note

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REVIEW ESSAYS

Britain against Itself: The Political Contradictions of Collectivism. By Samuel H. Beer (New York: Norton, 1982. xvi, 231p. \$18.95, cloth; \$5.95, paper).

The British Prime Minister. Edited by Anthony King (Durham: Duke University Press, 2d ed., 1985. xiv, 275p. \$27.50, cloth; \$12.95, paper).

Reagan, Thatcher, and the Politics of Decline. By Joel Krieger (New York: Oxford University Press, 1986. xv, 320p. \$29.95, cloth; \$11.95, paper).

Dilemmas of Change in British Politics. Edited by Donley T. Studlar and Jerold L. Waltman (Jackson: University Press of Mississippi, 1984. xiv, 251p. \$17.50, cloth).

The study of British politics is in a remarkably creative period of self-conscious rethinking and reappraisal. As the old verities of British politics have crumbled over the last two decades, political scientists have had to rebuild their understanding of a rapidly changing political order. The years since the late 1960s have been traumatic ones for several western democracies, but no western democracy has experienced a greater shift in self-perception than Britain. In the 1950s Britain was regarded as a paragon of political consensus and effective government; by the 1970s Britain had been transformed into a negative model of political discord and pluralistic stagnation.

Our understanding of British politics must encompass changes reaching into every sphere of political life: decreasing political trust, deference, and consensus; the diminishing salience of class alongside increasingly unpredictable patterns of voting; a rapid decline of Conservative and Labour duopoly of electoral support and a loosening of their grip on parliamentary representation; an intensification of ideological dispute between, and especially within, the major parties; mistrust and bitterness in industrial relations; a nationalist challenge to the unitary state; and a unitary assault on local government autonomy. These developments have taken place at a time of fundamental reorientation of policy priorities and insistent demands for major institutional

change. The vaunted capacity of the British to avoid hard choices by finding a path of least resistance now appears more a liability than a strength.

How do we make cumulative sense of these developments? What are their underlying causes? What do they portend for liberal democracy in Britain?

These questions are met head on by Samuel Beer in *Britain against Itself*, published in 1982, but not previously reviewed in *APSR*. Beer's thesis is, at bottom, a cultural one. He argues that the fundamental source of the transformation of the British polity is an unprecedented decline in the civic culture, the balance of modern participatory orientations and traditional deference, civility, and trust that underpinned political stability and governmental effectiveness in collectivist Britain. In thrusting political culture to the foreground of his explanation, Beer is continuing a line of idealistic analysis that he developed in his immensely influential book, *British Politics in the Collectivist Age*. The result is an original and challenging analysis of what Beer calls the "new populism," an assertion of values sympathetic to radical individualism, popular participation, and decentralization and antagonistic to traditional hierarchy and authority. Beer traces the origins of these values in the New Left and mass culture of the 1960s and explores their expression in electoral behavior, intra-party conflicts, back bench assertiveness in the House of Commons, and shop floor activism in industrial relations.

By relating change in British politics to a common source—the new populism—Beer brings a satisfying measure of coherence to his explanation. At the same time, he avoids reductionism by emphasizing the unique implications of the new populism in each of the spheres he describes. But the reader can hardly avoid the conclusion that populism means very different things to different people. For those on the left of the Labour party, it is expressed in demands for the democratization of government, the Labour party, and industry. For neoliberals in the Conservative party, populism underlies the attack on traditional

Tory commitments to *noblesse oblige* and hierarchy. Within the mass culture of the 1960s, populism is seen in the Edenic imagery and romantic individualism of popular rock music. But if we cannot distill the consequences of the "new populism" into a consistent formula, this is more an inevitable result of the complex cultural analysis that Beer is engaged in than it is a sign of muddled thinking. To understand the genesis of political conceptions about society—which is ultimately Beer's project—we are compelled to rely more on interpretation than demonstration. In the end, the litmus test of the analysis is not whether it meets formalistic methodological criteria but how it sheds light on the course of cultural tension and change in Britain, and on this score Beer's study is admirably suggestive.

The most original—and contested—aspect of Beer's thesis is his attempt to show that the origins of the new populism are to be found in the counterculture of the 1960s, and particularly in the lyrics of the Beatles' songs. The Beatles were, he suggests, the "unacknowledged legislators" of Britain's populist revolt: "They did not come out for participation or decentralization in so many words, but the influential message of their songs was one of intense individualism and antihierarchy, romantic disavowal of customary social constraints, an appeal to universal human feelings and a pervasive "sense of place." To this writer, Beer's discussion of the Beatles' lyrics is subtle and revealing; other commentators have complained that the evidence Beer marshals is impressionistic and unconvincing. Whatever our view of the validity of Beer's thesis, he has uncovered an immensely important topic. At the very least, this book should precipitate further investigation of the effects of mass culture on political culture. Beer's argument rests mainly on textual analysis; there is a complementary need for systematic study of the ways in which such texts were actually experienced and interpreted.

Beer is virtually alone among established scholars of British politics to investigate seriously the roots of cultural and political change in the popular music of the 1960s. Casting aside the fear of ridicule by those who intone that an analysis of the Beatles' lyrics "cannot be taken seriously," Beer has, once again, extended our understanding of the sources of political culture. In this respect his

work is complementary to that of Richard Hoggart or Raymond Williams on popular literature, language, and thought.

If the civic culture has indeed collapsed, as Beer claims, the consequences for liberal democracy are likely to be serious. In their classic comparative study of political culture, Almond and Verba argued that a viable and stable democracy was possible only if the participant-rationalist model of citizenship was balanced with passivity, trust, and deference to authority. Yet for all the pluralist stagnation that Beer diagnoses in Britain, the foundations of liberal democracy seem secure. When we view Britain's present troubles from the perspective of the twentieth century as a whole, the recent urban riots, industrial disputes, intraparty conflicts, and proposals for constitutional change are hardly new in scale or intensity. Unfortunately, we lack a sufficiently distant baseline for diagnosing the severity of recent cultural change. The initial use of systematic techniques for analyzing political culture coincided with an extraordinary period of stability and prosperity in the late 1950s. Those balmy days have gone, perhaps forever, and it is not really surprising that this is reflected in a deterioration of the civic culture, but whether this amounts to an historic collapse must remain an open question. If trust and confidence in political institutions are at all influenced by their perceived performance, the 1950s may just as likely represent an unprecedented high point for the civic culture as the 1970s an historic low.

Alongside this cultural analysis, Beer poses complementary structural and class-based explanations for the failure of the political process in Britain. In his discussion of the structure of decision making, Beer's unifying theme is the prisoner's dilemma, where rational strategy on the part of isolated actors leads to outcomes where all are worse off. Beer diagnoses this situation in several fields of political decision making, from the self-defeating competition for resources among trade unions and subsidized functional groups to bidding between parties for electoral support and the predisposition of government ministers to defend their own departmental budgets irrespective of broader policy priorities. Beyond the varieties of immobilism at different levels of British politics, Beer argues that there is a common inner logic of decisional

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frustration. In each case, the sheer number of decision makers, their mutual isolation and lack of trust make it difficult for them to cooperate in achieving collective benefits. None of these individuals or groups, according to this argument, will have any material incentive to restrain their economic demands from "the commons" if they believe that their own restraint will not be reciprocated by those in a similar position.

Beer argues persuasively that the prisoner's dilemma has become a more accurate model of political decision making in Britain as nonmaterial incentives for self-restraint, based in traditional social norms and class solidarities, have lost ground. The rise of populism has undermined hierarchical and organic values; class decomposition has intensified group rivalries; the sheer number of interest groups has greatly increased. On the one side, the capacity of the collectivist state to aggregate interests has diminished; on the other, interest groups themselves have become more fragmented. Functional interest groups, voters, and parties find themselves imprisoned in a system of self-defeating competition that punishes isolated attempts to cooperate.

Beer draws on the logic of the prisoner's dilemma to produce an elegant account of Britain's political problems. It also confirms his conclusion that the solution to Britain's political problems must involve the restoration of nonmaterial incentives for creating common benefits; in short, there must be a restoration of political trust. Beer views the Social Democratic party as the best agent for this, because it has the potential to adapt the disintegrative force of the new populism to creative reform and the renewal of legitimacy in Britain's political institutions.

One of the most outstanding virtues of *Britain against Itself* is that it encompasses structural, social, and cultural factors within a coherent explanatory framework. Beer recognizes that no one of these factors can account for the failure of political decision making in Britain, and he manages to combine them in convincing fashion. Each, Beer argues, may be seen as an outcome of the contradictions of collectivism. The new populism, with its emphasis on radical individualism and participatory democracy, is a reaction against the faceless impersonality and functionality of collective representation. Class decomposition

is intensified by the preponderance of functional groups rather than classes as a channel for expressing economic demands, and pluralistic stagnation is exacerbated both by Hobbesian competition among numerous producer groups within the managed economy and the lack of any self-imposed budgetary constraints among individual voters in their response to party promises. Implicit in this analytical framework is the sensible supposition that we must combine cultural analysis with structural and sociological approaches to gain an open-textured understanding of political change in Britain.

Virtually absent from the picture, however, is any sustained discussion of the role of economic factors, an omission all the more surprising in light of the dramatic relative decline of Britain's economic fortunes in these decades. Beer explains in the introduction that his concern is with the way in which decisions are made rather than the substance of the decisions themselves. In neglecting public policy, Beer is true to his professed aim, to restrict his analysis to the political process itself. But the question we may ask is whether it is meaningful to analyze the process of political decision making in isolation from its performance, or, to put it epigrammatically, whether in this case it makes sense to sever the "how" from the "who gets what" of politics. Beer's determination to focus on the one and ignore the other leads him to some one-sided conceptualizations. For example, political distrust is treated as the lack of correspondence between the way public choices are made and the normative standards regarding them. This is sensible as far as it goes, but especially in the case of Britain, it seems necessary to make conceptual space for political distrust that arises in the tension between expectations of government and its performance.

The transparent failures of economic policy and the realization that they were undermining the postwar consensus profoundly influenced British politics in the 1970s. The remedies that were sought in these years shaped the process as well as the content of policy. Sometimes the effects were unintended. For example, incomes policy, which was designed to reduce wage inflation, actually intensified wage competition and extended the scope of state involvement in the labor market, while diminishing its legitimacy. Incomes policy and pluralist stag-

nation, policy outcome and the political process, were so closely intertwined that we can usefully think of them as existing in a dialectical relationship, each acting on the other. Mrs. Thatcher's policy of excluding union leaders from government councils, avoiding any explicit or compulsory incomes policy, and relying instead on very high unemployment, seems to be a conscious response to the pluralist stagnation that was diagnosed in the 1970s. Here, then, is an instance in which the causality is reversed; the political process is the object, and economic policy becomes the lever.

One must tread cautiously in criticizing a pathbreaking book because of its omissions. It is an irony of our enterprise that the more precise, coherent, and elegant an explanation, the more it invites criticism as being incomplete. By any standards, *Britain against Itself* is an acutely conceptualized and superbly written analysis of Britain's recent political problems. Samuel Beer has that rare intellectual ability to distill the essential features of a complex situation into an explanation that makes sense. The result is a book that is as useful for the undergraduate as it is for the specialist in British politics.

In their edited book, *Dilemmas of Change in British Politics*, Donley Studlar and Jerold Waltman have brought together eight previously unpublished articles concerning the experience and future possibilities of reform in British political institutions. Given the breadth of topics covered—Parliament, the civil service, the party system, local government, devolution, the referendum, the European community, and the courts—the contributing authors show a remarkable degree of agreement that British political institutions are over-centralized, exclusionary, and excessively rigid. They follow Samuel Beer in emphasizing the rapid emergence of popular demands for decentralization and greater participation yet find little evidence of institutional adaptation to such demands. Pragmatism, moderation, compromise—values that were once seen as contributing to the vaunted flexibility of the British constitution, are now viewed as leading to *ad hoc* improvisation and immobilism.

With customary wit, Jorgen Rasmussen asks in the title of his chapter if Parliament is revolting but actually presents a historically informed analysis of the representation of constituency interests in the House of Commons and concludes, with considerable force, that

the emergence of highly disciplined parties in Britain in the late nineteenth century was and is inimical to the expression of local political diversity. This provides a justification for devolution to elected national assemblies in Scotland and Wales. But Rasmussen also stresses that the two major parties are unrepresentative of constituency interests within England itself, and, to cope with this, suggests a variety of far-reaching reforms including primary elections.

If the problem in constituency representation is party-induced rigidity, that within the civil service is, according to James Christoph, policy drift beyond the control of well-intentioned reformers. Even when institutional reforms have been deliberately planned and implemented, the intended consequences are confounded by external events. As Christoph observes, the Fulton-style reforms of the 1960s were either stifled by civil servants themselves or overshadowed by broader developments. The most important of these was the expansion of unionism into the middle and higher levels of the civil service, as public workers struggled against compulsory incomes policies and cash limits.

The theme of superficial change in an inflexible political system is also taken up by Douglas Ashford and Jerold Waltman. Ashford documents the follies of institutional reform at the local level and the self-centered and ultimately self-defeating partisanship of both Labour and Conservative governments. Lost in all of this, according to Ashford's astringent commentary, is any sense of the responsibilities of central government to local interests and aspirations.

Waltman finds that since the 1960s there has been a slight broadening of the scope of judicial review of administrative action in response to the arguments of some leading activists, notably Lord Denning, Master of the Rolls until 1982. But Waltman also recognizes that the enactment of a Bill of Rights establishing the rule of law as a curb on Parliament faces enormous political obstacles.

Chapters on the party system, devolution, the referendum, and the European community by Kenneth Wald, Norman Furniss, Harry Lazer, and Hans Michelman, respectively, emphasize fundamental continuity against a background of changing attitudes and institutional tinkering. The picture we are left with is one of archaic, centralized, and inflexible

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institutions unable to adjust to changing demands. A variety of reasons are suggested for this, but, as Donley Studlar points out in the conclusion to this volume, the most consistently cited culprits are the major parties and the centralizing tendencies of cabinet government.

Such criticisms are anything but new. Despite long-cherished notions of incrementalism and consensus as the guiding principles of British political development from the nineteenth century, political reform proceeded through a series of unpredictable, dissentient, jarring episodes motivated by fundamental critiques of political institutions and the hint—and sometimes the practice—of political violence. In the nineteenth and twentieth centuries, British institutions were not stirred around by military defeat, civil war, or revolution. Reform has had to come by breaking in or pushing very hard from the outside.

Once, not too long ago, U.S. observers were singing the praises of the British polity. Now they are among its harshest critics. Most of the essays in this collection are constructive. They begin with the presumption that more decentralization and more participation would be good and go from here to analyze the limitations of British institutions and occasionally suggest remedies. Few nowadays would take issue with the value of more decentralization and participation, but before setting out on the reconstitution of the British polity it would be useful to have some firm idea of the possible consequences of this for the regime itself. Moreover, unless we believe that all good things go together, we are faced with the prospect of some trade-off among values.

These underlying issues are not taken up in this volume, and the reader will have to turn elsewhere for sustained discussion of the perspective that infuses individual chapters. This aside, the essays in this volume set a high standard of informed commentary and analysis, and are another reminder that U.S. scholarship on British politics is as lively and confident—and critical—as ever.

A critical perspective on recent developments infuses Joel Krieger's newly published book. In *Reagan, Thatcher, and the Politics of Decline*, he traces the fall of the Keynesian welfare consensus in Britain and the United States and passionately denounces the neo-conservative nostrums that have guided gov-

ernments in the 1980s. Krieger places substance above method, and this book presents a readable and refreshingly committed overview of the changing principles of public policy over the last two decades.

Krieger argues that the postwar consensus on the benefits of government intervention to harmonize interests, regulate the market economy, and finance universalistic welfare standards was doomed by economic recession and geopolitical decline. The economic crises of the 1970s severely weakened labor and fragmented the political constituencies of traditional European social democracy and U.S. New Deal liberalism. At the same time, the economic principles of the Keynesian postwar consensus were discredited, leaving its defenders exposed to radical critiques from the Left and, more ominously, from the new Right. Geopolitical decline exacerbated these challenges. Britain experimented with the status of a lowly second-rate power and the United States lost some of its huge comparative advantage. In both societies room for economic maneuver diminished as the scope for conservative appeals to national symbols increased.

The story is, of course, a familiar one. But Krieger is able to hold the reader's interest by subtly weaving detailed narrative into the larger picture. Descriptions of key events, such as President Carter's "moral equivalent of war" speech and Prime Minister Callaghan's defiant speech in defense of continued incomes policy at the miners' gala, serve as lightning rods for broad generalizations. Krieger artfully shifts back and forth between the particular and the general, combining a sense of perspective, some sharp wit, and a fine eye for detail.

Krieger's condemnation of neoconservatism is focused on the social policies that it entails. Reagan and Thatcher, he argues, have picked up the pieces of the shattered Keynesian welfare state, and have put them together in a radically different fashion, emphasizing de-integrative strategies in place of consensus, political exclusion in place of participation, executive autonomy in place of accountability, and a broad retreat from universalistic welfare measures except where they serve a particular group's self-interest. These moral criticisms are powerful, but they apply more to the intentions of these governments than to their achievements. Thatcher and Reagan may have

wished to break the back of the welfare state, but, as Krieger himself recognizes, they have been mightily constrained by public opinion. Both have attempted to reduce the role of unions in policy making, and here they have largely succeeded. But neither government is really beyond the realm of interest group politics and popular pressure, as Krieger makes them out to be, or perhaps as they themselves would like to be.

Krieger's argument sidesteps rather than confronts the central claim of the neoconservative program that "there is no alternative" (TINA). Neoconservatives did not argue that high rates of unemployment or reduced welfare were good in themselves, only that these were entailed in dealing with other, more fundamental problems. A persuasive critique of neoconservative policies would have to examine the logic of this choice and the availability of alternative courses of action. The moral censure of Thatcher's social policies would be much strengthened by examining the assumptions and the practice of the government's economic strategy, especially during the wasteland years of 1979 to 1982. By arguing that neoconservative policies illustrate the dilemma of combining capitalism and democracy, Krieger appears to accept by default the view that there really was no viable alternative.

The comparison between Britain and the United States is an interesting one and allows Krieger to take analytical advantage of some close parallels. In both countries, economic performance in the 1970s failed to meet even modest expectations, both countries have seen a downward slide in their relative political and economic standing in the world, and the victories of radical neoconservative leaders were barely 18 months apart. Krieger discusses the larger implications of each of these insightfully, without forcing unique features of Britain and the United States into a theoretical straight-jacket. But the thesis linking these phenomena together is overly narrow and elusive.

Krieger interprets geopolitical decline largely in terms of empire, arguing that imperial decline undermines consensus on social reform and welfare provision. This follows, conversely, from the general argument that the growth of empire—witness Bismarck and Disraeli—is strategically linked to the growth of the welfare state. The more closely one examines

this line of reasoning the more problematic it becomes. Far too much intervenes between empire building and social welfare policies for there to be a straightforward relationship between them. Once we test the hypothesis against the experience of other colonial powers in Western Europe (e.g., France and Belgium, both welfare laggards) and countries that had few exploitable colonies (e.g., the Scandinavian societies), it becomes clear that the role of empire is ambiguous at best. Other factors seem more important, including processes of diffusion, the strength of middle-class liberalism, and the timing and character of working-class political mobilization.

Whether or not the hypothesized relationship between empire building and welfarism has any validity for the nineteenth and early twentieth centuries, it is highly doubtful for the post-Second World War period. After all, in Western Europe the most decisive advances of the welfare state coincided with unparalleled loss of empire and with a generalized geopolitical deficit compared to the prewar era. In Britain, Attlee's great reforming Labour government created the basis of the modern welfare state as it presided over an unprecedented breakup of empire.

Krieger dates the decline of the postwar consensus in Britain and the United States with the economic shocks following the oil crisis of 1973–74. Actually, as Samuel Beer points out in *Britain against Itself*, cracks began to appear in the mid-1960s with rising inflationary pressures, intensification of industrial conflict against the background of divisive incomes policies, and shop floor and student radicalism. In the United States we can only speak of a Keynesian welfare state in the most minimal terms, and, to the extent that it existed, it was challenged in the 1960s on several political fronts, by Goldwater and others within the Republican party and by rebellious students. In both countries, the political rot in the post-war consensus set in before the economic crises of the 1970s.

The comparison between Britain and the United States is an instructive one, and perhaps it can be taken further. Britain and the United States share much more than experience of economic recession or political decline, and some of their further commonalities appear relevant to the strength of neo-laissez-faire on the political right. Historically, laissez-

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faire has been stronger in Britain and the United States, both as a doctrine and in practice, than in any other societies in the world. In the United States support for laissez-faire was linked to individualism and antistatism, values that have deep roots in U.S. culture. In Britain, the Conservative party was a pillar of the establishment and traditionally emphasized pragmatism, patriachialism, and *noblesse oblige*. But in the twentieth century the party was also influenced by a strain of classical liberal thought reflected, for example, in the doctrinaire opposition to state planning voiced by leading Conservatives including Enoch Powell, Peter Birch, and Peter Thorneycroft in the late 1950s. Thus, in explaining the rise of neo-laissez-faire, it seems sensible to refer not only to the decline of the Keynesian welfare consensus, but also to the predispositions of significant groupings within the Conservative and Republican parties. Some party-political factors may also have played a role. Unlike Christian Democratic parties in central and southern Europe, the Conservative and Republican parties are free of traditional Catholic antiliberal, anticapitalist influence, and unlike the Protestant parties of the Right in northern Europe, they do not need to compromise in coalition governments with Centrist parties that are unsympathetic to laissez-faire.

Although the analysis presented in *Reagan, Thatcher, and the Politics of Decline* is sometimes narrow and methodologically unsophisticated, the commentary on the fall of the Keynesian welfare consensus and the victory of the radical Right holds the reader's attention by continually searching for the larger meaning of specific political episodes. *Reagan, Thatcher, and the Politics of Decline* is a book that will further stimulate debate on the causes and consequences of neoconservative policies in Britain and the United States. Joel Krieger stakes out his own position boldly, elaborating a critique of new right governance that is both emotionally charged and intellectually stimulating.

In a period when the prime minister lends her name not only to the present government, but also to the whole range of its policies, the institution of the prime ministership looms particularly large in any evaluation of British politics. In the second edition of *The British Prime Minister*, Anthony King has brought together a colorful collection of essays written

by one former prime minister (Harold Wilson), several who have observed the prime ministership at close range (Tony Benn, Kenneth Berrill, Richard Crossman, and Bernard Donoughue), and three scholars in the field (George Jones, Richard Neustadt, and King himself). As noted in the introduction, surprisingly little has been written by political scientists on the topic. For those teaching courses on British politics, this book fills that gap nicely.

By focusing on the institution at the summit of British politics and the individuals who have shaped it, this book provides a useful counterpoint to structural analyses of political and economic change. The role of political leadership, personality and, one should add, good fortune, is nowhere so apparent as in the way Thatcher has promoted her own policy goals as prime minister. As Anthony King emphasizes in his chapter on Thatcher's prime ministerial style during her first term, she is unusual and probably unique among prime ministers of this century in coming to office with a policy agenda quite distinct from that of most Conservative members of Parliament or members of her cabinet. Given Thatcher's initial isolation within her own government, the policies that were pursued—for example, privatization, drastic economic retrenchment until 1982, and union-curbing legislation—were extraordinarily dependent on her own determination and acute powers of political calculation and timing. Also, one cannot forget the luck—*fortuna* in Machiavelli's sense—is perhaps a better term—of the timing and outcome of the Falklands War, which allowed Thatcher to turn the tide both within the cabinet and in the opinion polls.

Thatcher's tenure as prime minister leaves no doubt about the flexibility of the institution. Quite simply, she has shaped it to her needs as she perceives them, asserting her views from the outset in cabinet meetings, extending her oversight of ministerial decision making, and influencing the promotion of senior civil servants. It is tempting to construe Thatcher's attempt to dominate government as a *post hoc* demonstration of Richard Crossman's thesis regarding the evolution of cabinet government into prime ministerial government. Thatcher has extended the boundaries of prime ministerial power, but for all her ambition to be at the hub of government, she still has had to convince or cajole independently

powerful cabinet ministers, who are competitors as well as colleagues. Thatcher has led from the front, but she has also been overruled by the cabinet probably more frequently than any prime minister this century. George Jones's classic article on the prime minister's power remains the more convincing account. If his balanced appraisal of the power of the prime minister is lacking, it is mainly because—writing in 1965—he could not take into account the establishment of the prime minister's own policy unit and more extensive use of personal advisers, the growth of the cabinet office as a further source of independent power, and the increasing delegation of policy making away from the cabinet to its various committees. But recent political change has not been exclusively in the direction of increasing prime ministerial power. Minority government (as from 1976 to 1979) and the rise of back bench assertiveness both reduce the ability of a prime minister to enforce her will in the House of Commons as party leader.

Thatcher's activist prime ministerial style and abrasive personality provide a perfect foil for the more receptive style of Harold Macmillan or the more passive style of Douglas Home. By contrast with Thatcher, Harold Wilson appears even more distinctly as the "great conciliator." A prime minister, in Wilson's words, should be concerned with "face-saving and binding up wounds, intervening also to soften asperites and curb the invocation of personalities." Wilson's essay, along with some others in this book, may be read at two levels, for they reveal as much about the author and his own predilections as about the institution itself. Indeed, as King emphasizes, we must be cautious about analyzing the prime ministership in the abstract; within broad limits the office is what the prime minister makes of it.

Each of the books reviewed here calls for some reinterpretation of contemporary British politics. From their different perspectives these books compel us to reconsider the strength of democratic values, the effectiveness of political decision making, the flexibility of political institutions, the democratic integration of out-groups and minorities, and the concentration of political power in Britain. Britain suffers, we are told, from the decline of its civic culture, pluralistic stagnation, adversary party politics,

a loss of geopolitical standing, and excessively centralized, unresponsive, political institutions. Choices have become tougher. In recent years it has become immensely more difficult to blend tradition and modernity in British politics, to combine cabinet government and collectivist bargaining, parliamentary sovereignty and participatory democracy, nationalization and free enterprise, the unitary state and local rights.

But before all disappears in dense gloom, we might do well to maintain a sense of historical perspective. Widespread criticism of British political institutions is by no means new, and has in the past been instrumental to the process of political reform in Britain. The first two great reform acts of the nineteenth century were the product of political conflicts at least as intense as any we have seen over the last two decades. Compared to the 1950s and early 1960s, there has been a sharp increase in political discontent and criticism, and an unambiguous decline in political-economic performance. But the period following the end of the Second World War was itself unusual. The two-decade long conjunction of political consensus, industrial harmony sustained though moderate economic growth, and full employment was as unique in Britain as it was in most other Western European societies.

Many of the criticisms themselves represent changing standards as well as declining performance, and it would be surprising if this were not so. Beyond doubt, the emphasis on economic performance in recent years, and our assumption that government is responsible for it, have colored evaluations of political performance. At the same time, traditional standards have come under attack by those stressing greater participation, decentralization, and quality of life issues. From a longer historical perspective, it is hard to deny that the presumption of stable liberal democracy in Britain has itself helped to shift the grounds of evaluation.

Given the scope of political change in Britain, it is not outlandish to hypothesize that liberal democracy is a far harder kind of arrangement than writers in the 1950s and 1960s believed it to be. They lived through an era in which stable liberal democracy appeared to rest on a rather precise and unusual set of social, cultural, economic, and political conditions. Whether they looked at the explosive

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failures of democracy in Europe between the wars or the abortive experience of creating democracy by constitutional transplantation in former colonies, their concerns about the restrictive basis of liberal democracy were confirmed. Britain in the 1980s and 1990s may lead the way in revealing to us the adaptability, inherent toughness, and surprising autonomy of liberal democratic norms under economic decline, political dissent, and institutional inflexibility.

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Political Science: The Science of Politics.

Edited by Herbert F. Weisberg (New York: Agathon Press, 1986. xii, 307p. \$30.00, cloth; \$15.00, paper).

A book invariably begins with a title, and a title presumably conveys a message. Making a title redundant gives an author or editor a strategic advantage. It disarms the would-be reader even before he or she has had a chance to ponder the title's meaning. How can the reader resist redundancy? That, I suppose, is what editor Weisberg had in mind when he inserted the colon (:) between title and subtitle. Regrettably, he does not tell us just what the exact relationship of *political science* to the *science of politics* is, might be, or should be. At least I cannot believe that he equates the two. In that case, he would surely have used the equal sign (=) to denote the message. So much for my initial decoding of the title's meaning and the book's intent.

This, then, is a book about political *Science*. There is no room here for political philosophy, practical politics, descriptive case studies, grand theory, the craft of statesmanship, bringing the state back in, and all the sort of thing that fascinates a good many political scientists. Besides, the editor informs us, "the important scientific work being done in international relations, comparative politics, public policy, and the study of race, gender, and ethnicity issues cannot be considered here," but he assures us that "the in-depth analysis of the state of science in the areas included helps make up for this loss" (p. ix). Weisberg evidently has a sense of humor in all scientific seriousness.

The volume contains "theme papers" delivered at the 1983 annual meeting of the APSA that were intended to "extend the science of politics," and indeed several panels at the meeting "were specifically devoted to assessing the status of science in the study of politics" (p. ix). Weisberg as organizer of the meeting and editor of the papers acknowledges that "no attempt was made to define or limit the meaning of the theme," that "different schools and scholars have different interpretations of science in the study of politics," and that "diversity is important to maintain" (p. 3). There is more such genuflection to scientific tolerance and pluralism, but I suspect Weisberg had his tongue in cheek when writing this. If, even within the relatively narrow bounds of what a *science of politics*—in contrast to *political science*—is about, each author is free to interpret science differently, I wonder whether it makes any sense at all to make either *science* or *Science* the criterion for appraising a "discipline" that is as notoriously undisciplined as political science appears to be.

The trouble with making *science* a theme in assessing the study of politics is that, in field, laboratory, library, or even armchair, the scholar studying a *problem* does not begin his or her work by worrying about science, in whatever guise. Rather, he or she tries to solve a problem of interest in the most plausible way possible, by whatever means. I don't see why, *in the end*, we should worry about the scientific status of research once it has been completed and reported, there evidently not being a single road to salvation, protestations about science notwithstanding. As the anthropologist Clifford Geertz (1973, 5) once wrote, "If you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do."

I am happy to report, therefore, that several of the papers assembled by Weisberg meet Geertz's criterion and allow us to peek at what their authors as practitioners are up to. I would place among them Shepsle's "Institutional Equilibrium and Equilibrium Institutions," Dodd's "The Cycles of Legislative Change," Miller and Moe's "The Positive Theory of Hierarchies," and Franklin and Jackson's "Structural Estimation with Limited Variables." These four papers try to cope with

very different problems and do not worry about the scientific status of the work involved but give us some insight into how their author-practitioners go about it.

There is a second set of papers, more self-consciously reflective and in part prescriptive but clearly informed by research experience. Their authors are more concerned with what one calls the "state of the art" in a given field of inquiry than with the state of science in some Platonic heaven. I would include here MacRae's "The Science of Politics and Its Limits" (its title notwithstanding), Niemi's "The Dynamics of Public Opinion," and Weisberg's own "Model Choice in Political Science."

A somewhat different genre is represented by a set of papers that are more or less comprehensive assessments of entire subfields, and their authors seem to worry a good deal about the scientific status of particular contributions in these fields. They are also more or less critical in terms of some presumably self-consistent criteria of science, but as the criteria tend to differ from one subfield to the next it is difficult to compare their scientific status. Fortunately, these papers come off the word processors of talented research practitioners who, in fact, tell us more about their own state of mind than the state of their subfield. I list among them Rockman's "Presidential and Executive Studies," Gibson's "The Social Science of Judicial Politics," and Beck's "Choice, Context, and Consequences: Beaten and Unbeaten Paths toward a Science of Electoral Behavior."

Finally, there is a paper that stands by itself, Kramer's "Political Science as Science." It is an unadulterated statement of SCIENCE. I wish I had the space to unravel all of Kramer's views; but this would only get me into the philosophy of science that, I prefer to think, is the business of the philosophers and not of the scientists. What these views come down to is that Kramer is a believer in deductive modeling and a reductionist methodology reminiscent of the Vienna Circle's logical positivism or logical empiricism and, therefore, just the opposite of what Geertz recommends as the pragmatic criterion for understanding what a science is. I want to say only two things in addition. If, as Kramer suggests, Milton Friedman's *A Theory of the Consumption Function* "could serve as a useful paradigm for political science: a precise,

simple, but powerful explanatory hypothesis, rich in empirical implications" (p. 22), first, only Allah himself will save us from this particular scientific dispensation; and second, any number of sciences—from astronomy to chemistry or geology, including political science, might as well close up shop. But, then, I don't believe that even physicists and mathematicians go about their work in the way that Kramer recommends (just read the confessions of the mathematician Jacques Hadamard [1949]). There is a metaphor for one method as scientific as any other: trial and error. The trouble is not with the method but that scientists, including those who cultivate the gardens of politics, rarely tell us what they tried and how they erred.

In general, however, the contributions to the Weisberg symposium are considerably less purist than the theme they were commissioned to address at first leads one to assume. This is not to say that I share Weisberg's politically congenial disciplinary pluralism, if he means it. Weisberg, it seems to me, confuses what we should expect from theorizing with what we should expect from (forgive the word) methodizing. Insofar as political science aspires to scientific status, it thrives on the competition of theories but also suffers from the fact that, unlike the case of "more scientific" fields, one theory never drives another out of the market. It may do so for a time, but that is more due to fad or fashion than proof or disproof. That is the reason I have never accepted Thomas Kuhn's (1962) image of scientific development as pertinent to the social sciences. Yet this image is refracted in much recent discourse about the science of politics, in this volume and elsewhere. All the talk about new paradigms notwithstanding, political science in its U.S. version has never been anything but excruciatingly normal. It is our civic and secular religion, even in its neomarxist clothings. What has changed, however, is its methods. I respect Achen's (1983) critical stance in the matter, but I believe that in the last 30 years or so methodological change and advance have been the most remarkable aspect of development in political science. When Beck, for instance, unashamedly refers to the study of electoral behavior as "the most scientific" field in political science (p. 241), he clearly thinks more of changes in method than in theory. Theories change in the sense that they

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are extended, refined, explicated, and so on, but one theory is never jettisoned forever in favor of another. Old methods are in fact discarded, however, and replaced by new when and if the new prove to be superior to the old. I have a hunch that most of the authors have this in mind when they appraise the scientific status of various subfields. Whereas theories divide and should divide, methods are or should be the bond. When all is said and done, the validity of a theory and the reliability of theory-driven findings can only be ascertained if there is a modicum of agreement on methods.

By *method* I do not simply mean, of course, *technique*. I include the formulation of theories and hypotheses, the criteria of evidence and inference, and what Landau (1972) so felicitously called "due process of inquiry." Most of the papers in Weisberg's symposium deal with these matters in one way or another and more or less successfully. The problems of method, especially the choice of methods, is not a matter of taste, as I think the choice of theory often is, but a matter of persuasion. We expect the researcher to persuade us, and we want to be persuaded, that the ways in which he does business, so to speak, are appropriate. Let me illustrate this by juxtaposing the papers of Kramer and MacRae. The former sees "model building" as the primary task. As theorizing in the deductive mode, no matter how rigorous it appears to be, is rather intuitive and permits many alternatives, I would expect the sophisticated modeler to take a rather broad and tolerant view of the scientific enterprise. But Kramer's position is rigid. MacRae, on the other hand, proposes a most demanding criterion for scientific status—prediction. One might expect him, therefore, to take the narrow view that a science is as good as its predictions are good—and on this criterion the social sciences, including political science, are certainly flawed. Yet MacRae entertains a perspicacious view of the enterprise. He concedes the contributions that phenomenology or hermeneutics can make to our understanding of politics that Kramer would in all likelihood read out of a science of politics. I am more persuaded by MacRae than by Kramer.

It is similarly convenient to juxtapose the essays of Dodd and Shepsle. Dodd constructs a "cyclical theory" of institutional change in legislatures that is a highly stylized and empir-

ically suspect description of Congress in the early 1970s. (As Dodd himself points out, "the theory should fit this case, if no other, since it was created in response to these reforms" [p. 99].) Dodd's method is one of extrapolation of micromotivational tendencies to the macro-organizational level. I am not persuaded by this method of theory construction. By way of contrast, there is Shepsle's effort at formulating a "new institutionalism" that, in effect, restores politically-inspired sanity to economic hallucination. Shepsle retains what I think is best in rational choice theorizing but makes adaptations to the real world of legislative praxis that, it seems to me, are plausible and, above all, falsifiable. I am persuaded.

A third set of papers conveniently juxtaposed are Miller and Moe's and of Franklin and Jackson's. Although their topics are very different, both are commendably pedagogic. Both are concerned with capturing reality, one by empirically germane modeling, the other demonstrating a statistical "observation model" that presumably accounts for more than the usual regression models. Both make a common assumption: that what we observe is not the same thing as what may be out there in reality. It is quite usual for analysts to go off in theory or method and then, having reached dead end, to tell us, in what is called "ordinary language," where they have been or where they are now. This procedure, of course, simply confuses the real world with ordinary language. As the philosopher Carl Hempel (1965) instructed us some time ago, what we have when we speak of "facts" or "data" are at best "observational reports." Both theory and method ought to be sensitive to the contingency that reality and the reports we have of it are not the same thing. Both papers, each in its own way, make Hempel's argument. They are persuasive and of the kind on which to cut one's teeth.

I tread with caution in comparing two other papers, essentially surveys of their respective subfields by Rockman and Gibson. Both authors clearly took seriously the instruction to assess the scientific status of executive and judicial politics studies. Not surprisingly, each paper comes out with different conclusions, largely, I suspect, because the two fields face different empirical problems. It is the familiar "few cases, many variables" syndrome in the study of the U.S. presidency (the core of Rock-

man's essay), and the converse "many cases, few variables" syndrome in the study of judicial politics. But if the former makes Rockman somewhat cautious and rather gentle (for much of what he writes about is more soft thinking than hardnosed research), the latter makes Gibson rather reckless and critical in appraising often mindless empirical research. Nevertheless, I should note that both authors give due recognition to at least one piece of research, respectively, that seems to meet their criteria of good science in these subfields—in the field of presidential politics the work of John Kessel (1975), and in the field of judicial politics the work of Woodford Howard (1981).

There are what I would call epidemics of consciousness that from time to time sweep across political science. More often than not, they are as quickly forgotten as last Sunday's sermon. For some years now, the word *context* has been as fashionable in article titles as genuine contextual analysis has been missing (but for the most recent exception see Huckfeldt 1986). In general, the specification of context and its treatment as either an exogenous or endogenous *variable* remains vague. Unless contexts are in fact varied, it is doubtful that the contextual criterion has been satisfied. For this purpose, relevant data are often simply not available. So it is with "dynamic analysis," a notion that covers a multitude of possibilities. As one turns to Beck's long and lucid explication of what he calls the "Michigan Model" of electoral studies, the broad range of contextual and dynamic problems in the analysis of political behavior becomes apparent. The paper is worth reading on that account alone, even if one is not interested in voting behavior as such.

Beck makes many suggestive observations, but I must limit myself to a general comment. I am prepared to accept Beck's notion that the Michigan or Michigan data-based voting studies are prototypical of what good political science can be. Given this bias, I draw this lesson from the exemplar: the fact that a field becomes "more scientific" does not mean that there will be fewer controversies as to theory or finding (see Niemi and Weisberg 1976). On the contrary, as data making and analysis improve and follow different paths, the field's findings become more circumspect and tentative. This may appear to be a paradox, but it is not. If the task of science is to reduce ignorance

rather than assert some truth posited in a theory's premises, the evolution of the National Election Studies gives warrant to the field's status as "the most scientific" in political science.

Curiously, a false sense of ambivalence seems to pervade Beck's treatment. I could cite several examples, but one must suffice. One reads, for instance, that "the foundations of the Michigan Model are shaken by these results" (occasioned by nonrecursive modeling), but this comes "at the expense of the considerable parsimony and theoretical strength of the Michigan formulation" (p. 260). I think this false ambivalence has two sources. First, Beck flirts on and off with the Kuhnian notion of "scientific paradigms" but cannot make up his mind whether Kuhn's theory of scientific development is appropriate in the case of the voting studies (as I suggested earlier that it is not). And second, Beck hypostatizes the "funnel of causality" metaphor presented in *The American Voter* into a "model," which, I believe, it was not intended to be. It was a graphic device to bring some order into the vast array of factors presumably influencing the vote. As I noted in reviewing the book 25 years ago, "most of the empirical analyses proceed without the benefit of the 'funnel of causality,' and, perhaps more important, derive from theoretical propositions quite unrelated to it" (Eulau 1960). If there is a Michigan Model, then, it is considerably more sophisticated and complex than the funnel notion suggests. In fact, I find little in the revisionist literature, of which Beck makes a great deal, that cannot also be found in the original Michigan research or its later elaboration. This is not to denigrate the work of those who descended or dissented from what they wrongly conceived to be the Michigan Model. It is to suggest that it is wrong to confuse particular emphases of the Michigan research in a particular temporal context with a single model that, presumably, made a universally valid claim to some Truth.

In conclusion, a general observation on such an effort as that represented in this (admittedly interesting) volume: How wise is it for an APSA program committee to commission a set of papers that presume to be thematic (in this case, something called the "state of science" in political science) but that turn out to be either something more or something less. There is, it

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seems to me, something inherently artificial in such endeavor. It is a kind of imposed scholarship from the top that has surface plausibility but does not ring true. Our national (or regional) meetings ought to reflect what scholars are in fact doing rather than what particular convention organizers in any given year think they should be concerned with. This is the reason why, among the papers in this book, I find those reporting on their authors' ongoing work to be by far the most stimulating, regardless of whether I agree with their messages or whether they "extend the science of politics."

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The Moral Life of Children. By Robert Coles (Boston: Atlantic Monthly Press, 1986. 302p. \$19.95).

The Political Life of Children. By Robert Coles (Boston: Atlantic Monthly Press, 1986. 341p. \$19.95).

Political socialization, neglected by political science, may yet be salvaged by the impressive efforts from other disciplines. I have previously noted in these pages that cognitive psychologists examining social and moral development have contributed excellent insights questioning many standard assumptions on the evolution of political awareness. Now, from a divergent theoretical perspective, Robert Coles has appeared with two complementary overviews not only of the fieldwork reported in his magisterial five-volume set, *Children of Crisis*, but also of the research subsequently pursued around the world.

At first glance, Coles's books might seem mystifying to political scientists. They are discursive, at times even novelistic; hypotheses are scarce; and the children's own words are accorded seemingly as much space as Coles's analysis, itself almost unsettlingly candid about his occasional unwillingness (or inability) to reach definitive conclusions. Yet it is precisely Coles's unusual approach and unfamiliar method that make these books so essential. Coles is not uninformed of the literature in political socialization; far from it. But by adopting an inductive approach that allows the children's comments themselves to set the books' agendas, Coles has alerted us to many possibilities that political science has scarcely considered at all.

For instance, one of the most central assertions to be derived from these books is that children possess a capacity, even before adolescence, to be thoroughly competent in

making moral and political judgments. This conclusion partially comes across through Coles's tone; instead of referring to his subjects as "naive informants" (to use the label of U.S. political socialization), Coles prefers to initiate a dialogue in more egalitarian terms, as in the case of one 12-year-old Cambodian girl whom he calls "an extraordinary teacher of mine" (*Political Life*, 278). But Coles also shows considerable evidence that children do not merely parrot ideas from parents and teachers but instead creatively work these elements into what, without exaggeration, he calls their "political and moral consciousness" (*Political Life*, 303). The position that children can be and are intuitive moralists has been impressively charted by recent examinations of moral development, and thus *The Moral Life of Children* may be the less immediately valuable of the two books. Coles's claims for children's political understanding, on the other hand, have been accorded less currency in political science and deserve greater scrutiny.

To Coles, nationality is a constant in all lives, including those of children. By his psychoanalytic reading, nationality provides not only an outlet for the id or a source of the proscriptions of the superego, but, most intriguingly, a key instrument in the construction of the ego. Coles suggests, presenting pages of evidence from children in numerous locales and regimes, that the institutions, traditions, and symbols of a nation provide clues for children seeking general ways to concoct an identity out of the particulars of their lived experiences. It should be noted that such personal uses of nationalism are not synonymous with what was once dubbed "system support." Nationalism can, of course, be exploited to support the state, but in Coles's examples, it often undermines it, either by allegiance to another political entity (as among Catholic children in Ulster) or its use for cultural and religious institutions against political ones (e.g., in Poland). Nor is nationality an easily avoided fact for any inhabitant of a nation, as Coles persuasively elucidates in his superb chapter on South Africa, relying, as always, not only on interviews but also on exemplary interpretations of children's drawings. To be sure, racial differences are acute: white children, both Afrikaner and English, have fully appropriated racist nationalism; colored children, caught between classifications, are

shown to be "the most color-conscious children I've ever met" (*Political Life*, 205) by their extraordinary attention to the skin color in their drawings; black children, loath to portray themselves in their drawings except in a collectivity, express rage at the regimentation of apartheid. Yet, Coles nevertheless concludes, "these boys and girls all think of themselves as South Africans, even dream of themselves as South Africans, no matter what a government says and does and announces it will do" (*Political Life*, 223).

It is difficult to know how generalizable this argument of the near universality of nationalism can be. Clinical psychiatrists are, by training, most interested in the abnormal; Coles has only turned the focus to how normal individuals cope in the midst of unusual and stressful environments and times. While such choices may be apt for noting the resilience of children facing moral and political crises, they may overestimate the extent to which nationalism is a central component of children's identities, political or otherwise. For most of the children studied here (e.g., those from Ulster, Nicaragua, Quebec, Poland and South Africa, and the refugee children of Southeast Asia), nationalism has become a central concern at least in part because of extraordinary political preoccupation with the question of nationality in each society. Whether or not nationality would prove to be a crucial component of identity in less dramatic circumstances is only briefly touched upon in the chapter on Brazil and the United States, where Coles sees nationalism as severely refracted by class. But the discussion is too cursory to allow us adequately to assess Coles's point on the last page, "A nation's politics becomes a child's everyday psychology" (*Political Life*, 310).

Still, the contribution of these two books may not rest on how definitive Coles's conclusions are. His service may be greatest in spotlighting aspects of the child's relationship to politics and morality that have thus far remained insufficiently noted. Instead of pointing to "agents of political socialization," he implicitly urges the reader to emphasize children's personal experience in the immediate surroundings that they often share with those "agents," most particularly the influences of religion and class. Instead of automatically assuming that children are in need of civic and moral training, he favors methods to assess less

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what children fail to understand than what they grasp on their own terms. And instead of looking for the uses that politics has for imbuing system-supportive behavior in children, he pinpoints the potential uses children have for politics in helping to create their identities. All three of these implications are significant departures from our standard assumptions in political science and alone would make these books worth reading. But with Coles's sophisticated command of psychoanalytic theory and the humaneness and beauty of his prose, they attain the status of must reading. Coles, to be sure, may not be able to provide us with solid answers on the political and moral worlds of children; but he has raised questions that should concern us for years to come.

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Weberian Sociological Theory. By Randall Collins (Cambridge: Cambridge University Press, 1986. xi, 356p. \$42.50, cloth; \$14.95, paper).

This readable and well-crafted volume is not so much an exegesis of Weber's thought as an effort to demonstrate its coherence and contemporary relevance. Collins attempts to do both Weber and modern social scientists a favor by correcting or ignoring misconceived or inconsistent aspects of Weber's writings in light of their implicit theoretical unity and their potential for explaining important social phenomena unanticipated by Weber himself. Recognizing that Weber called into question on methodological grounds the possibility of a unified, predictive sociological theory, Collins nonetheless purports to find such a theory at least suggested in the corpus of Weber's work.

This book has substantial virtues that make it profitable reading irrespective of one's methodological commitments. Foremost among these virtues is the emphasis on the importance of political factors in *all* of the diverse social processes treated in Weber's work. For instance, by Collins's analysis, the most important dimension of Weber's mature theory as to the necessary conditions for the rise of capitalism is cultural and political competition. He also argues that Weber differed most from Marx in the conclusion that capital-

ism could exist indefinitely, given an appropriate political context, and makes a persuasive case that Schumpeter's highly political analysis of the actual workings of market institutions complements Weber's historical analysis of their development much better than the classical views that still influence modern economic theory.

Similarly, Collins attempts to show that religious heresies are occasioned by power struggles within universal religions characterized by a certain organizational structure. He then suggests that the same factors can be seen in secular forms of explicit ideological conflict. Even family structure and the cultural and social status of women were presumed by Weber to be largely the result of military technology and political possibilities. In explicating these arguments, Collins does not hesitate to modify or amplify Weber. Yet he is very familiar with the work of his protagonist, and in most instances it seems to me that Weber lends himself to these modifications, that these arguments can be plausibly claimed as Weberian. This is especially so in that Collins, despite his explicit rejection of Weber's "idealist historicism," for the most part observes Weber's strictures against unicausal reductionism. While politics may be a key factor, it is never a sufficient cause.

There is, however, a very paradoxical exception to this general practice, and that is the treatment of politics itself. Collins argues that internal politics is largely determined by external geopolitics because the ability of a faction to dominate a state largely depends upon its prospects for achieving international victories. Contrary to the importance most commentators on Weber's thought attribute to the concept "legitimacy," it is here reduced to a secondary factor. It is derived from the power-prestige of the state. Indeed, even "nationalism" is no more than a "feeling of awe toward the state," which is the "result of the success of a state in power politics" (p. 154). Although much of the analysis that Collins derives from this starting point is suggestive, it tends to be strained and overly one-sided in its emphasis on the primacy of geopolitics. Furthermore, he violates his own ground rules by insisting that this is the genuine Weber, hitherto misunderstood.

Actually, it is similar to a number of previous interpretations, most notably that of J.

P. Mayer. More importantly, it ignores much of Weber's analysis and many explicit statements. For instance, in *Economy and Society*, Weber asserts that cultural factors may fuse with such political prestige, and when this happens "the naked prestige of 'power' is unavoidably transformed into other special forms of prestige and especially into the idea of the 'nation'" (pt. 2, chap. 9, sec. 5).

Nevertheless, this section of the book is as interesting and suggestive as the others, especially if one views its perspective as one of those one-sided accentuations called ideal types. Collins, however, would not wish to have his arguments so characterized. In any case, he has successfully demonstrated the continuing relevance of Weber's work by offering a number of interesting applications and extensions. But whether this success is attributed to the validity of an implicit general causal theory or to the normative relevance that Weber's concerns still possess for our culture is a question not really addressed in this book.

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The Nature and Limits of Authority. By Richard T. De George (Lawrence: University Press of Kansas, 1985. viii, 305p. \$27.95, cloth; \$12.95, paper).

This wide-ranging and insightful book concerns the concept, justification, and limits of authority in various contexts. It is the best to date on the topic. Although it contains lengthy discussions of political authority, it considers nonpolitical authority throughout society. Indeed, a strong point is the showing that consistent anarchists must accept various types of nonpolitical authority. The central claim is that acceptance of authority is the norm, that within limits it is rationally defensible, and that challenges to authority require defense (p. 1).

De George begins with a general working model, which is refined for specific types. An authority stands in a relation of superior to inferior to someone in some realm (p. 14). The fundamental distinction is between *executive* and *nonexecutive* authority. Executive authority is the right or power to do something (p. 17); all other authority is nonexecutive. Un-

fortunately, the concepts of *right* and *power* are not adequately analyzed, and some arguments seem to use different senses of them. Executive and nonexecutive authority are subdivided by the kinds of response appropriate. Executive authority is divided into *imperative* (evoking obedience) and *performatory* (doing something directly to or for one); nonexecutive authority is divided into *epistemic* (evoking belief) and *exemplary* (evoking limitation). A plethora of other distinctions are made—including *de facto*, *valid*, *legitimate*, *de jure*, and *effective*. These distinctions are then used to analyze authority in the family, voluntary organizations, business, politics, morality, religion, and universities.

The argument for the central thesis is not as explicit or persuasive as desirable; the most explicit defense is given for political authority. Legitimate authority is originally defined as that for which there are good reasons (pp. 19, 142), and the primary justification for any authority is that it benefits those subject to it (pp. 39, 45, 72, 85, 135, 152, 182). This seems to place the burden of persuasion on one who defends authority and thus subverts the main thesis. However, a government is legitimate if most of the people subject to it accept it and think it justifiable (p. 143). This general acceptance of, or consent to, authority switches the presumption to legitimacy so that criticism of authority requires defense (pp. 160-61).

We are left with a tension. Justifiable authority must actually benefit those subject to it (e.g., p. 163); acceptance alone cannot justify authority—only change the presumption. One way to support shifting the presumption of legitimacy is to claim that those subject to authority are the best judges of whether they benefit. This has the advantage of being limited to authority and other matters where benefit to the individuals is at issue, but the disadvantage of resting on an empirically questionable premise. A second way is to claim that if one wants to dissuade persons from believing or accepting something, one must accept the burden of proof. This has the advantage of applying to all practical arguments, but the disadvantage that it has no more to do with authority than anything else.

MICHAEL D. BAYLES

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Law's Empire. By Ronald Dworkin (Cambridge: Harvard University Press, 1986. 470p. \$20.00).

What is law? Ronald Dworkin in *Law's Empire* takes up the eternal question of jurisprudence, but to his credit he does not try to nail down an answer. Instead he investigates the status of the question by offering an account both of how judges decide cases and of how they ought to do it. His claim is that judges must interpret first the practices of the legal system of which they are a part and then the moral principles of their society that must inform the law. Only thus can they give the law both "grounding" and "force" in order to legitimate their decisions and the use of coercion by the government. The facts of cases are deliberately ignored, which is unfortunate, since half the terrain of interpretation is thus left out.

Dworkin's version of interpretation is closely tied to a communitarian political theory. Philosophers and judges are to articulate the immanent practices and shared understandings of their communities and not to impose abstract rational and universal standards upon them. Political theorists have long been familiar with Michael Oakeshott's subtle representation of this view. Dworkin regrettably has chosen literary hermeneutics as his model, with all its hostility to causal explanation and reliance on empathy and intuition for understanding social phenomena. Dworkin never asks just how law resembles literary texts for purposes of "reading"; its authoritative character is only mentioned in a footnote and in another context. Nor is it clear why he insists that social scientists can only understand the members, not the practices, of a culture other than their own. To say that they are engaged in an investigation, not a conversation, hardly serves as an argument. There is, in fact, reason to think that we do understand foreign societies and the past of our own. In any case these assumptions justify Dworkin in limiting himself to the practices of his own "flourishing" legal systems of Great Britain and the United States, and in relying on only four, highly unusual, appeal court cases to illustrate his points.

It is easier to know what Dworkin's "law as interpretation" is not than what it is. It is not the "semantic sting" of analytical jurispru-

dence, used to clarify the existing structure of norms by defining them all exactly. John Austin's and Herbert Hart's jurisprudence cannot account for the real divergences of opinion among judges, for it is based on a simplistic psychology and is devoted to one political end, the security of established expectations, which it fails to examine critically. The undeniable appeal of this flawed theory is due to what Dworkin calls the "conventionalism" of most lawyers. They believe that law is just there, that it is a gapless hierarchy of norms that judges have to find and when it really offers no rule or convention then the judges legislate, but do not tell the public. But since this is not what judges actually do, it is a matter of self-deception or false consciousness. Though they do not know it, judges do interpret the past political decisions that make up the precedents, statutes and political mores of the system, by teasing out their meaning here and now. Interpretation also does not look for an idea of justice. Justice is what anyone in a pluralistic society thinks is best for the community as a whole. And fairness is just impartiality. The two may therefore be at odds, though both are social virtues. So much for John Rawls. Pragmatism, the notion that judges should look only to the future welfare of society, is treated with more respect, though it offers no airtight protection to rights and makes unrealistic moral demands on citizens. However, as policy must enter the decisions of judges, it has its place.

Law, then, is not a system of rules, a pursuit of justice, or a plan for social happiness. It is the interpretation of the social practices inherent in past political decisions and in the principles of the community as they appear to a man and judge of "integrity." Lastly, interpretation is not explanation, or the "external" Marxism and the intellectual muddles of Justice Holmes. In fact the "external" view does not ignore the inner structure or development of law, but tries to integrate it into the political history of the society of which it is a part. The social and political history of law, whether narrative or analytical, is a perfectly plausible enterprise, as is the history of medicine or religion. One may even come to grasp the "natives' point of view." Indeed, Dworkin's brand of hermeneutics is not as acontextual as all that; it reaches out to the moral and political practices of the community at large

for its self-understanding. On the whole he also avoids turf fights, and he should have stayed away from this one.

To see what a man of integrity, Hercules the Judge, would do, Dworkin begins with a fiction. Society is to be treated as a personification, like a corporation, and so can be held accountable for its conduct and understood as a single unit. Hercules, in interpreting its legal practices, conceives of respect for statutes, precedents, and consistency as an intellectual virtue. He does not, however, ask about the intention of the authors of a law or even of the Constitution, because it is unknowable. That is also current literary theory. Instead he imputes intentions to them that are likely to put society's practices "in the best light" now, that seem most "attractive" and are in keeping with his "genuine" moral convictions as well as those of other members of the community. There is no way of telling how much consensus would be required for such a judicial performance. Its chief doctrines are limited government, the equal protection of the rights of all members of society, and an equal concern for the fate of all. These are the best part of the political principles of both the legal system and of the polity as a moral unit. What it excludes is slavery and racial discrimination, but, for the rest, its meaning is pretty vague. Most of these moves are at a preinterpretive stage of "law as integrity." The fully interpretive task is one that brings morals embedded in the society to bear on decisions that will have "fraternity" as their aim and principle.

Although it is often repetitive this book is clearly written and will make for lively graduate seminars. Whether one finds it either coherent or persuasive is, as Dworkin would say, a matter of what one happens to think right now.

JUDITH N. SHKLAR

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Critical Theory and Public Life. Edited by John Forester (Cambridge: MIT Press, 1985. vii, 337p. \$30.00).

The intention of this highly varied collection of essays is to demonstrate the practical relevance of critical theory in dealing with problems of politics and policy analysis drawn from

everyday life. With the exception of Habermas all of the authors are affiliated with U.S. or Canadian universities, yet the collection avoids North American ethnocentrism by including two pieces that draw from the revolutionary reflections of Third World authors, Frantz Fanon and Paulo Freire.

The overwhelming emphasis is nevertheless on the U.S. political experience or, more generally, on problems derived from advanced industrial societies. There are no contributions by women, and with the exception of Ben Agger's essay, none of the contributors directly addressed questions emerging from the women's movement.

What was distinctive and singular about the critical theory of Adorno and Horkheimer and Marcuse was its repudiation of modernity and the promise of the enlightenment insofar as the scientific knowledge that made it possible to rationalize human existence also augured the "end of the individual." Objectified man in the totally administered society was the logical endpoint of the project of modernity.

Coupled with this pessimism, Adorno and Horkheimer (and, to some extent, Marcuse) mourned the passage of high culture and the promise of autonomy with the emergence of mass society and mass culture. It is these aspects of the older critical theory that allow us to view our narcissistic age and our illiberal liberation with a critical eye. Critical theory compels us to become conscious of the emptiness and the superficiality of life in the technocratic society where the falsely liberated, therapeutic personality has replaced the autonomous ideal.

Jürgen Habermas represents the second generation of critical theory, and his project has been to retrieve the essential optimism of the enlightenment and of critical Marxism while fully cognizant of the structural transformations in contemporary society that have made the rational and critical promise of these traditions problematic. In fact, one might say, that Habermas's work has been dedicated to the systematic reexamination of critical theory in order to reunite its powerful theoretical critique with a conception of action appropriate to advanced industrial societies.

The essays in this volume stand as a tribute to the theoretical richness of Habermas's writings. But, as one of the authors put it, discourse about critical theory tends to be very

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abstract and fails to make contact with those mainstream social scientists "in the trenches" who are still locked into a positivist epistemology for the lack of a better alternative (p. 253). What these essays intend is to demonstrate to colleagues in political science and sociology, in urban planning and in the policy sciences, that critical theory is a viable alternative to positivism.

For the most part the pieces succeed in moving the discussion away from the theoretical abstractions and towards practical questions. Frank Fischer demonstrates how a critical perspective on the Head Start program can challenge the claims of an empirical evaluation in the Westinghouse Learning Corporation's report. By exploring phenomenological questions about the subject's self-understanding of Head Start and critical questions about the ideal of society presupposed by the program, the narrow and distortive empirical measures of the Westinghouse study are placed in perspective. Similarly, John Forester demonstrates the value of critical theory to planners, whose roles are reinterpreted here from the perspective of Habermas's communicative competence. No longer viewed as simply technicians with expertise, planners emerge as agents in a discursive process aimed at democratizing the system that binds together the planner and his public.

Somewhat less innovative are the essays by Ray Kemp and Peter Grahame. Kemp argues that the Windscale Inquiry of 1977, which was to explore "a proposal by British Nuclear Fuels Ltd. (BNFL) to construct a thermal oxide reprocessing plant" (p. 177), turned out to be an inquiry serving the interests of "state and capital" and therefore an exemplification of the systematic distortion of the communication process. I doubt whether such a finding requires the analytical power of critical theory. Equally questionable is the essay by Peter Grahame, which seeks to use *Consumer Reports* as an ideal type of literature creating a focal point for critical opposition to the manipulative advertising of the major corporate producers. Things get carried a bit far when Habermas is invoked to conclude that *Consumer Reports* "occupies a unique position as a critical interlocutor of mass market definitions of satisfaction" (p. 149).

David C. Hallin writes informatively about the news media in the United States and claims

that a technical and professional definition of their reportorial role prevents these media from providing the kind of active public sphere central to critical theory's project of democratic revitalization.

Along very different lines, Dieter Misgeld presents a powerfully convincing interpretation of Habermas's life-world colonization by using Paulo Freire's *Pedagogy of the Oppressed*. Essentially, Misgeld argues that linguistic colonization occurs when the concepts and definitions of the oppressors are employed by the oppressed to define themselves. The oppressed referred to here are not only colonized Third World peoples, but inhabitants of advanced industrial societies who have allowed their life-worlds to be transformed by the scientific knowledge possessed by the experts in the helping professions, the new agents of social control. John O'Neil similarly finds much complementarity between Habermas's emancipatory project and Frantz Fanon's and Paolo Freire's decolonization.

The two essays that launch the book, one by Ben Agger and the other by Timothy Luke and Stephen K. White, are disappointing. Luke and White straddle the fence on the question of the prospects for a more democratic society. On the one hand, they see the life-world colonization as enhanced by the informational revolution; on the other, they find "ambivalent potential" in the broadly defined "ecological path to modernity." Agger, while challenging "high-tech capitalism" as a panacea, points to the impending crises in the next stage of capitalist development. Despite its tacky metaphors of romance, affairs, and bedfellows, John Forester's second essay in this collection serves a useful purpose in juxtaposing the central tenets of policy analysis to the basic ideas of critical theory.

Alone among the contributors, Trent Schroyer boldly raises provocative questions about the adequacy of critical theory to understand the U.S. experience. Reminiscent of Tocqueville, and, more recently, of Robert Bellah, Schroyer argues that our civic and religious traditions, our "cultural surplus" give credence to claims of U.S. exceptionalism. It is the revolutionary and critical components of those traditions that Schroyer turns to as potential resources for the revitalization of the "universal principles for political participation" and a communicative ethics to ground a

new consensus (p. 311).

There are ironies in a collection of essays on critical theory designed for a U.S. audience. The ideas suffer more than a sea change as one reads that *Consumer Reports* represents a critical negation of mass advertising, with no recognition that the consumer project is more rationally enhanced by the presence of such reports. There is irony also in the critical analysis of the Head Start program. While it reveals the broader horizons possible for the culturally deprived, little is made of the legitimization function served by Head Start, and there is no "critique" of the social reproduction of capitalism that takes place in the midst of this and similar programs. Perhaps Adorno was ultimately right when he argued that the only posture for critical theory is negation without resolution. For, in the post industrial age, it seems that a positive or practical application of critical theory may lead unintentionally to a new form of legitimization. The essays in this volume inadvertently pose that dilemma and deepen our interest in critical theory.

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Between Philosophy and Politics: The Alienation of Political Theory. By John G. Gunnell (Amherst: University of Massachusetts Press, 1986. 240p. \$25.00, cloth; \$12.95, paper).

This trenchant work provides a "synthesis, clarification, and elaboration" of arguments made elsewhere by Gunnell alleging the "alienation" of academic political theory (p. ix). Not the first to suggest that dominant practices in this field are forms of intellectual masturbation, Gunnell nonetheless presses this central point with considerable zeal and with a delightfully acid tongue. He is thereby sure to provoke contention if not the "transformation" he desires.

Transforming the field requires an understanding of its present-day ills. Hence, the first chapter offers a reconstruction of the intellectual development (or rather devolution) of the field over the last three decades in order to identify a variety of historical forces that in turn help account for the underlying cause of

the alienation of political theory today: its entrapment in and by philosophical and meta-theoretical discourse, or "its absorption with metatheory" (p. 154), or (more creatively) its "philosophization" (pp. 28, 31).

This dreaded disease engenders in political theory a distorted and pretentious self-image. Symptoms include the following: the subject is ensnared in myths (identified on pp. 1 and 194); is unreflective about its parasitic relationship to "the transcendental and epistemological traditions in philosophy which . . . are themselves alienated enterprises" (p. 4); and is unable to achieve an authentic appreciation of its actual relationships to politics and political inquiry. Though fruitful relationships are virtually nonexistent, most theorists believe, or pretend to believe, that their estranged and rarified discourse provides authoritative standards, foundations, and illumination for both political practice and political inquiry, and on which, therefore, both are or ought to be dependent. They even believe, or make believe, that much of this discourse, being in some (distant) way about politics, is itself political.

These illusions and delusions serve a variety of purposes, but foremost among them is, of course, the bolstering of self-image and of the conviction that all this talk—about epistemology, methodology, explanation, and interpretation; about metaethics, normative foundations, and facts and values; about the universal dilemmas, fundamental concepts, and philosophical dimensions of politics; about historiography, hermeneutics, and the great tradition of epic theorists—actually constitutes an authoritative script for inquiry and an authoritative and authentic "theoretical and practical engagement of politics" (p. 138).

How might the patient be cured? Academic theory "redeemed"? Discourse "transformed"? Two prescriptions seem crucial. First, theorists must come to recognize and freely admit that political inquiry and politics are autonomous practices, quite capable of getting along without transcendental foundations and legitimations. Second, they must rethink "what theory is and can be in political theory." As we shall see, taking this cure can help theorists "come to grips with the question of the relationship of academic political theory to politics," though it cannot resolve this difficult problem (p. 155).

With respect to autonomy, Gunnell's posi-

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tion is the contextualist one that criteria defining practices and standards of good practice are internal to practice; consequently, second-order reflection on practice, no matter how well performed, cannot be automatically authoritative (though it *may* be influential). This position means that science does not need metascience and that politics does not need metapolitics. Like science, politics has its own standards and theories, which may or may not be influenced by academics. But that most of what passes for political philosophy and political theory today could possibly affect practice, Gunnell very much doubts. To suppose it could would be to suppose that it took political practice as its object and that it had an audience with which to interact. In fact, however, it takes philosophical objects, projects, and perplexities and *ascribes* them to politics. Thus, Rawls's work and the cottage industry it has inspired "is not about any human practice; it is not about any state of affairs. It is about concepts and logic." Similarly, MacIntyre assumes in *After Virtue* that moral relativism is a political rather than an academic problem, and that the latter is both the cause of the former "and the site of a solution" (pp. 176, 179).

What all this suggests is that political theorists should engage politics in the manner, for example, of that portion of the feminist literature that confronts an "existential problem . . . and that speaks to and for an actual audience" (p. 122). Such a prescription means only that *metatheory*, not *substantive* theory, be given up. Advancing *substantive*, *constitutive*, or *ontological* theory should be a primary task of academic political theory.

By *substantive* theory Gunnell means "that class of claims that establishes a domain of facility—what exists and the manner in which it exists—and provides the criteria of explanation, description, evaluation, and prescription" (p. 143). An example of such a theory, one on which Gunnell has been working for some years, is provided in the final chapter. It draws on theoretical (not metatheoretical) claims advanced in the philosophies of action and language, and thus postulates ontological claims about social phenomena that are familiar features of the literature on interpretive forms of social inquiry. It just happens to assert the conventionality of politics, thereby undercutting the contrary ontological

positions of transcendentalists while underwriting an interpretive form of inquiry that would require taking conventional politics as its text.

Gunnell tries in this way to unite theory, inquiry, and practice. Because his (corrigible) theory entails the conventionality of politics, inquiry must proceed along interpretative lines. This in turn ensures that conventional politics will be the object of analysis, thus opening the possibility of practical engagement. By focusing on conventional political understandings and standards, the theorist just might find an audience interested in what he or she has to say. Of course efficacy is not guaranteed, but the alternative is hopeless. To continue the quest for transcendental political forms and standards is a sure loser even if such entities could be discovered. For these would resolve only transcendental crises and dilemmas and leave politics quite untouched.

It is this essentially political message that makes Gunnell's position a compelling and timely one. Unfortunately, debate will focus on the metatheoretical, methodological, and theoretical claims he advances; the irony of this situation will be examined in great detail; protests aimed at his exaggerations and stinging rhetoric will be heard from many quarters; and the thundering political silence will continue.

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Aspects of Toleration: Philosophical Studies.

Edited by John Horton and Susan Mendus
(London: Methuen, 1985. ii, 180p. \$29.95).

John Stuart Mill's *On Liberty* has set the context for modern discussion of the principle of toleration. *Aspects of Toleration: Philosophical Studies*, edited by John Horton and Susan Mendus, is a collection of essays that attempt to put Mill in the context of the 300 years that culminated in his seminal essay, to examine and to clarify his philosophical assumptions, and to link his work to contemporary issues.

The essays in the collection are reworked papers presented at seminars and conferences supported by the Morell Trust based at the University of York, England. All but one of the

authors is, or has been, a member of the political science or philosophy faculty at that institution.

Sexual, religious, and racial toleration are the basic themes of the essays. Racial toleration is discussed by Alex Callinicos. Religious toleration is the theme of David Edward's essay. Sexual toleration, in particular the toleration of pornography, is the subject of Susan Mendus's paper. These issues are dealt with collectively by Thomas Baldwin who uses them to examine the relationship between freedom and toleration. Peter Jones and John Horton, in separate papers, discuss the relationship between harm and toleration. Albert Weale and Peter Nicholson analyze the justification for, and the meaning of, toleration in modernity.

The essays are more interesting and more provocative of good conversation than the subjects might suggest. Horton makes us rethink Mill's belief that toleration is only limited by a test of whether the action being tolerated harms others. We do not always agree as to what is harmful: harm is not a neutral criterion. Jones argues that harm must be read to cover the moral effects of action: it may be licit to require seat belts because the consequences of not wearing one impose moral duties on others to aid the injured. Mendus raises pressing questions as to whether pornography might be corrupt in itself and thus beyond need to be judged by the harm principle.

Edwards and Weale base their case for toleration on the principle of respect for persons. Edwards argues that the most compelling case for protecting religions conviction against abuse, but not against criticism, should be based on this principle. Weale concludes that this principle provides the soundest foundation for toleration. Both sever Mill's argument from any lingering utilitarianism.

The best internal debate is between Baldwin and Callinicos. Baldwin argues that the duty to tolerate rests on others' right to freedom; toleration is obligatory in a free society. Callinicos, while rejecting Marcuse's concept of "repressive tolerance" argues that the requirements of freedom set limits on the practice of tolerance. Toleration should not be extended to fascist groups promoting violence.

Taken as a whole these essays might suggest the philosophical collapse of liberalism. Mill's

assumptions, as analyzed, seem to provide an uncertain guide to modern society. Mill's philosophy seems to be judged shallow. Modern liberalism seems not to know how to defend itself against its enemies. Rampant pluralism tugs at old values: Can toleration extend to *Hustler* magazine, to virulent anti-Semitism, to the Moonies? The authors suggest that liberalism may be arrogant when rejecting questions of abortion, of the duty to protect crime victims, of the feminist critique of pornography.

But the authors do not draw this conclusion. They insist that the principles of liberalism, while needing to be rethought, are still to be trusted. The garden of toleration needs tending, not plowing under. Though these essays do not suggest a complete strategy for how toleration may be adjusted to pluralism, and do not suggest any final response to the threat of the Right or the Left, they do serve to continue a useful dialogue.

The philosophical analysis offered by this book should commend itself to U.S. audience. The United States is also an heir of Anglo-Saxon liberalism. The case studies, however, are almost excessively British. The U.S. reader is left to puzzle out the details of the *Gay News* case of 1977 that reaffirmed blasphemy as a crime. We are assumed to know all we need know about the National Front, a British right wing organization. Questions arising from AIDS, so crucial to a discussion of toleration on this side of the Atlantic, are not discussed. *Aspects of Toleration* should be read by U.S. citizens interested in the issues it raises; but it would be more useful to read it as a companion to Jacques Thiroux's third edition of *Ethics: Theory and Practice* (Macmillan, 1986). That work is also rich in analysis and the examples will be familiar to the U.S. reader.

BRADLEY KENT CARTER

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Political Cognition. Edited by Richard R. Lau and David O. Sears (Hillsdale, NJ: Lawrence Erlbaum Associates, 1986. x, 408p. \$39.95).

For many years, political scientists have drawn from the work of social psychologists in

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efforts to understand the attitudes and behaviors of ordinary citizens. *Political Cognition* represents the latest merging of these two disciplines, and its focus reflects the cognitive revolution currently under way in social psychology.

The book is composed of papers presented at a mid-1984 conference among experts in political behavior and social cognition. The first of three major sections introduces readers to the social cognition perspective and illustrates ways in which this body of work might inform the study of politics. Composing the second section are eight papers describing empirical explorations of political cognition. These papers form the core of the volume. Essays written by political scientists synthesizing the empirical chapters and speculating about future prospects for studying politics from the social cognition perspective constitute the final section.

As the first section's chapters point out, social psychologists studying social cognition do not have grand integrated theories to offer political scientists. Rather, their work highlights concepts (e.g., schemas), empirical regularities (e.g., systematic errors in information processing), research methods (e.g., sentence completion tasks), and independent and dependent variables (e.g., reaction time) that suggest hypotheses and ways to test them. The empirical chapters here draw primarily on concepts developed in social cognition research while using the traditional methods and variables of political science.

Work in social cognition focuses largely upon memory: how information is selectively gathered from individuals' environments, stored in memory, accessed later, and integrated into decisions or judgments. A central tenet of social cognition work is that the structure of knowledge stored in memory has important impact upon how that knowledge is used and upon the judgments people make with it. Two people with identical knowledge about an object will sometimes make different judgments about it if the individuals differ in terms of the organizing structures, called schemas, linking their bits of knowledge together.

Expertise is the principal variable used in social cognition research to differentiate people with different knowledge structures from one another. People who are experts in a par-

ticular domain are thought to have more elaborately structured knowledge in that domain, allowing them to process information more quickly and accurately and to store larger volumes of relevant knowledge in memory. Experts are thought to be best prepared to infer unobserved characteristics of actors and events on the basis of observed ones and are thought to evidence substantial belief constraint.

The first five empirical chapters demonstrate that these general claims are readily applicable to political cognition. At first glance, these chapters seem to address a wide range of issues. One examines the bases of presidential candidate evaluations, another studies perceptions of candidates' issue positions, a third refines symbolic politics theory, and a fourth attempts to explain the decline in partisanship since the 1950s. However, all of these chapters compare citizens who are political experts (as gauged by political knowledge, exposure to political information, or interest in such information) to those who are political novices. This theme provides a useful frame with which to review these chapters.

Hamill and Lodge demonstrate that political experts are (1) especially knowledgeable about the ideological orientations of political actors and about the typical attitudes of liberals and conservatives, (2) quicker and more accurate when making ideology-related judgments, and (3) better able to remember ideology-related information. Lau shows that political experts are more likely to base their evaluations of political actors on considerations of group benefits, policy issues, and candidate personalities, therefore evidencing greater constraint among their political judgments. According to Conover and Feldman's evidence, people most interested in politics are likely to infer candidates' issue stands by using (1) the candidates' party affiliations in combination with perceptions of the parties and (2) the candidates' ideologies in combination with beliefs about liberals and conservatives. This is also evidence of stronger constraint among political beliefs. Sears, Huddy, and Schaffer show that among political experts constraint among values, issue positions, and ideology is greater, and Miller demonstrates that partisans (who might be thought of as party experts) (1) have more party-related information stored in memory, (2) are more likely to perceive dif-

ferences between the parties in terms of policy stands and past performance, and (3) are more likely to evaluate the parties on the basis of the candidates who represent them, yet another form of constraint. All this and other evidence in these chapters builds confidence in social cognition's basic theoretical perspective and refines our understanding both of political cognition generally and of differences between political sophisticates and novices.

The critical reader is likely to find these conclusions quite reasonable but may nonetheless be a bit dissatisfied with these chapters. The schema concept appears prominently in all of them, but the authors present no direct evidence of the existence of schemas. Instead, their existence is either assumed (e.g., Sears, Huddy, and Schaffer) or is measured by (1) mentions of a concept in response to standard open-ended survey questions (e.g., Lau), (2) correct answers to quasifactual questions (e.g., Hamill and Lodge), or (3) willingness to place a political actor on a seven-point scale (e.g., Conover and Feldman). The goals of these chapters are to test predictions derived from ideas about schema functions; confirmations of these predictions are the only evidence here for the existence of schemas and for the accuracy of the theoretical perspective. However, since many of the hypotheses tested can be justified on theoretical grounds quite well without any mention of schemas, the skeptic may argue that the schema concept does not play as important a role in this research as the authors suggest.

The last three empirical chapters turn away from recent research in social cognition. Drawing on other work by social psychologists, the authors argue for the importance of three bases of political judgments that have received little attention in the past. Kinder argues that judgments of presidential candidates' personalities are potent bases for citizens' candidate preferences. He shows that assessments of competence, integrity, leadership, and empathy drive overall candidate evaluations over and above the impact of party, issues, and other such factors, and that the weights attached to personality assessments vary across candidates in intelligible ways. Tyler reviews literature documenting that citizens' judgments of procedural and distributive justice influence their evaluations of political actors and government more generally. Finally, Roseman, Abelson,

and Ewing argue that people differ from one another in terms of the emotions they typically feel and that evaluations of political organizations depend in part upon the match between the emotions a citizen typically experiences and the emotions aroused by the organizations' appeals. In these chapters, social psychology is responsible for alerting researchers to these useful independent variables.

The empirical work in all these chapters can be criticized on a number of grounds. For example, Lau devotes no discussion to the fact that voters he claims do not have issue schemas nonetheless evidence strong effects of their issue attitudes on candidate evaluations (see p. 119). Conover and Feldman, Kinder, and Roseman, Abelson, and Ewing fail to provide empirically validated explanations for between-candidate, between-issue, and between-emotion differences they uncover. Sears, Huddy, and Schaffer's regression analyses testing indirect causal effects offer more support for their hypotheses than they recognize (see p. 179). Miller performs regressions with cross-sectional data in efforts to explain trends over time without performing the necessary longitudinal analyses. Tyler's regressions fail to deal with the problem of reciprocal causal effects. Hamill and Lodge document that political experts process political information differently than novices do, but they fail to link information-processing strategies to consequential political behaviors.

These problems and others are not trivial, but they do not discredit the important contributions of these chapters. Overall, this volume represents a top-notch integration of the newest ideas in social psychology with the sophisticated study of U.S. citizens' political thoughts. The implications of this body of work are far-reaching, as the final section's chapters illustrate, and it will be interesting and thought-provoking reading for both political scientists and psychologists.

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Toward a Just Social Order. By Derek L. Phillips (Princeton: Princeton University Press, 1986. x, 460p. \$50.00, cloth; \$12.50, paper).

This ambitious book has three general aims. One is to give an account in terms of the best available sociological theory of how social order arises and is maintained. A second is to construct a rational justification from that and from the best available ethical and political philosophy for the kind of social order we ought to build and sustain: a just social order. And a third aim of the book is to argue for the legitimacy, within social science, of the sort of normative sociology represented by the conjunction of the first two aims. This last involves showing that normative propositions about justice, no less than explanatory ones about social order, are capable of rational justification.

Given Phillips's previous work, neither the aims of his new book nor its excellence comes as a surprise. His *Equality, Justice, and Rectification* (London: Academic Press, 1979) considers a few of the same normative issues, and both that book and his *Wittgenstein and Scientific Knowledge* (London: Macmillan, 1977) address the legitimacy of what is sometimes called normative sociology. This new book is characterized by masterfully lucid and compact exposition, together with accurate criticism, of some of the same themes. But its scope and its unrelenting emphasis on rationalistic ethical theory are startling. It deserves careful study, along at least the following lines.

First, the exposition and criticism. Wittgenstein once said that philosophy is the process of assembling reminders for particular purposes. Phillips follows that route over a vast terrain, summarizing solutions to the social order problem (private interest, situational, consensus, and conflict theories), recent work in ethics and political philosophy (Rawls, Nozick, Habermas, Gewirth), theories of socialization (Freud, Parsons, Kohlberg), major theories of law (with accounts of natural law theory, legal realism, positivism, and a long consideration of Ronald Dworkin's views), as well as recent accounts of children's rights and major approaches to the rational justification of an obligation to obey the law.

All of this material is deployed as part of the constructive argument of the book—meaning

that the object is to weigh competing theories against each other and then to adopt, develop, and integrate the best of the lot into a coherent account of the just social order. This is a project fraught with the temptation to lay other people's theories out on Procrustes' bed, and the steadiness with which Phillips resists that temptation is admirable. As a result, the expositions he gives are almost invariably models of brevity, clarity, and accuracy (but two cautions are noted below). The expositions are intellectually rewarding to read quite apart from the general line of argument they serve.

The second area that deserves careful study concerns the suddenness with which Phillips occasionally incorporates other arguments into his own (or at least forms an alliance with them). In my view, he is too ready to use even his properly qualified version of Kohlberg's account of the form of moral development, and too ready to ignore both the methodological criticisms of Kohlberg's work and the lines of inquiry opened by Gilligan and others. This is not a mere quibble. To the extent that Kohlberg's critics can successfully attack the identification of moral development with the ability (and propensity) to think in terms of rationally justified, universalizable principles, Phillips's project in this book will be more difficult.

There may be a similar problem with Phillips's endorsement of Alan Gewirth's account, in *Reason and Morality* (Chicago: University of Chicago Press, 1978), of a rational foundation for the generic rights of rational agents to freedom and well-being. Those rights—and their rational justification—are central to Phillips's vision of the just social order. Gewirth's monumental book certainly deserves the central place Phillips gives it; it is the most rigorous and thoroughgoing rational justification of rights in existence.

Philosophers are likely to blink, however, at the firmness with which Phillips dismisses the most widely held and fundamental objection to Gewirth's deduction. Again, this is a point of some consequence for Phillips's argument. Gewirth is generally acknowledged to have shown that rational agents must, on pain of self-contradiction, assent to the proposition that certain sorts of freedom and well-being are necessary goods for all rational agents. What is at issue is the next move in his argu-

ment—the conclusion that rational agents must assent to the proposition that all have rights to freedom and well-being. The issue, here, put another way, is whether Gewirth has produced a foundation for a value-based (teleological) morality or, as he claims, a foundation for a duty-based (deontological) one. Phillips's project goes much more smoothly with the deontological foundation, and he is certainly aware of the controversy on this issue. It may deserve more attention than he devotes to it, however, given the central use he makes of Gewirth's arguments.

One more thing about Gewirth: one of the reasons Phillips finds Gewirth's work so congenial is that it is worked out in terms of the generic features of actual human agents—rather than in terms of the hypothetical, idealized rational agents so often used in ethical, political and economic theory. It is also important, philosophically, that Gewirth works from *within* an agent's point of view, coming to what he calls "dialectically necessary" conclusions. What he shows, strictly, is not that rights exist "out there" somewhere, but that an agent cannot without contradiction deny certain propositions. It would have been interesting to see what use Phillips could have made of this aspect of Gewirth's position.

The major area in which this book should repay close study, however, is in Phillips's outline of a just social order. The general features of the outline are familiar ones: effective and humane socialization; social control through a liberal democratic political order; an economic framework of market socialism. Nor are its more detailed features (children's rights in the socialization process, the importance of emotions, the justification for obedience to law) radically innovative. Phillips does not intend them to be. It is not his purpose to produce a *novel* vision of the just social order. Rather, what he wants to show is how a fairly familiar vision of it may be filled out and rationally grounded not only in what we know about the way things are but in what we know about the way things ought to be. The best thing about this book, in my view, is its sustained argument that we must put those two areas of knowledge back together.

LAWRENCE C. BECKER

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Philosophical Papers. Vol. 1, *Human Agency and Language*. By Charles Taylor (Cambridge: Cambridge University Press, 1985. vi, 294p. \$39.50, cloth; \$12.95, paper).

Philosophical Papers. Vol. 2, *Philosophy and the Human Sciences*. By Charles Taylor (Cambridge: Cambridge University Press, 1985. 340p. \$39.50, cloth; \$12.95, paper).

In these two volumes, the distinguished Canadian philosopher Charles Taylor has brought together 20 of his previously published papers, to which he has added two new essays and an illuminating introduction. Several of these essays—especially "Interpretation and the Sciences of Man" and "Neutrality in Political Science"—have attained near classical status since their publication (and subsequent republication in various edited volumes). Taylor's admirers and critics will, however, be disappointed to note that a number of his recent essays—for example, "The Philosophy of the Social Sciences," in *Political Theory and Political Education*, edited by Melvin Richter (Princeton, 1980) and "Understanding in Human Sciences" in *Review of Metaphysics* 34 (1980)—are unfortunately absent from the present collection. Even with those regrettable omissions, however, these two volumes are remarkable for their range of learning, subtlety of argument, and philosophical acumen.

Despite the appearance of diversity—ranging from "Hegel's Philosophy of Mind," "Cognitive Psychology," and "Theories of Meaning" in volume 1 to "Understanding and Ethnocentrism," "Foucault on Freedom and Truth," and "What's Wrong with Negative Liberty" in volume 2—these essays are, as Taylor confesses, "the work of a monomaniac" armed with "a tightly knit agenda" (1: 1, 8). A number of themes recur. Foremost among these is Taylor's attempt to articulate and justify an alternative account of human reason, action, and agency. He is deeply dissatisfied with the "modern" (i.e., post-seventeenth century) view that the "values" we hold represent individual choices from among a range of options, any "mix" of which is equally plausible and none of which is intrinsically preferable to any other. Against this modern orthodoxy Taylor argues the case for a radically different account of who and what we human beings are. Our identities are not

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contingently or causally related to the values we hold and the choices we make, but are constituted by the ways in which we understand ourselves and interpret the meaning of our own and others' actions. Like Heidegger and Gadamer (and, more distantly, Herder and Humboldt) Taylor holds that the human world is an "expressive" or communicatively constituted world of utterances, gestures, and actions whose meaning must, in order to be intelligible and justifiable, be interpreted by the participants themselves. Unlike other creatures, we human beings are by nature "self-interpreting animals" (vol. 1, chap. 2). These interpretations are constructed out of the concepts and categories of understanding made available to us by the language we speak.

Yet the moral language of modern man, Taylor argues, has been deeply imbued with and impoverished by naturalistic theories of language, meaning, and action (vol. 1, chaps. 1-4, 9-10). More than a metatheory or a philosophy of social inquiry, naturalism constitutes a system of meaning and "deep evaluation" in terms of which modern men and women have attempted—disastrously, Taylor thinks—to order their lives and their communities. Several of these essays are, accordingly, devoted to criticizing naturalism in its

several guises and varieties, including behaviorally oriented political science (vol. 2, chaps. 1-3).

Taylor is scarcely less critical of the "deconstructive" turn taken by "post-structuralists" like Derrida and Foucault (vol. 2, chap. 6), and their greater grandfather Nietzsche. Their theories of human language, agency, and action are "underdemonstrated," "impressionistically [sic] argued for," and "hostile and dismissive towards the scientific outlook" (1:7). What is needed, Taylor maintains, is not more derision but a better and more critical understanding of the sources and attractions of naturalism. The elements of such an interpretation are tantalizingly sketched in his lucid analyses of "political atomism" and the liberal-individualist notion of negative liberty (vol. 2, chaps. 7 and 8) and, more particularly, in "The Concept of a Person" (vol. 1, chap. 4).

Taylor is the first to admit that these essays are exploratory rather than definitive. These "promissory notes" will, he says, soon be redeemed in a larger and more systematic work. Until that arrives we are very fortunate indeed to have in hand this weighty and impressive preface.

TERENCE BALL

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AMERICAN POLITICS

Intergovernmental Management: Human Services Problem-Solving in Six Metropolitan Areas. By Robert J. Agranoff with the assistance of Valerie Lindsay Rinkle (Albany: State University of New York Press, 1986. x, 199p. \$39.50, cloth; \$12.95, paper).

Robert Agranoff's concisely written monograph springs from the basic authors and texts toward which most political scientists nod recognition when embarking on an exploration of intergovernmental relations (IGR). With appropriate homage paid to David Walker, Deil Wright, and the Advisory Committee on Intergovernmental Relations (ACIR),

Agranoff moves toward understanding the implementation of policies that derive from an intergovernmental context.

Through a series of fairly logical steps, the author explains that intergovernmental management (IGM) involves daily transactional arrangements among public administrators and among their organizations. His focus is on local government and the quasigovernmental and not-for-profit agencies that constitute various policy arenas. Such a concentration on local agencies and their interrelationships is justified by arguing that the local level is where federal and state programs ultimately have to work.

To provide an action orientation to IGM,

Agranoff chooses problem solving as a model for understanding interorganizational coordination. Although he introduces the concept as a rational, formal, systematic process, he concludes from the study that it is not always rational, formal, or systematic, nor does it always explain the multiple relationships in a complex intergovernmental environment. It is useful, however, for understanding that successful implementation of human service programs involves the use of creative and conflict management as techniques for coordinating agencies that must work together.

The research to which these various ideas are applied consists of six exhaustive case studies of IGM. Drawing on field work of over 80 persons in six cities, Agranoff analyzes case studies of human services implementation in a common conceptual framework. By painstakingly adhering to a rigorous methodological sequence, he has largely escaped the idiosyncratic liabilities of the case study. Hence, his conclusions can be defended on grounds of intuitive logic as well as on its well-documented comparability. No doubt only the most ardent students or practitioners will wade through the densely detailed cases; however, they are readable and may serve as useful lessons for the intergovernmental or public administration classroom.

Of most lasting usefulness, perhaps, are the general findings about IGM and the research agenda Agranoff proposes in the last chapter. Since he places his objectives in understanding the management of interorganizational programs within a highly fragmented IGR system, rather than proposing reform of that system, Agranoff's findings are most relevant to the public administrator or to those who teach such practitioners. In a larger sense, his rich research agenda speaks to a wider range of serious IGM students who may be curious about "getting things done" in a day-to-day, operational fashion.

Given the increased demands of current devolution and decreased funding from Washington, such research is critical not just to creative management but to agency survival as well.

DAVID SINK

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Making the Managerial Presidency: Comprehensive Reorganization Planning, 1905-1980. By Peri E. Arnold (Princeton: Princeton University Press, 1986. xiv, 374p. \$37.50).

Peri E. Arnold has made an important contribution to the literature on the presidency. His analysis of comprehensive reorganization planning from Theodore Roosevelt's Keep Commission to Jimmy Carter's Reorganization Project identifies the critical role played by reorganization planners in developing and gaining acceptance for the concept of the managerial presidency. While reorganization rhetoric has stressed the neutral goals of economy and efficiency, the central purpose of reorganization has been to enhance the president's capacity to manage and direct the executive branch. Arnold concludes that "at one and the same time reorganization planning aimed at strengthening the presidency while presenting the issue of enhanced presidential capacity as merely managerial and irrelevant to politics" (p. 159).

To some extent Arnold covers the same ground as previous studies of federal reorganization and reorganization projects by Oscar Kraines, Richard Polenberg, Herbert Emerich, William Pemberton, Harvey Mansfield, Sr., and Ronald C. Moe, but he has unearthed a good deal of fresh material. He provides the first detailed examination and evaluation of the studies done by the Joint Committee for Reorganization during the Harding administration, President Eisenhower's Advisory Committee on Government Organization, President Johnson's Price and Heineman task forces, and President Nixon's Ash Council. He demonstrates that each of the reorganization efforts, while established under different political auspices and ostensibly for different purposes, shared common values and operated from a common set of theoretical assumptions.

The continuities are striking. Arnold correctly observes that the Brownlow Committee, rather than being an innovator, distilled "a tradition of thinking in public administration that reached back to the work of the President's Commission during the Taft administration" (p. 116). Scholars have largely ignored

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the Harding administration's Joint Committee for Reorganization, but it anticipated the Brownlow report with its emphasis on the managerial centrality of the presidency in the executive branch and its concerns with budgeting, departmental organization, and presidential staff resources.

By focusing almost exclusively on reorganization commissions, committees and task forces, Arnold underestimates the role played by the Bureau of the Budget in the 1940s and 1950s in articulating and promoting the doctrines of executive supremacy in administration and the managerial presidency. While presidents may have appeared to rely primarily on ad hoc reorganization vehicles, this was not, in fact, the case. Arnold does note the close relationship between the Bureau of the Budget (BoB) and the first Hoover Commission and BoB's influence on the commission's report on management of the executive branch. The bureau also provided the staff support for Eisenhower's Advisory Commission and Johnson's Price task force on reorganization. The bureau was responsible for developing a reorganization program and implementing the provisions of the reorganization act and it was by no means subordinate to the ad hoc groups.

While stressing the importance of the attempts by reorganizers to transform the president into a manager, Arnold concludes that the effort has failed. He believes that the managerial presidency may become "a trap, offering increased capacity and influence to presidents but creating even greater expectations about presidential performance" (p. 362). Many now question whether the doctrines advanced by the Brownlow Committee and the Hoover commissions are relevant for the federal government as it is organized and operates in the 1980s. It is significant that President Reagan has criticized prior reorganization efforts as being concerned too much with structure and too little with process. Control of procedures and regulations becomes more important than structure when the federal government increasingly relies on non-federal agents to administer its programs.

Making the Managerial Presidency is likely to be the definitive work on federal reorganization. It has the virtue of being readable—something that cannot be said of many books on government management and reorganiza-

tion. Despite its price, it merits a wide audience.

HAROLD SEIDMAN

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Apathy in America, 1960-1984: Causes and Consequences of Citizen Political Indifference. By Stephen Earl Bennett (Ardsley-on-Hudson, NY: Transnational, 1986. x, 198p. \$30.00).

Are U.S. citizens more apathetic about politics than they used to be? If so, what difference does it make? These are the questions that guide Stephen Earl Bennett's analysis. Bennett makes the concept operational with University of Michigan survey data by combining a question on interest in political campaigns with a question on interest in politics in general.

Beginning with a cursory glance at U.S. history, Bennett reviews the trends in voting behavior and various theories of political participation. Next comes one of the highlights of his book, a careful justification of the method described above, which he calls the "political apathy index." Unlike other conceivable scales, it measures only the degree of political *interest*, and not behavior, partisanship, or other concepts that have muddied other indexes. Always admirably aware of just how far his data can take us, he finds the index fairly stable from 1960 to 1984, although somewhat lower in recent years than in the late 1960s and early 1970s. It correlates with the variables familiar to those versed in the literature of political behavior: education, age, gender, degree of partisanship, sense of civic duty, and external efficacy. Bennett also seems taken with generational explanations and argues them well. Those born in the 1940s were especially interested in politics in the 1960s, and the elderly are increasingly interested in recent years. The apathetic, as expected, do not follow politics in the mass media, know much about public affairs, vote, or engage in other political acts.

Bennett has produced a well-argued and diligent analysis of a phenomenon important in democratic theory. Some of the data should have been presented in a manner easier for the reader to grasp, such as more simplified tables

and some graphs, and the author's gingerly use of multiple regression should have been augmented by some path analysis to flesh out the causal sequences. There are a few methodological glitches and points where the data presented do not bear out his conclusions. The author's own admonition about not confusing interest with voting turnout is ignored in the historical overview, and the discussion of generations should have been informed by that literature. These are peripheral qualms about an otherwise admirable job of data analysis and presentation.

Ultimately a study such as this raises some of the most fundamental questions of the relationship between citizens and the state. As proficient as he is at survey analysis, and although he does summarize the writings of various theorists and scholars about the proper degree of political behavior, Bennett seems relatively uncomfortable with macro-level analysis. Noting in several places how little political participation U.S. citizens engage in, he does not begin to explore why this is so. No attempt at comparative analysis is made in order to ascertain if U.S. citizens are unusual in this regard, and, if so, why. Theorists of a variety of ideological backgrounds have attempted to relate macropolitical phenomena to declining citizen involvement, from Samuel Huntington and his "democratic distemper" to Michael Parenti's rejection of democratic capitalist politics. Bennett mentions Parenti, but only to dismiss his position as "tendentious" (p. 32). But survey data, being at the micro level, cannot give us a picture broad enough to get at these essential questions.

These limitations, one must add in Bennett's defense, are common among those who engage in survey research. He has produced a fine piece of micro-level analysis of an important subject. Perhaps he will next turn his talents to a system-level explanation of why many people just don't care.

HOWARD L. REITER

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Politics and Social Welfare Policy in the United States. By Robert X. Browning (Knoxville, TN: University of Tennessee Press, 1986. xiv, 205p. \$18.95).

American Social Welfare Policy: Dynamics of Formulation and Change. By David A. Rocheft (Boulder, CO: Westview Press, 1986. xii, 206p. \$22.00, paper).

Since the late 1970s, an expanding empirical literature on the growth of the welfare state in the affluent market-oriented democracies has significantly altered interpretations of welfare policy determination imparted by the industrial society theorists of previous decades. Robert Browning begins his new book on the post-World War II growth of the U.S. welfare state by echoing an important theme of this literature—that ideology and political institutions matter.

Browning initially hypothesizes that the partisan balance in Congress, the party of the president, the interaction of these factors, the electoral cycle, economic conditions, shifts in the jurisdiction of relevant congressional committees, and incrementalism determine variations in social welfare expenditures. The bulk of the analysis actually focuses on the hypothesis that Democratic presidents and Democratic congresses, particularly those with large numbers of nonsouthern House Democrats, facilitate welfare spending growth. Consistent with these propositions, Browning shows that Democratic presidents are four times as likely as Republican ones to initiate new welfare programs and that Democratic congresses actively initiate new programs under both Democratic and Republican presidents. However, an examination of real percentage growth rates reveals that total welfare spending and income security outlays (by far the largest welfare category) grew most rapidly under three Republican presidents (Eisenhower, Nixon, and Ford) and one Democratic president (Johnson). The Reagan administration is the one notable exception to the tendency for this spending to grow more rapidly under Republican presidents. Growth of in-kind spending, such as outlays for health, education, and social services was most pronounced during three Democratic administrations (Truman, Kennedy-Johnson, and Johnson), although spending accelerated signifi-

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cantly during the second Eisenhower and Nixon presidencies.

Regression analyses of the welfare spending effects of Democratic presidents and the number of nonsouthern House Democrats, controlling for unemployment rates, confirm that Democratic presidents and the number of nonsouthern House Democrats did not facilitate growth in total or cash welfare outlays. Statistical models actually reveal that the number of nonsouthern House Democrats depressed the growth of these expenditures during both Democratic and Republican presidencies. The most notable finding to emerge from the analyses is that in-kind welfare expenditures grew more rapidly as the number of nonsouthern House Democrats increased during a Democratic presidency. Yet Browning is able to obtain this result only if Jimmy Carter, a Democratic president faced with significant pressure to control in-kind outlays, is treated as a Republican when coding presidential administrations.

Browning offers several plausible explanations for these findings. He argues that the generally higher rates of spending growth during Republican administrations were the results of Democratic congresses "bidding up" the social spending requests of Republican presidents. This argument may indeed explain some of the observed patterns in the data and statistical findings. However, the explanation is partially undercut by the statistical analyses clearly showing that increases in the number of nonsouthern House Democrats, which presumably make Congress more pro-welfare, systematically depressed total and cash welfare spending growth during Republican administrations. Browning interprets this finding as a product of the relationship between recessions and Democratic electoral success. In his own words, "Their increased numbers in Congress are usually associated with declining expenditures for social programs associated with declining unemployment" (p. 130). However, this explanation holds only if one does not account for the welfare-spending effects of unemployment; Browning's finding that nonsouthern House Democrats dampen the growth of total and cash welfare spending emerges from statistical models that control for unemployment.

The most plausible explanation for these anomalous findings is that Browning fails to

account systematically for many likely sources of variation in social welfare outlays, attempting instead to attribute much of the variation in the data to partisan control of Congress and the presidency alone. For instance, Browning largely ignores such widely discussed determinants of social welfare spending as changes in the sizes of the poor and elderly populations, economic growth rates, extra-institutional collective action by black Americans, and structural changes within the economy, among others. Although the book provides a wealth of aggregated and disaggregated data on welfare programs and supplies several useful insights into the policy-making roles of Congress and the president, failure to account systematically for these and other forces simply impairs our ability to disentangle the causal roles of partisan and nonpartisan factors.

In several ways, David Rochefort's new book on the sources of social welfare policy is more satisfying. Rochefort's thesis, as he admits, is not entirely original. James Patterson's *America's Struggle against Poverty, 1900-1980* (Cambridge: Harvard University Press, 1981) is excellent companion reading. The author's basic argument is that major innovations in social welfare policy are the results of changing images of individual responsibility and perceptions of the seriousness of the social problem and its causes held by policy makers, activists, and the general public. Images and definitions of social problems change as the result of a complex interplay among historical events, scientific discoveries, social research, shifts in professional and disciplinary paradigms, demographic changes, and broad intellectual and political currents. In other words, mass and elite attitudes, social scientific ideas and theories, and concomitant problem definition determine policy. This social images perspective on social welfare policy formation is developed in the context of a critique of existing theoretical perspectives. The plausibility of the social images perspective is arguably established by case studies of the enactment of the Community Mental Health Centers Act of 1963; of passage of important policies directed at the elderly including the Older Americans' Act, Supplemental Security Income, Medicare, and amendments to the Social Security Act between 1965 and 1972; and of Richard

Nixon's failed Family Assistance Plan.

Rochefort's thesis, however, is not without problems. The principal theoretical deficiency is the author's lack of sensitivity to how ideas of policy makers may interact with self-interest and ideology and to how action based on ideas may be conditioned by policy maker reaction to the conscious mobilization of group interest. While Rochefort does not discount such factors completely, he explicitly distinguishes the social images perspective from the view that economic and other material interests are central to an understanding of policy formation (p. 147). However, numerous examples of the policy-making roles of these widely discussed factors may be gleaned from Rochefort's own case studies. Sensitivity to the interplay of ideas, interests, and ideology during theory development would have improved the theory's richness and applicability.

In sum, both books, despite the aforementioned problems, make a contribution to the literatures on social welfare policy formation and policy studies more generally. They also point to a deficiency in the rather large literature on U.S. welfare policy; we have numerous partial accounts of welfare policy determination and few efforts at theoretical integration and subsequent empirical analysis.

DUANE SWANK

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Cloning and the Constitution: An Inquiry into Governmental Policymaking and Genetic Experimentation. By Ira H. Carmen (Madison, WI: University of Wisconsin Press, 1985. xv, 223p. \$22.50).

Two logics permeate modern science. As a *form of knowledge*, scientific inquiry is defended as an intrinsic good, obtained in a forum of free inquiry, underwritten by the presumption that only the most compelling (and rare) circumstances should limit the right to pursue knowledge wherever it may lead. This logic leads to the ideology of basic science. As a *form of power*, the scientific enterprise manifests itself in the Baconian formulation of science as a form of control and conquest and an instrument for human betterment. Seen this way, science becomes the servant of the state,

the corporation, or of whoever is able to command it. This leads to the ideology of science as a national resource, to be harnessed, channeled, and ultimately controlled for policy-determined objectives.

Genetic research since the Watson and Crick breakthrough—specifically recombinant DNA, gene splicing, and cloning—confront scientific researchers and policy makers with the acute issue of the social control of a new application of fundamental knowledge that has the potential for both very useful and very harmful consequences. It is to the political and particularly the constitutional/legal aspects of genetic experimentation that this book addresses itself. Carmen's introductory chapter, "Science in American Constitutional History," is densely packed with information and provocative generalizations, and provides a succinct overview of the history of science in the U.S. polity. While not comprehensive, it is a stimulating discussion of the constitutional context of U.S. science policy. Chapter 2, "Cloning as American Constitutional Freedom," argues that DNA research, as a specific form of scientific activity, is entitled to a considerable measure of constitutional protection. He maintains that "just as government lacks carte blanche to manipulate free expression through the purse strings, so it lacks carte blanche to manipulate scientific experimentation in general and recombinant experimentation in particular by attaching capricious constraint to munificent reward" (p. 35). Chapter 3, "The Recombinant DNA Debate as Constitutional Debate," describes in considerable detail the contours of that debate, with all the major actors making their appearance on the constitutional stage, playing their parts, and making their case. Carmen's prism is the constitutional issues raised by the controversy, the manner in which scientists, National Institute of Health (NIH) officials, and others reacted to a social phenomenon "prompted by a unique methodology and potentially unique outcomes" (p. 60). He concludes that scientific development in this area (as, no doubt, in others!) is "capable of causing not only culture shock but constitutional shock" (p. 61). His focus is on exploring the dynamics and consequences of developments in a volatile area of scientific experimentation in terms of the impact on the political domain. The complexities and subtleties of that process and of the

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issues brought to the fore are carefully and neatly delineated.

Carmen undertook a series of interviews with a random sample of recombinant DNA scientists and biosafety committee chairmen at universities (the sample is rather small). Chapter 4, "Cloners and Their Watchdogs," is based on these questionnaire and interview materials. The data indicate that the researchers are "as a whole much more receptive than are the Institutional Biosafety Committee (IBC) chairmen to the NIH model of review and oversight through binding norms of laboratory decorum" (p. 135). Not surprisingly, both researchers and chairmen (who are typically scientists themselves) overwhelmingly support the proposition that scientific inquiry is a form of constitutionally protected free speech. Scientists believe that governments are "limited in their capacity to impose strings" upon their work (p. 151). This may be wishful thinking to some degree, since the partnership of science and government, is often itself a subtle form of control in the interest of those in power.

The concluding chapter, "Recombinant Constitutional Issues," is woven around Carmen's concept of "a living constitution" in which the new challenges and demands of change and accommodation develop into a "new constitutional profile . . . to meet the many challenges of genetic engineering, while at the same time keeping faith with yet deeper patterns of right conduct" (p. 161). Carmen sees politics as an essentially neutral activity. "Like cloning itself, politics can be a tool for achieving any number of purposes . . . which, very often . . . are not so neutral in their social implications" (p. 170). The neutrality of the process is open to question, since it ignores the issue of the infusion of ideology, the shaping of knowledge by the social forces at a given historic period. Implicit in Carmen's model is a refined version of the invisible hand, a muted hymn to the virtues of the pluralistic model of U.S. politics. For a less sanguine view, one which also focuses on institutional parameters and control of U.S. science, the reader might turn to David Dickson's *The New Politics of Science* (New York: Pantheon, 1984).

Notwithstanding these caveats, Carmen's argument is tightly constructed and impressive in its command of the constitutional aspects as these relate to the history of the scientific and

political debate over recombinant DNA research. Carmen provides much incisive scholarship on the profound problems with which he grapples. For the serious student and scholar a close and careful reading should prove most rewarding.

JOSEPH HABERER

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**Contemporary Constitutional Lawmaking:
The Supreme Court and the Art of Politics.**
By Lief Carter (New York: Pergamon Press, 1985. xviii, 217p. \$29.50, cloth; \$12.95, paper).

This book's purpose is to get jurisprudence out of a bind that, Carter says, flows from two facts: first, modern philosophy (pragmatism, hermeneutics, and deconstructionism) tells us that belief in the determinateness of legal texts, the intelligibility of the intent or goals of lawgivers, and the attainability of a national consensus about fundamental political values are naive pipedreams; but, second, we also know that community can only be maintained if citizens believe in the normative goodness of the community and their participation in it. Professor Carter offers an "aesthetic theory of jurisprudence" as the way out of this bind.

Early sections of the book describe U.S. constitutional history as a series of discrete events that cannot be understood in the light of any coherent legal principles, although there are some minimal political patterns observable in it (e.g., judicial decision making based on the justices' hunches about social conditions and on their normative values). Interpretivism (which he calls "preservatism") is sharply dismissed on the basis of empirical observations (judges have not acted that way) and theory (hermeneutics shows that interpretation is really a form of creation). Professor Carter takes an interesting trot through recent constitutional scholarship (Wechsler, Bickel, Choper, Ely, Horowitz, Chayes, Miller, Perry, and the critical legal studies movement), finding various attempts to provide political norms of goodness for evaluating constitutional decisions to be deficient, especially because of their obsession with the false question of legitimacy, which is a surrogate for fundamental and scientifically unanswerable normative ques-

tions of political philosophy. He then discusses "normative alternatives to interpretivism" (e.g., pragmatists, Posner, Rawls, Dworkin, Murphy, Fiss), which are inadequate but do, more or less, contribute elements to the "aesthetic theory of jurisprudence" developed in the last two chapters.

According to this aesthetic theory, there are no right answers in constitutional law on the basis of legal reasoning. Yet we cannot stop there, because we need genuine faith in the existence of moral principles in order to make community possible. What makes for "good" opinions is good performances, which have the power to constitute community by creating momentary visions of coherent normative order, thereby sustaining faith in our ability to converse morally. If there is any substantive value implied in this approach it is the need to respect and protect individual dignity, without which conversations about political goodness cannot proceed. We need those conversations to create the experience of order and meaning so as to "protect us from the dark"; that is, the fact that, although meaning is essential to the survival of the race, it is an illusion. The "truth" of a constitutional opinion, accordingly, lies in the fit it persuasively creates between the facts of the case, the rules that the Court's audiences deem to bear on the dispute, beliefs about the empirical nature of the community, and normative claims that exist in common discourse.

Professor Carter's theory is an illuminating display of what happens to constitutional law when it becomes rooted in modern philosophical assumptions that destroy the possibility of genuine philosophy or, indeed, of any meaningful speech. If we're all just huddling in the dark and all meaning is illusion—although it is not clear how we can know the truth that we are incapable of knowing the truth—conversations are no better than silence. It is doubtful, moreover, whether his theory really constrains justices, giving us evaluative criteria for distinguishing between better and worse opinions. The assumption in favor of human dignity seems gratuitous, and in its absence the community could respond favorably to performances that create "visions of coherent order" that look like hell. If all meaning is illusion—and Professor Carter explicitly "rejects at the outset any framework based on statements of the universal rights of man" (p.

161)—there are no grounds for restricting individual freedom, but no grounds for defending it either.

Noninterpretivists will find this book an imaginative, even daring, effort to elaborate a new justification for an activist judiciary. Interpretivists—those who believe that judicial review should be confined to enforcing the clear command of the Constitution—will, in their own way, find it a useful example of what happens when the very possibility of interpretation is rejected.

CHRISTOPHER WOLFE

Marquette University

No State Shall Abridge: The Fourteenth Amendment and the Bill of Rights. By Michael Kent Curtis. (Durham, NC: Duke University Press, 1986. xii, 275p. \$24.95).

Some authors have all the luck. Michael Kent Curtis's good fortune is to have his book on the meaning and intentions of the Fourteenth Amendment published at a time when the legal community can still be heard buzzing over the remarks of the attorney general to the effect that the Bill of Rights does not apply to the states, that the familiar theory of incorporation is a constitutionally suspect judicial innovation. The book, therefore, possesses a timeliness that no doubt will earn for it careful scrutiny from those with the potential for influencing our constitutional law.

Curtis, of course, is not entering virgin territory. Inquiry into the intentions of the framers of the Fourteenth Amendment, specifically whether or not these men expected their revised Constitution to nationalize the protection of civil liberties, has long been the subject of intense scholarly and judicial interest. In arguing that the rights in the Bill of Rights were rights that, through the first section of the amendment, limited the powers of states as well as the federal government, Curtis aligns himself with Crosskey and Justice Black in opposition to Fairman, Berger, and most of the justices who have sat on the Supreme Court since 1868. He suggests that the failure to comprehend the underlying purpose of section 1 accurately is attributable to a neglect of the broader historical context from which the Fourteenth Amendment emerged. The only

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way to make sense of the issue of intentionality is to examine the efforts of the framers against the backdrop of the antislavery crusade that set the political agenda in the decades leading up to constitutional change.

Curtis focuses his attention upon the constitutional and political thought of the Republican party. He summons considerable evidence to demonstrate that Republicans believed that the Bill of Rights limited or should be made to limit the powers of the states and that this was not an issue that divided conservatives and radicals. There had long been dissatisfaction with the South's interference with rights guaranteed by the Bill of Rights (notably the right of free speech). The determination to remedy this grievous anomaly, according to Curtis, was the motive force behind the sponsors of the first section. In developing his thesis that the privileges and immunities clause was to be the principal vehicle through which this crucial task was to be accomplished, the author provides a detailed, careful rejoinder to the contrary interpretations of scholars such as Fairman and Berger, whose work has had considerable influence in shaping the course of constitutional development.

Curtis is careful not to overstate his conclusions. "In a real sense one can never prove that the amendment was designed to apply the Bill of Rights to the states" (p. 217). It is impossible to read this book without taking seriously the strong possibility that this was precisely the design of those who crafted the amendment. The debate will continue, and Curtis's significant volume should assume a prominent place in the ensuing discussion. That discussion should seek to resolve two related issues of fundamental importance that are not directly addressed in *No State Shall Abridge*.

The first is suggested by an increasingly observable phenomenon: the tendency of scholars seeking a broader role for the Court in guaranteeing rights to cite as authoritative the work of Fairman and Berger as a necessary step in the abandonment of original intent. Curtis, who more than once makes evident his endorsement of such an expansive role, must (as Justice Black did) confront the possibility that his interpretation of the Fourteenth Amendment will in the end be used to limit the Court's role. Thus, his findings suggest that the privileges and immunities clause protects the

fundamental rights of U.S. citizens against state intrusion, but only those rights stipulated in the document. To this reviewer, at least, he has not produced evidence that would support the contention of John Hart Ely that this clause was a delegation to future constitutional decision makers to protect rights nowhere specifically indicated in the document. The radical implications of the Republican achievement may not appear so radical within a transformed jurisprudential environment of non-interpretive review.

Perhaps inadvertently, then, Curtis's work will be as much of a challenge to activists as to self-restrainers. This suggests one final point. From Chief Justice Marshall's opinion in *Barron v. Baltimore* in 1833, to *Dred Scott v. Sandford*, to the post-Civil War amendments decisions, and down to our own time, the Supreme Court, according to Curtis, has been consistently wrong in its understanding of the responsibility of the states for the protection of individual liberties. If true, then, it is more than just a rhetorical ploy to wonder about increasing the authority of an institution that has made such a mess of our constitutional liberties over so many years. Curtis's impressive book should find a receptive audience on both sides of the debate over the appropriate exercise of judicial review.

GARY J. JACOBSON

Williams College

Minority Vote Dilution. Edited by Chandler Davidson (Washington, DC: Howard University Press, 1984. xi, 298p. \$24.95).

The Joint Center of Political Studies, which commissioned this volume, is a research institute that conducts public policy studies of "special concern to black Americans" and promotes "informed and effective involvement of blacks in the political process" (jacket copy). The underlying assumption of this book is that minority vote dilution is "the most common form of electoral discrimination in the United States today" (p. vii). In 12 chapters, 16 authors define minority vote dilution, analyze its history, parse statutes and judicial decisions, and recommend legal doctrines, electoral arrangements, and enforcement mechanisms to root out minority vote dilution.

These essays are a powerful example of the use of social science to advocate public policy. Although the policy goals were plainly foreordained, that has not compromised the high quality of this book's scholarship.

Indeed, this volume's assault on multimember districts, the leading contemporary method of minority vote dilution, has already borne fruit. The Supreme Court, in *Thornberg v. Gingles* (54 U.S. Law Week 4877 [1986]), adopted an interpretation of the Voting Rights Act that permits "results-based" challenges to multimember legislative districts. The justices cited four chapters of this book and other works of several of its authors as support for their decision.

Minority vote dilution is broadly defined by Chandler Davidson as "a process whereby election laws or practices, either singly or in concert, combine with systematic bloc voting among an identifiable group to diminish the voting strength of at least one other group" (p. 4). At-large elections, racial gerrymandering, anti-single shot ballot rules, slating groups operating in either primary or general elections, decreased size of governmental bodies, and runoff requirements have all been used to dilute minority representation. This volume devotes a chapter to each of the first four of these vote dilution devices. J. Morgan Kousser, however, goes beyond this short list of dilution techniques: in deriving lessons for a "Second Reconstruction" in the 1970s and 1980s, he identifies 16 methods used to hamper black political power during the First (post-Civil War) Reconstruction.

Several authors strongly criticize the Supreme Court for insisting in *City of Mobile v. Bolden* (446 U.S. 55 [1980]) that multimember districts unconstitutionally dilute minority voting only where the intent or purpose is discriminatory. Peyton McCrary, J. Morgan Kousser, and Chandler Davidson and George Korbel establish in separate chapters that southern progressive voting reforms—including at-large elections, multimember districts, and commission government—had a discriminatory purpose or intent. Armand Derfner decisively demonstrates that the Voting Rights Act amendments of 1982 were designed to overturn *City of Mobile v. Bolden*'s purpose or intent requirement. *Thornburg v. Gingles* adopts that position.

James Blacksher and Larry Menefee go still

farther: they reject established constitutional doctrine that discriminatory intent or purpose is essential to establish a Fifteenth Amendment voting rights claim. The Supreme Court not only relied on Fifteenth Amendment cases to establish the one-person-one-vote doctrine under the Fourteenth Amendment, but it detected violations of that doctrine entirely on the basis of "effects" (i.e., numerical variations) without any attention to intent or purpose. An effects test to establish Fifteenth Amendment violations of black voting rights, through various dilution devices, also seems warranted.

An insightful analysis of administrative enforcement of section 5's preclearance of electoral changes is rendered by Howard Ball, Dale Krane, and Thomas Lauth. The Voting Rights Act applies a "bottleneck principle" by requiring that all state and local electoral changes receive Justice Department or judicial preclearance. The department attempts to involve local black political leaders in the preclearance process, thus forcing white political leaders to negotiate electoral changes with local blacks. But the authors raise very serious doubts about section 5 enforcement: many electoral changes are apparently never precleared, many are submitted so close to elections as to preclude thorough review, and many are so vigorously pressed in every detail that Justice's objections are significantly eroded.

If there are disappointments in this volume, they are a weak chapter that fails to persuade that discrimination against blacks has deprived them of public benefits and a chapter that argues for such alternatives as proportional representation and add-on voting, which are at such odds with U.S. practice as to be implausible.

On the whole, however, this volume combines outstanding scholarship in diverse disciplines with forceful advocacy that has already had a major impact in reshaping Supreme Court doctrine.

DAVID ADAMANY

Wayne State University

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Stability and Change in American Politics: The Coming of Age of the Generation of the 1960s. By Michael X. Delli Carpini (New York: New York University Press, 1986. xxv, 374p. \$40.00).

The ambitious goal of this book is to estimate the effect of "the sixties" (here defined as the years 1963-1974) on mass political attitudes and behavior, especially on the attitudes and behavior of the "sixties generation." As such it is, in its author's words, "a detailed case study of the political impact of generational replacement" (p. xx). Further, Delli Carpini wants to argue, first, that during the sixties values and ideas were diffused from the peripheral to the dominant subcultures in the United States (a reversal of the normal process); and, second, that the generational change represented by such value transformation has not persisted because it was not accompanied by structural or institutional changes.

In addressing the "impact" question, Delli Carpini is faced with the traditional problem of separating generational, period, and life-cycle effects on aggregate measures of attitude (or, more correctly in this case, of opinion). His solution is to use dummy variables to indicate membership (or nonmembership) in a generation and to indicate the potential presence or absence of period effects. Ordinary least squares analysis is used to estimate the effects of the three factors (generation, life-cycle, and period) on mean responses (of Michigan survey respondents) to a number of survey questions (I confess to a desire to see percentages here: what exactly is the difference between the sixties generation and their elders on measures of efficacy, political interest, and so forth? One might note also that constructing scales, rather than analyzing individual items, would have produced greater reliability and more concise analysis).

The resultant findings are interesting, if not surprising. For example, the sixties generation is more "liberal" than other generations, although between 1952 and 1980, period effects and life-cycle effects were pushing them (and the population) in a conservative direction. In terms of partisanship, the sixties generation is a "generation without a party"; they tend also to be less psychologically involved in politics. These descriptions of the

sixties generation and of their impact on the aggregate changes in partisanship and participation should be familiar from the Jennings and Niemi studies and from such survey-based research as that of Nie, Verba, and Petrocik's *The Changing American Voter*.

A major problem with this analysis lies in its definition of generations, particularly the sixties generation. The solution chosen by the author to the identification problem assumes that one either is or is not a member of a particular generation; this means that assignment to a generation is critical. The categorization of all individuals who were between the ages of 1 and 35 between 1963 and 1974 as part of the "sixties generation" is far too broad. Moreover, this group is differentiated only by age (younger members are assumed to be "socialized by the sixties," whereas somewhat older members "experienced" the period, and the oldest are assumed to have been "ambivalent"). It is odd that despite the discussion of Mannheim, one of his central concepts, that of generational "units," is not used here. Mannheim's notion was that different groups (those in different socioeconomic positions, for example), would "work up" their common generational experience in different ways. We know that some of George Wallace's strongest support in 1968 came from those in their twenties; that only a small minority of college students protested the Vietnam War; that many people who came of age in the 1960s did not share the "culture of the sixties." Yet the present analysis essentially treats all those of a similar age as being of a similar political mind.

This problem is related to the lack of even an implicit theory of political learning. There is talk of generations being "imprinted," but no real discussion of how this occurs; the role of parents, for example, is pretty much ignored. The author goes so far as to say that even if experiences shape the particular political "character" of one generation, "nothing about the development of that generation allows one to say much about the character of the . . . following generation" (p. 62). This is true only if parents play no role in socializing their children. (Paul Beck's generational theory of partisan change is not cited here but is quite relevant.)

Finally, and disappointingly, the book fails to offer evidence in support of the two arguments proposed initially, those relating to the

peripheral and dominant subcultures and to institutionalization of value change.

KRISTI ANDERSEN

Syracuse University

Environmental Law and American Business.

By Joseph F. DiMento (New York: Plenum Press, 1986. 228p. \$29.50).

Making Bureaucracies Think: The Environmental Impact Statement Strategy of Administrative Reform. By Serge Taylor (Stanford, CA: Stanford University Press, 1984. 410p. \$29.50).

If the significance of new public policies were measured in terms of visibility, then the National Environmental Policy Act (NEPA), signed into law by President Nixon in January of 1970, certainly would be considered a great success. This federal legislation has been subjected to microscopic analysis by dozens of policy scholars over the last 16 years, and it has prompted the more conservation-minded states to enact similar legislation at the state level. Even some nations abroad have copied it. The basic question remains, however: has this highly visible and minutely studied legislation changed anyone's behavior (other than the behavior of "NEPA specialists")? The two books reviewed here, like others before them, seek answers to this knotty question.

Environmental Law and American Business and *Making Bureaucracies Think* read well in tandem. Professor DiMento's research encompasses not only NEPA but other state and federal antipollution legislation that followed, for the most part, in NEPA's wake. As the title of his informative book suggests, DiMento selects as his central focus the impact of environmental laws on private sector behavior—on how industries, both large and small, have complied with the numerous conservation-oriented regulations now on the books. In contrast, Taylor's scholarship focuses more exclusively on NEPA. His primary concern is with its impact on public sector behavior. Thus, read together, one is rewarded with a quite comprehensive treatment of the extent to which both the public and private sectors have complied with the legislation of the "environmental decade." Both authors likewise agree that there exists a

considerable gap between promises and performance.

DiMento's book will interest both public policy scholars attentive to the intricacies of implementation, and practitioners, who have to find ways and means to comply with an expanding universe of governmental regulations. As the author says in the preface, "This book offers policy reforms that aim to achieve the goal of inducing business to comply with reasonable environmental law" (p. viii). After a nice introductory chapter in which he describes four cases of noncompliance ranging from the trivial to the life-threatening, DiMento gets to the crux of the issue: "The problem of violations of environmental law is immense" (p. 20). It is immense both because of the nature of environmental regulations and the possible costs involved. "The EPA estimates that the cost to comply with the Clean Water Act will be \$118.4 billion by the year 2000" (p. 30). Add to that figure the costs for air pollution reduction, toxic waste cleanup, and so forth, and it becomes abundantly clear why there is so much foot dragging in the private sector when industries are asked to clean up their acts. Not only is it expensive, but the scope of environmental legislation has become so all-encompassing that it is a practical impossibility to uniformly force compliance.

Most of DiMento's book is, therefore, a description of (1) the various tools the government has at its disposal to effect compliance and (2) a detailed analysis of strategies used by industry to get around compliance and the reasons for adopting them. The first issue can be summarized as the "three Cs" of compliance: "Government can court, coerce and cajole" (p. 65). It can bring down the full weight of the criminal justice system on non-compliers and/or it can sit down at the bargaining table and hammer out a reasonable compromise. Generally, the latter is to be preferred over the former. And concerning the second issue, industries do not comply with environmental laws for a wide variety of reasons. They utilize all sorts of guerilla warfare tactics to get around them. But not infrequently, says DiMento, the "dilemmas of compliance" with environmental laws hinge on the most basic of reasons—people do not understand them. "Among the most unresponsive [businesses], even to informal and personalized

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attempts to promote compliance, are dry cleaners, service stations, and water heater manufacturers. . . . Many of the employees of these firms do not speak English . . . and clearly do not understand the letter and spirit of some environmental rules" (p. 156).

Serge Taylor's book is also about compliance, but the drama now takes place within the federal bureaucracy and not in corporate boardrooms or under grease racks. Much has been written about this subject, and Paul Culhane has described *Making Bureaucracies Think* (appropriately, I believe) as the last published of first-generation NEPA studies. Certainly it will be of interest to NEPA scholars; its 408 pages contain a blow-by-blow description of how two of the most powerful federal agencies—the U.S. Forest Service and the Army Corps of Engineers—have proceeded in implementing NEPA. Generally, Professor Taylor's research corroborates what previous studies have found: Both of these federal agencies made serious and sustained efforts at incorporating environmental values into their activities, though not without some back-sliding. Agency behavior has been modified as a result. "There is no question that all districts and forests do better in terms of avoiding environmental damage than they did before NEPA" (p. 130).

But in my opinion the book is more interesting, and certainly more provocative, in its effort to identify environmental issues with what Taylor calls "The Science Model." As the title of his book suggests, Taylor equates the pre-NEPA bureaucracy with a thoroughly politicized bureaucracy, one that paid precious little attention to the disinterested values of science and reason in carrying out its activities. Enter NEPA, the science of ecology, and the "environmental decade." In Taylor's view, environmental analysis—and environmental analysts working within agencies—had the effect of introducing scientific norms and values into federal agencies.

There are many problems with Taylor's analytical framework. I can discuss only the most important ones here. First, he ignores the classic work by Max Weber on the nature of bureaucracy, work that postulates a close correspondence between the professionalism embodied in bureaucracy and rational-scientific values. (The only reference to Weber is in a quotation from sociologist Arthur Stinch-

combe, critiquing his *Economy and Society*.) We are all well aware that the U.S. variant of bureaucracy is more politicized than its European counterparts, but that is still far from arguing that U.S. bureaucracy is totally devoid of scientific, professional norms. Taylor comes close to saying just that.

Second, the author assumes that a concern with environmental values is by definition scientific. Many people—including this reviewer and some officials in the agencies he studied—would disagree. In their view, NEPA introduced a new set of interests, and perhaps even a new perspective, into agency activities, but environmentalism was as much a political movement as it was an embodiment of the scientific method and rational values. In fact, the science of ecology—of interrelationships, interdependencies, and impacts—is in its infancy. Even Taylor admits that "assessing environmental impacts is uncommonly difficult" (p. 17). When compared with an agency's traditional areas of expertise—in these cases, the science of silviculture and the applied science of civil engineering—ecology's scientific credentials fall far short not only of Taylor's hypothetical "Science Model" but also of these scientifically grounded and respected disciplines.

Taylor's airtight science-politics dichotomy gets him into trouble in this book as he attempts to argue that environmentalism is science and bureaucratic behavior is politics. What the book actually documents, as do others on the subject, is that the science of ecology didn't alter agency behavior; rather, it was the political pressures brought to bear by environmentalists—in the courts, in Congress, in presidential administrations, and in the press—that prompted recalcitrant bureaucrats to see the light.

JEANNE NIENABER CLARKE
University of Arizona

Politics and the Cinema: An Introduction to Political Films. By Michael A. Genovese (Lexington, MA: Ginn Press, 1986. viii, 124p. \$17.96, paper).

Political scientists who make use of feature films and documentaries in their courses or who are simply film buffs with a special inter-

est in films that deal with political topics will welcome this slim volume. The author is a political scientist who loves movies and teaches a course on political films.

Genovese considers films as tools of socialization and therefore considers all films political because of what they say or fail to say. After a brief description of the rise of the film industry, he deals with such topics as censorship, the Hollywood blacklist of the 1950s, political dramas, comedies, war and antiwar films, the specific contributions of several major U.S. and European directors and the historical images of men, women, and blacks, primarily in U.S. feature films.

There are two principal problems with this book. One is derived from Genovese's insistence on a major theme, and the other may stem from the volume's brevity. These liabilities damage the book severely.

His major theme is that "most [U.S.] films support and glorify the status quo," and he asks, Why would the wealthy "finance the making of films designed to overthrow the system which protects their wealth?" (p. 3). He misses the point. Hollywood aims for formula mass entertainment and is quite willing to package antiestablishment films if the market is there. Indeed, when he uses illustrations of political dramas, he cites *The Graduate*, *Easy Rider*, *Medium Cool*, and *The China Syndrome*, all distributed by mainstream studios, and all critical of the "system." He could also have noted that in the 1960s and early 1970s there were such films as *Getting Straight*, *The Strawberry Statement*, *Woodstock*, and *Alice's Restaurant*, all major studio efforts, and all antiestablishment. If the genre petered out, it was strictly a box office judgment. Powerful (and profitable) films that purport to hold a mirror to the nation's shortcomings are not all that rare: *Bonnie and Clyde*, *Chinatown*, *The French Connection*, *Silkwood*, *The Verdict*, *Birdy*, *The Killing Fields*, and *The Color Purple* certainly fit. Nor does he note such significant films of the old studio days on racial and religious tolerance as *Crossfire*, *Gentlemen's Agreement*, *Home of the Brave*, *The Defiant Ones*, and *Bad Day at Black Rock*.

In his brief discussion of the 1930s, the author insists that the film industry engaged primarily in ostrich cinema, virtually ignoring the depression. Yet he fails to acknowledge

such significant problem films as *An American Tragedy*, *I Am a Fugitive from a Chain Gang*, *Black Fury*, *Winterset*, *Dead End*, *One Third of a Nation*, *The Informer* and *The Life of Emile Zola*. He touches on the clumsy portrayal of politicians in U.S. films, but ignores *The Best Man* and *The Candidate*. His concern for the western as political metaphor is limited to consideration of several John Ford films, slighting such achievements as *High Noon* and *McCabe and Mrs. Miller*.

Genovese's politics are intrusive. He repeatedly and justifiably denounces the impact of McCarthyism on the nation and the arts in the 1950s, yet when he quotes Charlie Chaplin's *Monsieur Verdoux* to the effect that business is murder and murder is only a form of business, he describes the film as Chaplin's "most biting, hardest hitting piece of social criticism" (p. 37). When he notes that the Emile de Antonio documentary *Millhouse* "creatively" intersperses different quotes from Nixon, he states that the power of the film is "indisputable, even if its method *may* be biased" (p. 55, italics mine). The 1982 film, *Missing*, directed by Constantin Costa-Gavras, merits particular attention. Despite the absence of any evidence, the film charges that a young U.S. citizen was killed during the overthrow of President Allende in Chile in 1973 with the knowledge of, and probably with the collusion of, U.S. officials. Genovese concedes that "this film takes liberties with some of the facts" (p. 87), but he neglects to inform his readers that the message on the screen at the beginning of the film states, "This film is based on a true story. The incidents and facts are documented. . . ." And his concept of judicious evenhandedness leads to the following: "In the U.S. conglomerates have enormous control over which films are or are not made. In the Soviet Union, it is the state which determines what films will be made. The result in both countries is clear: often we get propaganda" (p. 90). Presumably we can anticipate the Soviet equivalent of *Apocalypse Now*, and it will be about a portly Russian colonel sequestered in an Afghan cave.

Then there are the editing problems. The author tells the story of the Department of Justice attempt to label a Canadian film as propaganda twice within eight pages (pp. 56, 64). His quote from Michael Corleone of *The Godfather* to a U.S. senator, "we're both part

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of the same hypocrisy," is invoked twice, in consecutive paragraphs (p. 76). His section on D. W. Griffith is placed between sections on John Ford and Francis Coppola. The book is also marred by far too many typos and page 83 of the reviewer's copy was a total blank; and there is no index.

In his preface, Genovese asks if the subject of political films merits an entire book. It most assuredly does.

SIDNEY WISE

Franklin and Marshall College

The Democratic Imagination in America: Conversations with Our Past. By Russell L. Hanson (Princeton: Princeton University Press, 1985. xi, 478p. \$45.00, cloth; \$12.95, paper).

It is a pleasure to praise this book for the insightful way it traces the development of competing conceptions of democracy in the United States, for its richness of detail, and for the manner in which it traverses the intellectual space that has come to separate the humanities from the social sciences. These are accomplishments of high order, and they owe much to the fact that the author grounds them in the interpretivist and critical theories that spring from the writings of (and the debate between) Habermas and Gadamer.

In the intellectual territory that now divides the humanities from the social sciences, two preoccupations rise high on the landscape. One has to do with the uses of the past. And the other is an awareness (as a literary scholar recently said) that those who study human affairs cannot escape their own involvement with the subject that they study.

Humanists, traditionally, have accepted this involvement openly and cheerfully, using their own humanity to discover what Tocqueville once called "habits of the heart" among those they study. In so doing, they become confident of their ability to move outward from a single case to the whole of the human condition. For most social scientists, accepting their own involvement with what is studied comes far less cheerfully. How, we ask, can we avoid the contamination of our own values? And—even more painful to ask—can reality be other than a social construction? Sometimes, we avail

ourselves of Weber's *verstehen* or Popper's situational logic. But with notably few exceptions, we prefer to seek the objectivity that is thought to be safeguarded by an aggregation of data and a sufficiency of comparable cases.

With respect to uses of the past, most social scientists take it as a given that the past is to be used to inform the present—to help us understand how the present came to be. But for humanists the premise is far different. The present is to be used to inform *on* the past, each generation of scholars taking on itself the obligation to use contemporary sensibilities to reinterpret the past. As a distinguished historian of art put it, the humanist's highest achievement comes when he can "look with fresh eyes" upon a subject that has become "tarnished with familiarity."

It is within these humanist traditions that Hanson holds his "conversations with our past," interposing his own definition of contemporary sensibilities between the written and verbal exchanges of those who have been important contributors to the ideas (and ideals) of U.S. democracy—all the while working "outward" from holographics to the whole of an era's political thought.

History is Hanson's organizing framework and the reader thus moves through periods that can carry familiar labels: the "Time of the Founders," the "Age of Jackson," and so on. But almost always, the author manages to look with fresh eyes upon those times, for he writes with a sensibility that is informed by the textual deconstruction of contemporary literary studies and with a sense of contemporary linguistic archaeology, peeling back layer after layer of word meanings until the terms of politics stand revealed as legitimators of an era's dominant social and economic structures.

Hanson rejects the convenience of a transcendental history, especially the idea that ours has been a progression from grudging and fragmented beginnings toward some richer and better version of democracy. If anything, according to Hanson, the development of the democratic idea in the United States has been more regress than progress. At the founding of the republic (understandably, given the social and economic class of the founders) the idea of democracy carried with it a concern for limited government, the obligations of civic virtue, and the expectation that private desires be



balanced against the common good. But on Hanson's showing, each succeeding period altered that expected balance, until, in the post-New Deal period, the idea of democracy was transformed into a democracy of "rights-bearers," a democracy whose politics has come to turn on the right to material entitlements and the satisfaction of consumer wants.

There are arresting omissions and commissions in Hanson's account. No mention, for example, is made of Thoreau and only passing reference is given to Lincoln, but there are several pages of attention to the futurist novels of Ursula K. LeGuin, the student movements of the 1960s and seventies, and that era's prophet of new consciousness, Charles Reich. But we may presume that the commissions (along with the omissions) contribute to Hanson's larger purpose: to argue on behalf of an alternative future that would bring to U.S. democracy a politics of moral concern grounded in opportunities for enhanced citizen participation in life-affecting issues and for living lives that are satisfactorily rich in non-materialist concerns.

LAWRENCE J. R. HERSON

Ohio State University

Running for Office: The Political Education of Campaigners. By Marjorie Randon Hershey (Chatham, NY: Chatham House Publishers, 1984. xii, 306p. \$12.95, paper).

Can one book say something new and significant about congressional elections, political parties, political action committees (PACs), "single-issue" groups, and representative government? This one does. In a departure from the literature on campaigns, Marjorie Hershey focuses not on what candidates do, but on how, what and from whom they learn. Her theme is that campaigns "leave their mark on American politics less by changing the officeholder's face than by influencing the officeholder's thinking—and the perceptions and behavior of challengers, activists, reporters and others as well" (p. 31).

For candidates, the political classroom of the 1980s is a tough and unruly place. Hershey sketches brilliantly the new campaign environment, discussing campaign finance, PACs, single-issue activism, the Christian Right, cam-

paign technologies, the media, and the "declining" political parties. All these combine to make the candidate's lessons more complicated and difficult than ever.

How do campaigners cope with such an environment? Drawing on social learning theory, Hershey outlines several hypotheses about how candidates learn, whether from previous experience, observation of others, or models provided by outside experts. Especially provocative is her contention that parties—or at least the GOP—may be reasserting influence by providing campaign models and assistance. Still, as the author concedes, the Democrats, as well as state and local units of both parties, are ill equipped to assist candidates, especially in competition with interest groups, PACs, and campaign consultants.

Hershey tests the hypotheses about political learning by looking at the 1980 campaigns of six liberal U.S. senators targeted by anti-abortion groups. Drawing on extensive interviews, newspaper research and personal observation, she confirms that both candidates and activists are constantly learning, that they look to appropriate models, primarily among fellow partisans or issue enthusiasts, and that they often modify strategies in response. But what they learn is problematic. Voters provide little direct feedback during the campaign, and election outcomes are "blunt instruments" for conveying what citizens really want. Indeed, only the postelection "battle of constructed explanations," dominated by political and media elites, finally crowns the victor in the war of political meaning.

Campaigns, it would seem, make only a modest contribution to representative government. Not only do elections change few faces, they often fail to attune candidates to the real concerns of voters. As Hershey notes, "the learning environment of an-elected representative systematically amplifies the voices of organized groups, other officeholders, party leaders and media figures" at the expense of the "quiet citizen" (p. 269). She offers some ways to clarify the electorate's muffled tones, such as enhancing electoral competition—perhaps through public financing of campaigns—and increasing the availability of poll information, but these seem unlikely to effect major changes in candidate learning.

Despite Hershey's convincing analysis, the reader should be cautious about generalizing

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too broadly from a case study. The political environment of the late 1970s and early 1980s may be atypically unstable, making candidate learning unusually difficult. In addition, Hershey's conclusions about single-issue activism are tied too closely to one case, the struggle over abortion. In fact, single-issue activists and strategies defy easy characterization. And, of course, races for other offices occur in markedly different environments. All these issues require further attention, but future studies of candidate learning will, we hope, match the originality and insight of this one.

JAMES L. GUTH

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Governing Buildings and Building Government: A New Perspective on the Old Party.
By Bryan D. Jones (University, AL: University of Alabama Press, 1985. xvi, 230p. \$27.50).

Jones's book is a case study of Chicago's Department of Buildings. The book's subtitle, *A New Perspective on the Old Party*, reveals a central theme of the volume—that Cook County's Democratic party is involved in most aspects of the city's handling of the built environment, ranging from the policy arena in which building codes are developed, to the administrative arena in which building regulations are enforced, to the Housing Court in which violations are adjudicated. At first blush, one might wonder whether study results showing the centrality of the Democratic party in a particular area of Cook County governance are superfluous. They are not, for several reasons. For one thing, there is remarkably little empirical evidence of party influence in the Chicago urban service delivery process, despite widespread beliefs about the party's role in this regard. Ken Mladenka's recent study of the distribution of parks and recreation, fire protection, education, and refuse collection services in Chicago, for example, left scholars with quite the opposite conclusion—that is, that "distributional decision making is routinized and largely devoid of explicit political content" (*The Urban Bureaucracy and the Chicago Political Machine: Who Gets What and the Limits to Political Control*, "APSR 74(1980):991-98). If such studies raise

doubts about the relevance of party considerations in service distribution, parties' alleged disdain for matters of policy formulation is an even more widely accepted truth, perhaps best stated in Raymond Wolfinger's piece, "Why Political Machines Have Not Withered Away and Other Revisionist Thoughts" (*Journal of Politics* 34(1972): 365-98).

Jones's empirical analysis is most distinctive and challenging to existing work in its finding that the Cook County Democratic party exerts substantial influence over distributional outcomes at many points in the building code enforcement process. Of particular interest are findings that the strength of ward organization significantly affects not only the level of citizen demands and the seriousness of bureaucratic response, but also the extent of compliance. Using a skillful combination of quantitative data analysis and qualitative case study materials, Jones shows how the party manages to influence bureaucratic outcomes through a system of "attention rules" based on strength of ward organization, existing alongside the usual "bureaucratic decision rules" based on professional, technical criteria.

At first, one expects that this volume will offer an equal counterpoint to the notion that urban political parties (at least insofar as they fit the machine politics model) are largely uninterested in substantive policy formation. But Jones's chapter on substantive changes in the building code is more ambivalent on this matter. Jones applies the corporatist concept to analysis of building code policymaking, thereby emphasizing that this is a structured and relatively exclusive policy arena rather than a freewheeling, pluralist one. Within this corporatist model, the interests of city bureaucrats are brokered along with those of major private interests such as architects and building trade unions. But, as Jones concludes, "the party itself has no interest in substantive policy making. Rather, it acts as a resource for cementing the policy system and for ensuring that the participants in the process automatically take into consideration the policy preferences of the mayor" (p. 38). While the distinction between the policy preferences of the mayor as a governing official and the resources and interests of the mayor as a party leader is a valid one, one wishes that the possible connections among these were given

greater attention in this chapter. Nevertheless, it is a rich and lively one, concluding with a look at the city's new rehabilitation code—a policymaking "incident" that contrasts with the corporatist model and serves nicely as the exception that proves the rule.

Despite its main focus on the role of the party, the most revisionist aspect of this book may be its treatment of management control. Views of the "inherent" discretion of street-level workers in urban bureaucracies are widespread, and the corruption incidents that periodically engulf Chicago's Department of Buildings might seem to corroborate the idea that street-level discretion and its abuse is endemic. But Jones shows how bureaucratic responses to scandal, involving classic management control devices, can significantly constrain street-level discretion, albeit with some unintended costs. In fact, Jones's analysis of Chicago's response to periodic corruption scandals yields an intriguing model of a two-track policy system, one track dealing with substantive policy issues and the other with periodic management control crises.

In short, Jones's volume continues a long and distinguished tradition by which the city of Chicago serves as a laboratory for students of urban politics. It is a fine example of the descriptive richness and theory relevance that a good case study must have.

ELAINE B. SHARP

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Novus Ordo Seclorum: The Intellectual Origins of the Constitution. By Forrest McDonald (Lawrence: University of Kansas, 1985. xiii, 359p. \$25.00).

I suspect the next two years will see a rash of books published to commemorate the bicentennial of the Constitution. Forrest McDonald is an early entrant in this field. McDonald sets out to "make a reasonably comprehensive survey of the complex body of political thought (including history, law and political economy) that went into the framing of the Constitution" (p. ix). After laying out the many strands of political thought current in the United States in 1787, McDonald examines the use the framers made of political theory in the Constitutional Convention. He asserts that

"the political theories and ideologies at the command of the Framers were, as we have seen, of limited practical use" (p. 26). McDonald's thesis is that the Constitution is yet another example of political actors who selectively use political theory and ideology to justify decisions made on more pragmatic grounds.

McDonald's contribution to this familiar thesis is to argue that the transitions that political theory, economic theory, and jurisprudence were undergoing in the late eighteenth century made it easier for politicians to pick and choose from the numerous theories according to their political need. This, in turn, expanded the theoretical possibilities through the combination in practice of what had previously been theoretically distinct. For example, McDonald shows how the United States in the post-Revolution era passed mercantilist measures such as protective duties on imports at the same time they advocated increased competition in the shipping industry. In another example, McDonald contrasts the experience of the States with test oaths, sequestration acts, installment laws, and paper money with that of the republican belief in the state support of public virtue as evidenced by the passage of sumptuary laws.

Unfortunately, McDonald's book reads as if it were two. The first part is a book on the intellectual history of the ideas current with the framers. Here he provides a survey of the key theoretical concepts in the 1780s, such as property rights, republicanism, public virtue, and commerce. McDonald painstakingly documents the multitude of meanings these concepts took under different authors such as Hume, Mandeville, Bolinbroke, Montesquieu, Quesnay, and Smith. He records an exciting time when major changes were occurring in political, economic, and legal theory. McDonald focuses on property rights and the significance of the change in economic theory, started by Mandeville and Smith, that promoted the beneficial effects of an unrestrained commerce. He contrasts this with the political theory of classical republicanism that argued for the necessity of public virtue to the survival of republics. He concludes that no consensus existed that the framers could draw on in designing the new constitution.

McDonald next turns to the use of these eighteenth-century intellectual controversies

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by eighteenth-century politicians. This is the second part of McDonald's work. His thesis in this part of the book is that politicians exploited the lack of consensus in theory to develop hybrids of the various theories according to their political needs of the moment. He presents case after case where politicians in the states in the postwar era used theory to suit and justify their ends. McDonald argues that the experience of the states caused for many a "reconsideration [of] their ideas about republican forms of government and about safeguards to liberty and property" (p. 178). The lessons of this reconsideration only reinforced the diversity of opinion.

So, finally, when McDonald turns to look at the decisions made at the Constitutional Convention, he finds little influence of political theory in the design of the Constitution. Part of the blame lies in the fact that the framers came to the convention with theories of politics that were diverse and often inconsistent. The rest lies in the pragmatic use the framers made of theory to justify their positions reached from other motives. The framers, McDonald writes, "used political theorists to justify positions that they had taken for nontheoretical reasons" (p. 235). Yet he lauds the outcome, the Constitution, as a significant accomplishment for republican theory. Frankly, it seems to be a variation of the story about the emperor who has no clothes where the tailor is given an award. How can that be? McDonald fails to carry through with his analysis using the categories of theory he distinguished in the first half. For example, McDonald discusses the use of the various theories of republicanism by the framers. But, instead of using the categories arrived at in his earlier chapter, he creates new categories of division and bases his analysis on them. As such, his examination of the influence of republican theory in the minds of the framers is divorced from his early discussion of its origins. Further, his analysis of the decisions made in the convention often default to routine listings of the time sequence of events. It is no wonder, then, that McDonald finds so little influence of political theory in the design of the Constitution.

Although I suspect the book was written for a general audience, it is difficult to read. McDonald's approach to the complexity of the subject is to make a series of nested lists detail-

ing the various distinctions in the intellectual origins of the ideas. The book's aim is too comprehensive to be enjoyable. It would better serve as a reference book on the intellectual precedents to the Constitution. McDonald's development of the history of the idea of an unrestrained commerce and its relationship to republican theory was the liveliest part of the book. This carries over to this examination of the origins of the contract clause in the Constitution. It is unfortunate that McDonald did not confine his work to that area.

EVELYN FINK

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American Prince, American Pauper: The Contemporary Vice Presidency in Perspective.
By Marie D. Natoli (Westport, CT: Greenwood Press, 1985. xiv, 204p. \$35.00).

Marie Natoli's characterization of the U.S. vice president as both prince and pauper highlights the distinctive awkwardness of that office. The vice president is prince by virtue of being first in the line of presidential succession, and in the last forty years such succession has taken place three times (FDR-Truman, Kennedy-Johnson and Nixon-Ford). Other than the possibility of succession, however, the vice president is pauper in that the office lacks substantial authority of its own and is entirely dependent on the president to give it significance. So many presidents in the past have assigned their vice president so little to do that the post gained a reputation for obscurity and for being a dead end for political careers.

Natoli's study tracks the emergence and development of a more visible and vigorous vice presidency in the post-World War II period to the present, from Harry Truman to George Bush. Emphasis is given to the greater political attractiveness of the office, as evidenced by the willingness of major leaders to accept the vice presidential nomination, of presidents to make more effective use of their vice presidents, and of vice presidents to seek and to be expected to seek the presidential nomination of their party. Unlike some other commentators, Natoli does not term these changes a transformation of the vice presidential office. Nonetheless, she treats the uptrend as an enduring development that has consider-

ably strengthened the place of the vice president in the political system.

Such judgments seem premature not only because the evidence is mixed and uneven but because the generic constrictions on the vice presidency remain unchanged. What the vice president does in office, as Natoli sensibly stresses, continues to be a function of what the president permits and wishes. Hence the Carter-Mondale experience, considered by many to be the contemporary highwater mark in effective presidential-vice presidential relations, may or may not turn out to be a precedent future presidents will want or feel obliged to follow.

The vice president's gain in prominence necessarily takes the form of a more active role as an agent of the president. As a consequence—and Natoli underscores the point—severe constraints and vulnerabilities exist. When acting for the president, the vice president may have to advocate personally uncongenial policies, develop an image helpful to the president but not necessarily to him- or herself, and often handicap as much as assist future prospects and ambitions. These difficulties are most pronounced for a vice president who pursues the presidential nomination. The incumbent vice president can not readily reject current policies or espouse new ones at marked variance from those of the president without inviting party disunity and opening him- or herself to charges of personal disloyalty, lack of credibility, and untrustworthiness. The heightened political visibility of the modern vice president can come, then, at very high political cost to the incumbent's presidential prospects, as was the case with Hubert Humphrey and Walter Mondale. It remains to be seen whether George Bush's faithful service to President Reagan will constitute, on balance, a plus or a minus in his quest for the presidency in 1988.

Students of the vice presidency should find Natoli's account helpful in its descriptive coverage of actual vice presidential experiences since 1945 and in its sensitivity to the tensions inherent in the office and in its relationship to the presidency. The study would have been stronger had Natoli analyzed more fully her own proposals for change and those of others, including her dismissal of the Twenty-fifth Amendment as having "little merit" (p. 91).

ALLAN P. SINDLER

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The Life of the Parties: Activists in Presidential Politics. Edited by Ronald B. Rapoport, Alan I. Abramowitz, and John McGlennon (Lexington, KY: University Press of Kentucky, 1986. 242p. \$24.00).

The Life of the Parties provides an interesting and informative examination of a relatively neglected set of political actors—state party convention delegates—and makes an important contribution to the growing debate over the general decline of the U.S. political parties thesis. It is based on surveys of both parties' state conventions in 1980 in 11 states.

The heart of the book is found in chapters 4 and 5, which were previously published in the *Journal of Politics* and *APSR*. In chapter 4 the editors argue that partisan as well as purposive (candidate and issue support) motivations were operative, that the two types of motivations were interrelated rather than mutually exclusive, and that partisan motivations depressed support for Kennedy among liberal Democratic activists. While most would agree with the conclusion that partisanship was an important motivation among state party convention delegates, differences between motivational groups are clearly evident as party identification and political experience variables are more strongly related to partisan than purposive motivations. Perhaps in a more evenly divided Democratic convention without an incumbent president running for reelection, purposive motivations would have played a more important role than in 1980. The chapter, nevertheless, is an important contribution to the literature, which heretofore has relied heavily on the 1972 national Democratic convention, a convention dominated by liberal amateurs.

Chapter 5 by Abramowitz and Walter Stone is an especially excellent piece that ultimately employs recursive path analyses to examine whether delegates were more motivated to support candidates whom they perceived as ideologically closest to themselves or those they perceived as most electable in November. While the authors conclude that electability was more important than ideological proximity continued to exert a very significant total effect on preference. An especially nice addition involves an analysis of a sample of 1972 Democratic national convention delegates, which results in a revision of the literature to reflect the greater direct effect of electability than ideology on candidate preference. I

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would caution, however, that ideology may well have been more important in 1972 than eight years later, even for the same individual delegates. While the authors stress that party activists are primarily motivated by *perceived* electability, this does not necessarily contribute to strong parties seeking moderate candidates most likely to win, since delegates' perceptions of reality may be incorrect, and reciprocal but untested linkages may exist since delegates' candidate preferences may shape their perceptions of the candidates' electability.

Other chapters also provide valuable information and insights. Chapters 1 and 2 provide a nice introduction to the mechanics and scholarly literature on the presidential nomination process in general. In a descriptive analysis of the data set, the editors in chapter 3 document the relatively high socioeconomic status of party activists, their intense partisan identification, and the significant ideological and issue differences between the parties in each state. While such basic descriptive information is helpful and necessary, a more analytic approach comparing this group of activists with the state population and with other activists in regard to these characteristics would have been especially interesting. Chapter 7, by Laurence Moreland, Robert Steed, and Tod Baker, provides an interesting analysis of the impact of migration on the state parties, discovering that Democratic newcomers to a state were more liberal than long-time residents, largely because they were younger and better educated. This suggests that some of the same factors found by Everett Ladd and Charles Hadley to contribute to the ideological polarization of the parties, such as a college education, continue to operate and that our increasingly geographically mobile society may introduce another challenge to party leaders. Chapter 8 by Ronald Rapoport will be of interest to the numerous scholars who follow the issue constraint debate, as he examines issue constraint among party activists and finds constraint highest among the college-educated and those motivated by ideological concerns.

The remainder of the book contains considerable information, though it is not sufficiently guided by theoretical concerns. Chapter 6 examines the interest group associations of each party's convention delegates, chapter 9 employs factor analysis to identify the belief

systems of each party and various groups of delegates, and chapter 11 finds that few of the delegates were ideologically extreme and that even this small group did not significantly differ from other delegates in other important respects. Chapter 10 addresses the time-honored theme of the permeability of the parties, pointing out that Democratic delegates who had switched to that party in the 1960s were especially well educated, liberal, and attuned to the issue concerns of that decade.

This book will be of special interest to anyone specializing in political parties and political behavior generally. There is a definite need for more study of such midlevel party activists, and the identification and explanation of longitudinal trends can only be achieved by replicating this study in future years.

STEPHEN D. SHAFFER

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The Jesse Jackson Phenomenon: The Crisis of Purpose in Afro-American Politics. By Adolph L. Reed, Jr. (New Haven: Yale University Press, 1986. xii, 170p. \$17.50, cloth; \$5.50, paper).

Adolph Reed, Jr.'s *The Jesse Jackson Phenomenon* is among the first published of a number of works on the 1984 Jackson campaign for the Democratic presidential nomination. It is an explosive book destined to provoke debate and disagreement on Jackson himself, the appropriateness of a challenge for the presidency by a minister, the reality and the meaning of possible class differences among blacks and their support for Jackson, the significance of black concerns for the Democratic party, and the role and legitimacy of non-elected individuals and institutions. All of these are fascinating, complex, and important issues. Unfortunately, Reed's treatment of them is limited in two ways. He is right on a number of issues and he has interesting insights, but many of his hypotheses are left unsupported by data based either on his own research or on existing materials. Secondly, the analysis is hampered by Reed's unwillingness or inability to bring a reasonable form of discourse to his subject matter.

Reed is correct in saying that although the

black church is often described as an aggressive initiator of protest during the civil rights movement, it can display socially controlling, conservative behavior. The church also provides a framework within which individuals reshape their social and political behavior and external world, as Aldon Morris argues in a recent work with which Reed disagrees. Research therefore needs to be conducted on how some churches or religious leaders are more liberal while others are more aggressively conservative and on the factors that provoke the shift from a social control to a social change mode by such institutions.

Reed puts the claims of the Jackson campaign under careful examination. He cites the prediction by Jackson's supporters that he would mobilize large numbers of black voters, thereby significantly improving the Democratic nominee's chances in 1984, but Reed concludes that Jackson's voter mobilization capacities were ambiguous at best. While he is accurate in noting the weakness of Jackson's voter organization and mobilization structures, the book lacks any estimation of the total black and total voter turnout actually generated by Jackson. Reed's failure to present this information denies him the opportunity to prove conclusively that Jackson did not generate substantial black participation during 1984. Secondly, he cannot verify his claims that Jackson's support among black voters was not mass-based. While data cited by Reed, as well as data in other surveys, show that upper-income blacks supported Jackson more strongly than lower-income blacks, the candidate won a majority of black voters in every economic category in every state except Alabama. Reed also uses the failure of Jackson's allies' campaigns as proof of his failure at mobilization. He might have made this point more effectively had he compared Jackson's black allies' and opponents' turnout and total vote with the election results of black candidates who did not win Jackson's support. Instead Reed provides us with examples of a few failures, but we have no idea of the universe of electoral contests nor of the total votes involved.

Reed's concern with leadership validation of the protest and electoral wings of black politics and with the intersecting problems of conflict between elite and mass political interest pervades the book. He argues that the protest

wing has had to adjust to the existence of a new category of black political leaders in the post-civil rights era. Certainly adjustment and competition should be expected as new categories of political representatives arise, and procedures for legitimization of unelected interest groups are matters for concern, but Reed seems to think the latter lack validation simply because they are not elected. This is too facile. Reed should and does know better. The obvious implication following from this is that Jackson as a self-appointed leader had no previous electoral authorization and that his right to contend for the presidency is therefore in doubt. Reed's approach suggests that the only form of valid representation is through the electoral process. While electoral affirmation is critically important, the aggregation of political ideas, values, and concerns is accomplished in multiple ways including, but not limited to, competition for political office.

Finally, Reed must be faulted for his consistent lack of restraint. A *sine qua non* for serious work is rational discourse, but Reed too often succumbs to the cutting comment or the witty thrust instead of analysis, which suggests a basic lack of respect for his subject matter. He castigates those "unable to distinguish between a social movement and a group of people shouting in a church" (p. 12); he opines about "Pentecostals, Jehovah's Witnesses, and other flotsam and jetsam of the religious universe" (p. 58); he observes that "the [Jackson] campaign manipulated its adherents' fears and despair in much the same way as do the quick-fix purveyors of lucky rabbits' feet, millenarian cults and religions of positive thinking" (p. 37).

These observations do not mean that political scientists should not criticize the Jackson campaign or the candidate Jackson. It makes no intellectual sense to do so, however, unless the critical analysis adds to our understanding of politics in the process. There is much that is valuable in this book. Reed raises interesting and provocative questions but answers far too few of them convincingly. I expect he will do so in future work.

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The 1984 Presidential Election in the South: Patterns of Southern Party Politics. Edited by Robert P. Steed, Laurence W. Moreland, and Tod A. Baker (New York: Praeger, 1986. viii, 340p. \$39.95).

For almost a decade, Robert Steed, Laurence Moreland, and Tod Baker have fostered the study of southern politics through their biennial Citadel symposia on southern politics and a series of edited books developed from these conferences. *The 1984 Presidential Election in the South* differs from their three previous studies in that it focuses on a single election and treats that election primarily on a state-by-state basis. Although the book begins with William Havard's judicious overview of southern political trends and concludes with Harold Stanley's insightful interpretation of the 1984 election for the entire region, the rest of the study consists of careful accounts of Ronald Reagan's easy victory in one southern state after another. Each of the state chapters summarize the state's recent political history, reviews nominating activities and the general election campaign, analyzes the popular vote, and discusses the implications of the 1984 presidential campaign for future partisan politics.

The editors' decision to use individual states as the primary unit of analysis inevitably leads to considerable repetitiveness, yet the experience of reading 11 accounts of the 1984 election does have the virtue of hammering home the magnitude of the Democratic collapse and the breadth of the Republican superiority in southern presidential politics. In recent presidential elections substantial majorities of white southerners have voted Republican and even larger majorities of black southerners have voted Democratic. Since there are four times as many whites as blacks in the southern electorate, it is not difficult for the Republicans to win presidential elections despite achieving only microscopic black support. Republican success is especially likely when (as in 1984) a popular conservative Republican president runs against an undisputedly liberal Democrat. Given southern white antipathy for the candidates, issues, and groups generally associated with the national Democratic party, William Stewart's conclusion for one state—"The votes may simply not be there to construct Democratic presidential victories in

Alabama any more" (p. 87)—seems equally applicable to the other southern states.

All of the chapters are instructive. Dennis Ippolito offers a revealing account of the Texas Republicans' "victory of unprecedented and unexpected scope" (p. 159); Jack Fleer skillfully assesses the Republicans' presidential, senatorial, and gubernatorial victories in North Carolina; and Larry Sabato provides a thorough analysis of Virginia's "predictable results" (p. 276).

Although all 11 southern states awarded landslide votes to Reagan, considerable differences remain among the states in terms of Republican competitiveness in nonpresidential campaigns. Moreland, Steed, and Baker find the Republican party in South Carolina on the verge of major advances, as does Charles Hadley for Louisiana Republicans. In contrast, spectacular Republican gains in nonpresidential elections appear less likely in Stewart's analysis of Alabama, Thomas Walker and Eleanor Main's discussion of Georgia, Diane Blair's study of Arkansas, and the treatment of Tennessee by Anne Hopkins, William Lyons, and Steve Metcalf. One of the book's strengths is that contributors were encouraged to pursue the themes they considered most relevant to understanding southern party politics. As a consequence, Alexander Lamis emphasizes the salience of racial cleavage in Mississippi politics, while Mark Stern stresses south Florida's prominence as the locus of advancing Republicanism. The most ambitious chapter is Stanley's contribution, which argues that "Reagan's popularity and the Republican gains did not result primarily from white racial backlash" (p. 322) and that "Democratic dominance below the presidential level cautions against a sweeping conclusion proclaiming realignment in the South" (p. 324).

Students of southern politics will find *The 1984 Presidential Election in the South* a valuable exploration of southern Republicanism at its peak under Reagan's leadership and popularity.

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COMPARATIVE POLITICS

Chinese Marxism in Flux, 1978-1984: Essays in Epistemology, Ideology, and Political Economy. Edited by Bill Brugger (Armonk, NY: M. E. Sharpe, 1985. 218p. \$30.00, cloth; \$14.95, paper).

The Government and Politics of the PRC: A Time of Transition. By Jurgen Domes (Boulder, CO and London: Westview Press, 1985. xv, 316p. \$42.50, cloth; \$17.95, paper).

Institutional Reform and Economic Developments in the Chinese Countryside. Edited by Keith Griffin (Armonk, NY: M. E. Sharpe, 1984. x, 336p. \$37.50, cloth; \$18.95, paper).

The changes in post-Mao China have resulted in a plethora of studies addressing the actual implementation of policies and their apparent short-term results as China seeks to maintain socialism while reinstating material incentives and the pursuit of wealth. Emerging studies also reflect the increased possibilities for research in China. Field studies offer new opportunities to gather data, especially since cadres are more open in their discussions and data are more plentiful in Chinese publications.

The three books reviewed here are, to some degree, products of this new situation. The Griffin and Domes volumes are well served by the openness. Brugger is less beholden to the new data and relies more on the philosophical and ideological publications that have been integral to Chinese publishing since the early days of the People's Republic of China (PRC).

The Brugger volume consists of six papers drawn from conferences held in Australia in 1982 and 1984. The papers consider the Marxist theories that guided the pre-1978 leadership and how they have fared in the heady atmosphere of the post-Mao period. In the first chapter, Dutton and Healy argue that Mao never broke with the empiricism of Stalin, and indeed official Marxism has not yet done so. The current theories are rather a return to Stalin's empiricism, with the class struggle "collapsed into the economic instance"

(p. 3). In the second chapter, Sullivan discusses the theory of continuing revolution. Brugger follows with an able analysis of the problems of extensive and intensive development. Next, Hannan argues that Marxist-Leninist theory is inadequate regarding discrete modes of production. Additional chapters deal with the continued strength of the state and the law of value. The arguments, though theoretical and sometimes abstruse, are surprisingly well joined by internal comments and cross-references. It is possible to follow what are often obscure arguments, but the volume is likely to be attractive primarily to Asian specialists.

The exceedingly interesting anthology edited by the distinguished scholar Keith Griffin is composed of papers authored by development scholars rather than area specialists, who draw on field visits to China in 1982. The extensive and informative charts in the book are based on data from communes in Yunnan and Sichuan that the authors visited, and they make use of other material as well. The volume commences with a discussion of institutional change and income distribution (Keith Griffin and Kimberley Griffin) and goes on to a detailed analysis, "The Responsibility System and Institutional Change," by A. R. Khan. Subsequent chapters treat issues of concern both to those interested in general economic and social development and to those focusing on China. In particular, the analysis of rural reforms will be of wide interest.

The chapters are well written, comprehensive, do not confuse anecdotal materials with evidence, and limit their claims to those suggested by the data available to them. To some extent, however, events have passed the authors by. Because they are development specialists, the writers tend to emphasize continuity in politics. They note the ways in which pre-1978 policies have been adapted and stress that a number of the current changes have deep roots in Chinese economic history. Some judgments do not stand up well. For example, in the discussion of whether conditions had improved, it seems to be assumed that the mal-

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nutrition they observed will continue. For the most part, however, the authors are judicious and careful, and their balanced presentation of detailed data makes the volume especially useful in understanding the changes and tensions in contemporary China.

Jürgen Domes's book is certainly the most valuable of the three volumes reviewed here for political scientists. Domes is a well-known scholar, indefatigable in his research, who works both in China and in Taiwan. This book focuses on the post-Mao reforms. There is some discussion of the Chinese political tradition and the geographical and economic constraints on China. Much the largest part of the book, however, is devoted to the issue of leadership transition and includes analyses of key individuals, the compromises of 1980-83, and the possible lines of future development. Domes's book is a gold mine of information on elite backgrounds and characteristics. More than most scholars, he has the knack of organizing his work so that it maintains its relevance in a constantly changing field. This is a very useful book for the scholar and the more advanced student.

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Sozialpartnerschaft in der Krise: Leistungen und Grenzen des Neokorporatismus in Österreich. Edited by Peter Gerlich, Edgar Grande, and Wolfgang C. Müller (Vienna: Böhlau, 1985. 412p. oS386, DM58, paper).

Austria's social partnership, an institutionalized system of cooperation of the main interests among themselves and with the state in the making and implementation of economic and social policies, has been identified with the country's economic successes of past years. How will new challenges, such as the economic crisis of the eighties, the need for industrial restructuring, and the emergence of post-materialistic movements affect this system? Will it adapt or break down? Will it contribute to or impede Austria's response to these challenges? These questions explain the title of the volume, which contains contributions by political scientists and economists (academics as well as practitioners) from Austria and the Federal Republic of Germany. But the

focus of the volume is broader than is indicated by the title. It deals not only with the crisis-related questions mentioned above. It discusses not only the Austrian social partnership, but also the broader concept of neocorporatism and the theoretical debate about it. Thus the structure of the book, apparent in the chapter titles (e.g., "Preconditions and Development of Social Partnership," "Social Partnership and Politics," "Social Partnership and the Economy"), obscures an underlying, different structure. Some of the 14 chapters are almost exclusively devoted to the Austrian case (chaps. 2, 3, and 9-12), while others emphasize to various degrees theoretical and comparative aspects (chaps. 1, and 4-8).

The chapters with a theoretical or comparative focus are *prima facie* of greater interest to readers whose interest extends beyond the Austrian case. Among them, the chapter "Social Partnership in Comparative Politics Research" (pp. 85-107) by Gerhard Lehmbrock together with Philippe Schmitter, one of the initiators of the discussion on neocorporatism, is a valuable contribution to that discussion, as are the chapters by the editors. The chapter by Wolfgang Blaas and Alois Guger, "Industrial Relations and Macroeconomic Stability in International Comparison" (pp. 255-77) draws on data from 17 OECD countries for its conclusion that macroeconomic stability is more likely in countries with a cooperative structure of industrial relations and a cooperative social climate than in countries with a conflict-oriented social climate.

The chapters focusing on Austria provide new and valuable insights, especially when they deal with hitherto somewhat neglected aspects of social partnership such as the chapter by Müller on the role of the political parties (pp. 135-224), the chapter by Grande on the relevance of legal norms for conflict management (pp. 225-54), or the chapter by Nowotny on the relationship between finance policy and the functioning of social partnership (pp. 313-29). These insights are useful in a broader sense because of the paradigmatic significance of this special case of neocorporatism.

There is no unanimity among the authors about the existence of a crisis of the social partnership nor as to whether there are grounds for optimism or pessimism. Günter Chaloupek

and Johann Farnleitner, whose views represent those of two components of the social partnership—namely the Chamber of Labor and the Chamber of Trade respectively—are most optimistic (pp. 331–54). But the authors agree in general that the social partnership is more than a "fair-weather product," for instance Talos (p. 83), Grande (p. 244), and Farnleitner (p. 349). The more pessimistic authors develop scenarios regarding the possible effect of the economic crisis on the stability of the system. Under conditions of economic growth, corporatist cooperation is a non-zero sum game, in which all participants profit. When a decline in economic growth leads to a zero sum situation, will the unions under pressure from the rank and file give up their cooperation, or will capital do so in the face of a sharpening international competition? (see, e.g., pp. 17–18, 106–7). As for the unions, Grande (p. 245) and Blaas and Guger (p. 259) point out that the aggregation of rank and file demands into a "solidaristic wage policy" allows a monopolistic, centralized trade union federation to adjust wage demands to long-range considerations of the nexus between international competition, higher wages, and the employment situation. As for the employer side, Ewald Nowotny (p. 322) believes that the trend in other countries toward more aggressive strategies (e.g., use of the threat of increased unemployment to achieve wage restraint) will not be followed in Austria because of the strong position of the unions. Lehmbruch also notes (p. 91) that the use of unemployment as a resource for economic policy making remains a taboo. Chaloupek (p. 342) sees no evidence of a more aggressive incomes policy on the part of the unions.

Still, there is broad agreement that present tendencies will or should lead to adaptations in the social partnership. Wolfgang Fach and Gerd Gierszewski, in the chapter most critical of social partnership, foresee a transition from the present "gentle" corporatism to an "austere" corporatism in which the social partnership will allocate the cost of the economic crisis asymmetrically to the wage earners (pp. 291–94). Gerlich (pp. 1226–32, 366) and Nowotny (pp. 327–28) see a possible adaptation in a partial decoupling of the social partnership from government and a restriction of its activities to the sphere of income and employment policy with greater autonomy in

that sphere. Müller (pp. 180–208) documents such a tendency by case studies.

There is also the question whether the social partnership contributes to or impedes Austria's response to the economic crisis. Is it flexible enough to make a contribution, or is it fostering stability while handicapping dynamism and innovation? In particular, does it handicap the process of industrial restructuring, which is necessary under present circumstances? Chaloupek, who deals with the question in some detail (pp. 336–40), does not share the "structural pessimism" of others, and he is satisfied with the tempo of structural adaptation. But other answers to these questions are ambivalent, indicating some doubt about the flexibility of the system (pp. 285–90, 363–64). Georg Winckler believes that the social partners will discover their long-range interest in restructuring and innovation once they recognize that there are budgetary limits to the assumption of risks by the state. The question is to what extent they will be able to bear the costs of cyclical and structural risks that threaten to disrupt the explicit and implied understandings between them (p. 312).

This sketchy tracing of some of the themes of the book may have indicated its richness in details and insights. Contrary to many edited volumes, the contributions are of almost uniformly high quality. The extensive bibliography (pp. 367–95) is an additional bonus.

KURT STEINER

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The Political Economy of Corporatism. Edited by Wyn Grant (New York: St. Mary's Press, 1985. xii, 274p. \$29.95).

Varieties of Corporatism: Theory and Practice. By Peter J. Williamson (New York: Cambridge University Press, 1985. x, 244p. \$39.50).

Far from assuming the status of a normal science, research in political science is increasingly informed by distinctive and competing paradigms of political and social change. To pluralism and Marxism (in their many varieties) must be added public choice analysis and corporatism. While corporatist thinking is hardly new—it is now more than fifty years since Mihail Manolesco published his *Le Siecle*

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du Corporatisme—it has received fresh impetus over the last 15 years as a model applicable to politico-economic arrangements in advanced capitalist countries.

As theory, however, corporatism has not advanced very far. Unlike pluralism it has failed to generate a rich and complex body of empirical research. And it patently lacks the intellectual and emotional appeal of Marxism or the clarity and analytical power of public choice analysis. Both of these volumes are aware of these failings and, indeed, are attempts to provide greater theoretical coherence to corporatist thinking.

The Political Economy of Corporatism is an edited volume in the St. Martin's series, *Sociology, Politics, and Cities*. Perhaps this explains the slightly odd juxtaposition of four chapters on the relationship between corporatism and local government and politics (mainly concerning Britain) and five rather disparate chapters on corporatist theory, Austria, industrial relations, and the European Community. Wyn Grant's introduction does provide some unity to the volume by stressing that all the contributions concern "societal" as opposed to "state" corporatism. Societal corporatism is "characterized by the existence of singular, noncompetitive, hierarchically ordered representative organizations which are autonomous in their origins. Through an osmotic process they develop a symbiotic relationship with the state, so that the legitimacy of the state becomes in part reliant on the active consent of recognised interest organisations" (p. 10). Grant offers further refinements on this theme, but unfortunately none help very much. Societal corporatism ends up as a catch-all description on the complex pattern of state-societal linkages that prevail in all western liberal democracies.

Philippe Schmitter's contribution labels the same phenomenon "neo-corporatism." Schmitter is eager to disabuse his audience of any inference that neocorporatism is behavioral (i.e., depends on the preferences of civil servants) or functional (i.e., serves the interests of capital) or tactical (i.e., gives the impression of state neutrality while really serving some dominant class). Instead, neocorporatism is *structural*, or "grounded in the institutional interests of the state" (p. 41). But what are these interests? In effect, they consist of the constitutional and institutional status quo.

In stable democracies, politicians and civil servants want to remain in power, but there are structural constraints on the things they can do to remain so. It is these constraints that help mold state-societal relations. The obvious retort to this theoretical refinement is, so what? For, analytically, it adds little to the noncorporatist perspectives of the pluralists and neopluralists (Almond, Lindblom) or such non-Marxist theorists of the state as Nordlinger.

Of the remaining contributions, the most important is Bernd Marin's piece on Austria, in which he systematically demonstrates that in many crucial respects, Austria is not the paradigm case of what he calls liberal (or societal or neo-) corporatism.

Peter Williamson's book on *Varieties of Corporatism* is a serious attempt at systematic political theory. His task is a formidable one, for he sets out to test the analytic utility of the concept of corporatism. He does this by first examining what he labels *consensual-licensed* corporatism, or the essentially nonstatist corporatist thinking which emerged in Catholic states following the demise of the *ancien régime*. Second, he studies *authoritarian-licensed* corporatism, or the politico-economic systems associated with fascist Italy and Salazar's Portugal. Finally, he returns to postwar neocorporatism, which he duly relabels *contract-corporatism*.

Williamson writes well and thinks clearly. For each of his models, he sets out a list of defining characteristics preceded by a full discussion often studded with empirical references. His chapters on *consensual-licensed* corporatism primarily concern the communitarian thinking of such writers as Manolesco, La Tour, and Othmar Spann. Unlike the authoritarian corporatists, they had little opportunity to put their theories into practice, so Williamson is concerned only with the coherence and unity of their ideas. His chapters on Italy and Portugal are insightful and useful. But when he tackles postwar corporatism in Europe he encounters the same problems as Grant and Schmitter: What precisely is the role of the state in corporatist theory? Can neocorporatism easily be distinguished from pluralism? Are the varieties of corporatism (within and between countries and over time) so vast as to invalidate the approach as a useful analytical tool of political science? Unfortunately,

Williamson is unable to answer these questions. Like the Grant volume, his book, while interesting and sometimes provocative, adds as much confusion as clarity to the ongoing debate on corporatism. Ultimately, corporatist theorists depend on open-ended taxonomies rather than on a theoretically powerful central idea based on some conception of human nature or of economic interest. Along with other middle-range theories, corporatism is most valuable when we accept its limitations and use it as an adjunct to other more powerful theories of political and economic change.

DAVID MCKAY

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Spain After Franco: The Making of a Competitive Party System. By Richard Gunther, Giacomo Sani, and Goldie Shabad (Berkeley: University of California Press, 1986. xix, 516p. \$42.00).

Certain political events are so dramatic and so emblematic of larger issues that they seem virtually to tell their own story. Getting the facts straight may be the paramount challenge when, as with revolution or civil war, access is difficult, institutions are breaking down, ideological distortions are rampant, and the language of the protagonists has to be learned from the outside. The temptation is to let well enough alone: to assume that, if these difficulties can be overcome, it is superfluous to spell out the theoretical and cross-national implications of the conflicts under study. This approach is attractive to the degree that comparative politics often provides little in the way of rigorous conceptualization of rapid change.

If such a narrative strategy is to work, it has to depend on the skills of the storyteller and on a fascination intrinsic to the events that make up the story. Richard Gunther, Giacomo Sani, and Goldie Shabad are knowledgeable observers of Spanish politics. However, they recognize that, unlike the civil war of the 1930s, the transition to democracy in Spain that got under way in the 1970s has been occasionally exciting but mostly a matter of prolonged interelite bargaining and of ambiguous shifts in public opinion. At the same time, they also understand that the success of the Spanish

transition was scarcely foreordained. Although there has not been much political theater in contemporary Spain in the histrionic sense, drama of another kind has been plentiful.

It is in working the area between calculated efforts at conciliation and the threat, never far from the surface, of explosive confrontation—as exemplified in the deals struck by party leaders on religious, regional, and economic controversies once thought to be intractable—that *Spain after Franco* makes a real contribution. The drama may be oblique, but it is present nonetheless. Interviews with over 100 national and regional elites, careful attention to institutional regulations—for example, changes in electoral laws—and a lucid reconstruction of events from an abundant primary literature (newspapers, magazines, party documents, and memoirs by some of major actors themselves) enhance our understanding of the early days, from around 1976 to 1982, of the new party system. This is no mean feat. Rather than indulging in yet another world-historical overview of democratization—a genre that has come to consist for the most part in bows toward Gramsci and studied neglect of Lipset—the authors deliver an important account of short-term, grounded learning. This is refreshing, and it makes for a good read.

Yet, in other respects *Spain after Franco* is a disappointment. Much of the empirical analysis, especially the several chapters devoted to a mass survey conducted in 1979 with a brief look at survey results from 1982, is weak. And the book's contribution to democratic theory is marginal.

Analytical problems are not confined to the surveys of public opinion. Invaluable as they are as oral history, the elite interviews are occasionally underinterpreted. The elites are treated as informants rather than respondents. They are sources of descriptive information. But their discourse itself, the rhetorical style of the elites—a tendency to express calls for moderation with a quasi-religious fervor, as well as other slips between form and content—is overlooked. Moreover, the substantive value of these interviews hinges on the frequency with which the elites reveal perspectives that go beyond common knowledge of the sort available in newspapers. A by-product of *Spain after Franco* is that it confirms the suspicion that some Spanish elites read the

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newspapers but have few novel insights. Finally, the authors do not give sufficient emphasis to the synergistic combination of generational resentment, grasp of the utility of the mass media, boredom with extremist posturing, and lower-middle-class ambition that gave inexperienced leaders who were widely thought to be mediocre and even authoritarian such persistence, and such an opportunity, to push for democracy. Without taking these factors explicitly into consideration, it is difficult to understand why men like Adolfo Suárez, Felipe González, and others displayed such a mix of moderation and determination and succeeded when the obstacles to success were so high.

Still, these blemishes are minor in comparison with the problems encountered in the handling of the mass survey. The analysis of the data is organized around class, religion, regionalism, and left-right orientations. The approach is sensible insofar as these are the major cleavages of Spanish politics. But the outcome is a string of findings that fail to hang together and that are on occasion of doubtful validity.

The problem stems from a lack of cross-national comparability in the items and, more broadly, of a theoretical architecture to the questionnaire. The static nature of the survey need not have been so restrictive had the authors included items that could have been matched with questions from surveys carried out in other industrial democracies or had they shown greater familiarity with phenomena addressed by the survey research community, for instance, economic voting, system support, protest activity, and the like. Perhaps the most damaging lapse, in a book about the development of partisanship and the party system, is the failure to use standard or even plausible indicators of partisan attachment. Another glitch is an indiscriminate reliance on feeling thermometers and other shopworn gadgetry of the survey method.

The initial justification for resorting to such a parochial version of survey research must have been the belief that Spanish politics is "unique." The fallacy is evident: there is no way to establish or reject this idea if some measures are not comparable in the first place. Besides, as the authors surely appreciate by now, the key question concerns the relatively distinctive features of Spanish politics, not its supposed uniqueness.

The analysis compounds errors built into the design of the survey. Not a single causal model is estimated in the more than 200 pages of *Spain after Franco* devoted to the statistical study of the mass survey. Instead, the presentation plods along with cross-tabulations, correlations, and factor analyses. These are computational preliminaries that might more suitably have been relegated to an appendix. The what-goes-with-what mode of exposition not only neglects multivariate techniques but it also confuses independent with dependent variables and sometimes generates quite improbable inferences, such as the notion that younger Spaniards are more likely to consider themselves of the working class than are older Spaniards.

While these mistakes reflect limited methodological refinement, the root of the problem is theoretical rather than technical. As an example, the claim that the 1982 election, in which the incumbent center-right party collapsed and the socialists were swept to power, constituted a realignment assumes not only that partisanship has been adequately measured but also that the Spanish party system was aligned, rather than inchoate, to begin with. The first assumption is false. The second, in a new party system, is tenuous.

On balance, *Spain after Franco* has many of the virtues and some of the defects of a pioneering work. It is an indispensable guide to the elite negotiations that set the Spanish parties off and competing under conditions of great uncertainty. In this regard, the study is as close to definitive as we are likely to get. It is a flawed analysis of public opinion. Its contribution to democratic theory and comparative politics is tangential. Like its subject, it is a transitional book.

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Politics in Britain and the United States: Comparative Perspectives. Edited by Richard Hodder-Williams and James Ceaser (Durham, NC: Duke University Press, 1986. xvi, 232p. \$35.00).

A group of scholars from Britain and the United States gathered at Duke University in 1984 to commemorate the founding of the doomed colony on Roanoke Island 400 years

previously by discussing developments in British and U.S. politics. The present volume contains the revised papers presented at the conference.

The participants in the conference were by and large well-known political scientists distinguished for their contributions to the study of either British or U.S. politics; only one of the participants has engaged in extensive original research on both countries. As one might expect from the participants in the conference, the essays are informed, interesting, and sometimes thought-provoking. Most suffer from the fact that their authors have much stronger credentials to write on one country (not necessarily the country of which they are a citizen) than on the other. The essays sometimes read, therefore, as though their authors had tried diligently to come to terms with secondary literature with which they were not previously familiar and that they would not have confronted had it not been for an invitation to this conference. One contributor more or less acknowledges this point by describing his essay as an elaborate reply to a final examination question that he might have been set. Hecl's essay stands out in terms of its quality, in large part, I suspect, precisely because he is the participant with the best claims to have conducted original research on both countries.

The scholars who assembled at Duke are at their strongest when they discuss the traditional core subjects of British and U.S. politics courses. The chapters on the legislature and executive, bureaucracy, judiciary, and party systems either summarize adequately existing writing on those subjects or provide—usually within the author's area of expertise—some illuminating insights. The least satisfactory section of the book attempts to explain the rise of laissez-faire economic thought in both countries without coming to terms satisfactorily with the enormous body of literature which exists on the political economy of the two countries.

Two more general reservations may be made about the volume. First, potential purchasers should be aware that two chapters—King's on scandals in Britain and the United States and Donoughue's on economic decision making during the last Labour government (apparently there was some) have been published elsewhere. Somewhat scandalously, Donoughue's piece appears in another collec-

tion from the same publishing house. Second, the participants in the conference, though rightly proud of continuing the long tradition of comparing British and U.S. politics, were almost willfully blind to the interesting comparisons that can be made between the United States and European political systems other than Britain. Heclo mentions only to dismiss from further consideration the fact the continental European bureaucracies often have features contrasting even more vividly with U.S. bureaucracy than those of Britain. King ends his essay on the study of scandals with a call for more research on the topic (though I suspect more may have been done already than he acknowledges), particularly on France. One of the many books by Philip Williams, to whose memory this book is dedicated, was partly on that topic. However, King's observation is surely sound in its implication that U.S. politics might fruitfully be compared not just with British but with other European political practices, too. This, too, is, on reflection, a point that Williams himself made many years ago.

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Labour and the British State. By Barry Jones and Michael Keating (New York: Oxford University Press, 1985. vi, 214p. \$29.95).

The basic argument of this book is that the Labour party has consistently failed to challenge the form and institutions of the British state and, instead, has adapted itself to them. The existing state is not a suitable instrument for the implementation of Labour's goals. Consequently, the party's intentions have been continually frustrated by its failure to confront the limitations of the existing state. To demonstrate this thesis Jones and Keating examine four areas: management of the economy; the handling of territorial issues; the machinery of government itself; and the issue of British sovereignty. In each area a case is made for the ambivalence and incoherence of Labour's perspective on the state. Lacking a critical perspective of its own, the party became attached to what was already there and, in recent times, has become identified as the party of the British state. Jones and Keating characterize

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the nature of this state as remote, alien, centralist, undemocratic, and bureaucratic. Writing from a fashionably decentralist perspective, they view all of these features as undesirable and contrary to the basic purposes of the Labour party.

How did this situation come about? Labour's ambivalence and lack of clarity about the state was the result of three factors: the continuity of British political institutions and the absence of a revolutionary democratization of them; the influence of the atheoretical British radical tradition, of which Labour is very much a part; and the inconsistent objectives of different components of the Labour coalition. Ultimately these factors produced an intellectual failure to develop "a more sophisticated understanding of the nature of the state and the political system" (p. 72).

The authors' definition of the state (p. 2) conjoins the notion of a political community existing on a specific territory with that of a complex of institutions operating within a given society. The territorial dimension (which provided their point of entry into research on the Labour party) has priority in their analysis. Generally, the sections dealing with territorial and national issues are the strongest in the book. Labour's centralism and incomprehension of the national question (most notably in Ireland) has made it difficult for the party to handle these issues.

What is much more problematic is whether Labour's acceptance of existing state (in its societal rather than its territorial dimension) has posed major difficulties for the party in its dealing with economic and other issues. Given the modest and limited nature of the changes they wished to introduce in British society, were Labour's leaders wrong to suppose that the existing state would serve their purposes? There is no particular reason to think so. Had Labour's project been more radical, its failure to develop a critical theory of the state would have been a greater liability, and Jones and Keating's argument on this score more convincing. In fact, their book is generally less convincing on the nature of the Labour party than the Marxian analyses they reject (pp. 67-71). Certainly the consistency of Labour's practice, which Jones and Keating recognize, can be better understood as a logical product of social democratic ideology dominant within the party, rather than as a product of its failure to comprehend the nature of the state.

Overall, however, *Labour and the British State* is a useful addition to the literature and deserves to provoke considerable discussion.

STEPHEN MCBRIDE

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Marxism and the French Left: Studies in Labour and Politics in France, 1830-1981.
By Tony Judt (Oxford: Oxford University Press, 1986. ix, 338p. \$29.95).

It is hard to dislike a book that makes such deliciously nasty remarks about French Marxist intellectuals and their cross-Channel and transatlantic admirers. How account for the bloated reputations abroad of a succession of French intellectuals from Jean-Paul Sartre to Louis Althusser (and many lesser lights between) engaged in Marxist discourse? What was political in Paris, Tony Judt points out in a thoroughly enjoyable chapter, "French Marxism in 1945-1975," became theoretical in London and "merely academic in its final resting place further afield"—for example, in the *New Left Review* and the Yale English department. "Unable to grasp the rather particular circumstances of intellectual production in France, scholars and acolytes abroad could only attribute the intensity of feeling aroused by the works of Parisian *maitres à penser* to an originality and power of pure thought, to the identifying of which they have since devoted thousands of pages" (p. 169).

Tony Judt, fellow and tutor in politics at St. Anne's College, Oxford, puts that intellectual production in proper perspective. The writings of French Marxist intellectuals that appear from abroad as disembodied verities, he argues persuasively, can be understood only as part of an ongoing debate *within* the French Communist party about its role in the polity. For Parisian intellectuals, history made it impossible to be against the party; yet they were also repelled by much within the party. They had to work out their own position of support from the outside—and some, from the inside. What foreigners thought of as self-absorbed and timeless existentialism, comments Judt, "was to its founders and their colleagues an endless search for the philosopher's stone of a phenomenological marxism that could also act" (p. 187).

Sartre transcended the difficulties of the moment—the unappealing features of the real French Communist Party (PCF)—by placing faith in the cunning of history and structures. His point was grasped and further developed by Louis Althusser—who remained at all times a party member. Considering the shallowness and duplicity of party leaders Maurice Thorez, Jacques Duclos, and the egregious Georges Marchais, it made sense to abstract the role of human agents from a history determined by the interplay of impersonal structures and practices. Judt's critique of Sartre and Althusser is highly recommended for anyone who still entertains the idea that they were independent spirits.

The chapter on French Marxism is one of four long essays, plus an introduction, on disparate topics: the French labour movement in the nineteenth century; the French Socialist party from 1920 to 1936; and the elections of 1981. Each essay is self-contained and thoughtful, but it requires an effort by the reader to discern any unifying theme. Judt argues that Marxism is now history in France, and that French communism is on the way out as a powerful and autonomous force. He believes that the 1981 election was a decisive turning point in the evolution of the party system, marking an irrevocable break with revolutionary posturing and illusions by the Socialists.

How could the venerable rivalry between reformists and revolutionaries be so easily transcended? Judt contends that the historical divisions within the French Left have been exaggerated. Thus, the entire French working class was receptive to Marxist notions even before Marx appeared on the scene, and was oriented towards revolutionary activity by the repressive policies of Louis Bonaparte. For Judt, Proudhon's anarchism was always a minor trend, overshadowed by faith in the state as an instrument of working class aspirations, and "wrongfooted" to boot. Communists and Socialists after 1920, he believes, shared a revolutionary orientation. The Left is now expressing its underlying unity through one party, rather than through two parties as in the past.

In my view Judt greatly underestimates the internal rift of the French Left, downplaying Proudhon and the significance of the competition between Jaurès and Guesde and now between Socialists and Communists. He is thus

led to strange or misleading judgments. Michel Rocard, he states, "is isolated today in his political family" because he is outside the political culture of the mainstream Left. Considering that Rocard is a leading Socialist candidate for the presidency, this is a curious view of "isolation." Judt makes no mention of the official conversion of both Socialist and Communist parties to autogestion in the decade of the 1970s (an indication of the force of Proudhonian tradition). He characterizes the student leadership in the May 1968 revolt as consisting of Trotskyists and "non-ideological radicals"; to say that the anarchists and surrealists who made up one wing of the student movement had no ideology is simply wrong. Judt is also in error in describing the Common Program of 1972 as a platform for social democracy; that was certainly not the intention of the PCF. And he overlooks altogether the significance of the intense debate within the PCF over Stalinism and its causes and its determination to create a "democratic socialism," as contrasted with the "authoritarian socialism" that it now considers to exist in the Soviet Union. That the PCF has not been able to extricate itself from its contradictions is true, but its efforts to do so are worth recounting and explaining.

In 1981 the Left took power, and made the inevitable discovery that a belief in utopia does not resolve pressing political problems. Socialist intellectuals have become more aware of the needs of a complex society as they strive to advance the interests of workers and the disadvantaged. The Socialist party is now a credible party of government. Tony Judt's book, for all its flaws, helps us understand that momentous development.

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Corporatism and Change: Austria, Switzerland, and the Politics of Industry. By Peter J. Katzenstein (Ithaca, NY: Cornell University Press, 1984. 331p. \$35.00).

The concept of corporatism—or "neocorporatism" or "democratic corporatism"—arouses surprisingly strong emotions among political scientists. Writers influenced by Marxism tend to view corporatism as a late capitalist mechanism intended to sublimate or

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suppress class conflict. For students of democratic theory, corporatism implies a secretive decision-making process carried out among economic and bureaucratic elites and largely excluding popular participation and sometimes even the participation of elected officials. Even among those scholars who have themselves contributed to the burgeoning literature on corporatism, commitment to widely diverging definitions of the term has often led to sharp disagreements.

Peter Katzenstein has invited the ire of all these audiences in his important comparative study of the political economies of Switzerland and Austria, and has compounded the risk by voicing nearly unabashed approval of the workings of what he terms "liberal corporatism" in Switzerland and "social corporatism" in Austria. He argues that, in both of these small Alpine states, corporatism succeeds in narrowing political inequalities through a process emphasizing consensus building and inclusion of the politically weak. The result is both economic success—even under the difficult economic conditions of the 1970s (the period covered by his study)—and an enviable level of political stability.

Katzenstein makes a persuasive case. He first presents the reader with concise but compelling overviews of the political economy of each country. He then compares the two directly and seeks to show that, in spite of their differences—the preeminent position of internationally oriented businesses and the particularly powerful role of the major private banks in Switzerland, the more activist role of parties and the state and the strong influence of the trade union movement in Austria—the two versions of corporatism bring about surprisingly similar results. These he attributes in part to their common vulnerability to pressures from the international economy.

Katzenstein undertakes to support these arguments with four case studies of industrial policy, examining the efforts of the two systems to assist and to manage the adaptation of industries especially severely affected by the rapidly changing economic conditions of the 1970s—textiles in both countries, steel in Austria, watches in Switzerland. Because the case studies focus on only one particular type of economic policy problem, they do not illustrate as broad a range of the different patterns of corporatist policy making in the two coun-

tries as one might wish. But they are carefully executed and illuminate much that is distinctive about the two systems in facing problems that have become familiar to virtually all Western states.

The critics, one suspects, will not be entirely mollified. Some will deny that Switzerland is corporatist at all (because of the weakness of Swiss labor and the lesser degree of institutionalization of corporatist structures—there is no Swiss equivalent, for example, of the Austrian Joint Commission for Wages and Prices). Others will argue that Katzenstein gives too little attention to underlying class conflict (as expressed, for example, in the growing ideological divide between the right wing of the Swiss *Freisinnige* (liberal) party and the left of the Social Democrats). To my mind, the most notable omission of the book is its failure to address except in passing the argument that the Swiss and Austrian systems, in their concern for accommodating major social forces and conciliating conflict before it becomes overt, frustrate democratic participation and marginalize political actors who fall outside the existing elite cartels. One might go so far as to argue that political conflict in Switzerland and Austria has been displaced from critical economic issues to less crucial, more emotional and symbolic arenas—youth rebellion and the Jura and foreign worker questions in Switzerland, atomic energy and leadership personality conflicts in Austria.

Nevertheless, Katzenstein had produced a finely wrought and challenging book. The argument itself is clearly and forcefully presented, and the body of evidence he has marshalled on its behalf is impressive. Not only the relatively small number of students of Swiss and Austrian politics will find this book invaluable, but also the much larger number concerned with the interplay between the international economy and domestic political institutions and policy processes. Implicitly, Katzenstein commends the neocorporatist policy-making approach to larger polities, none of which can claim any independence from the tides of the international economy and many of which have coped with them far less successfully than have Switzerland and Austria.

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Comparative Government and Politics: Essays in Honour of S. E. Finer. Edited by Dennis Kavanagh and Gillian Peele (Boulder, CO: Westview Press, 1984. viii, 242p. \$32.50).

New Directions in Comparative Politics. Edited by Howard J. Wiarda (Boulder, CO: Westview Press, 1985. xiv, 239p. \$35.00, cloth; \$16.95, paper).

The comparative politics field is undergoing a difficult period of reflection; scholars are wondering what the configuration of the discipline really is, and they are debating about what direction holds the most promise for the future. Both of these volumes mirror the soul searching.

The Kavanagh and Peele volume is a collection of essays in honor of S. E. Finer, in which each contributor was encouraged to select a topic related to Finer's work. All of the writings deal with more than one polity (though some more extensively than others), and they focus primarily on substantive findings, with methodological issues receiving incidental treatment. The Wiarda book is an outgrowth of ongoing seminar sessions at Harvard, in which the participants took stock of the field and wrestled with questions of comparative methodology.

The work in tribute to Finer reveals the broad sweep of his intellectual interests. While a good portion of the analysis is centered on political institutions, the contributors also deal with political culture, national and local elites and their popular linkages, political participation, and electoral opinion.

The topics addressed by the authors are of current interest to political scientists. The strength of British government, according to Hugh Berrington, is really a source of weakness because it cannot claim that it lacks power to give interest groups what they demand; the governing party is reluctant to use sanctions against them out of fear of retaliation at the polls or through strikes. Vernon Bogdanor describes how the monarch is protected in the government formation process in five European countries. Jean Blondel treats the problem of "dual leadership," comparing the roles of the monarch or president and the prime minister, and the party secretary and the prime minister, and their impact upon regime stability. Raising some interesting research questions, Jack Hayward analyzes the socio-

economic leverage exerted by pressure groups and the "social partners" strategy employed in France. Alan Angell traces the development and recent trends of military regimes in Latin America, detecting little uniformity (or effectiveness) in their economic policies. Donald MacRae outlines the evolution of elite studies, including their application to Marxist thought and communist thought and practice. In their chapter on popular participation, Geraint Parry and George Moyser wrestle with the definitional problem, and then draw attention to the importance of examining participation in community subcultures whose influence is lost in national samples. Research designs of linked case studies, they argue, are a prelude to theory development. In a comparison of constitutions, especially British and U.S., Gillian Peele indicates how legal norms reflect political culture and what factors are involved in the resolution of constitutional issues. In the concluding chapter, David Robertson attacks the Finer thesis that parliamentary representatives, initially harboring views divergent from those in the electorate, adopt policies that make concessions to party extremists; thus getting farther out of step with the more moderate opinions of the electorate. He contends that the measurement instruments are defective, and, drawing upon data from Britain, the United States, and New Zealand, he concludes that the voting population does not have a moderate position that is deserted by the parties.

The purpose of the Wiarda volume is to subject the comparative field to rigorous self-examination, with major emphasis on methodological approaches. Older ways of attacking problems, especially the work of the Social Science Research Council's Committee on Comparative Politics, are examined through a critical lens, and newer perspectives that are attracting support are brought into summary view, in terms both of their strengths and their weaknesses.

A listing of the essay topics will provide a glance at the divergent signposts that are being erected in the field: the struggle between the state and other social organizations (or even the market) over social control and the mobilization of mass support (Joel Migdal); corporatism (Douglas Chalmers); the "input" analysis characteristic of typologies of democratic systems from a liberal perspective and

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contrasting typologies based on political economy (Peter Lange and Hudson Meadwell); the dependency approach (Tony Smith); the rejection of Western models of development by Third World scholars, who are experimenting with alternative views of the process (Howard Wiarda); neo-Marxist emphases on class analysis (Ronald Chilcote); and comparative public policy (Lawrence Graham).

Nearly all of these essays are excellent descriptions of conceptual design and research trends. While indicating the potential utility of the various approaches, the authors do not make excessive claims. They are careful to point out deficiencies, and in some cases they present recent evidence that tends to weaken the paradigm they favor. Among the salient messages conveyed to the reader are the following: the state is a major factor in the political equation, becoming revived as an object of investigation; economic variables must be taken into account; indigenous institutions, which are more durable than earlier research designs assumed, sometimes play a useful role in the modernization process; and newly developed nations are not destined to follow the paths taken by the more advanced countries.

Striking criticism of past research efforts in the comparative vineyard, both explicitly and implicitly expressed in the two volumes, tempts one to wonder whether much has been accomplished during the last three decades. But Sidney Verba's chapter in the Wiarda book is a modulating influence. He puts the intellectual developments into a balanced perspective, his appraisal of the past includes a list of noteworthy accomplishments, and he attempts to bridge the old and the new, setting forth the needs and problems of the future.

The two books clearly illustrate how fragmented the comparative discipline has become. A host of flowers bloom, and divergent approaches contend. Intellectual ebullition, of course, can be a wholesome development, but it nevertheless signals the appearance of a critical point in the evolution of the field. (Other segments of political science similarly seem to be on a plateau, and self-criticism, with contending paradigms fighting for acceptance, will be helpful to these segments, too.)

Hans Daalder, in the Kavanagh and Peele book, displays an awareness of the crisis when

he criticizes European scholars for not designing their studies within a comparative frame, and U.S. scholars for not being sensitive enough to contextual realities. Both criticisms need to be heeded; parochial studies by themselves contribute little to generalization, and failure to take situation and context into account results in faulty generalization. At a moment, the tendency in comparative research—reflected to some extent in both volumes—is to move away from the broader statements about the political world to more modest studies of phenomena in a given country or culture sphere.

This shift in research emphasis is understandable and can be worthwhile, but Verba's call for an amalgam of case studies and broader comparisons should be kept in mind. It is easy for scholars to become so preoccupied with parochial investigations that they lose sight of the broader challenge. Comparison, it is well to remember, is more a method than a field. All political scientists should be comparativists, for they are presumably interested in *commonalities*—statements about political phenomena that are applicable beyond the particularistic: across nations, culture spheres, and types of systems. The route to this objective, of course, is difficult to travel. But if scholars of politics fail to push ahead to wider generalizations, they run the risk of stumbling into a maze of "comparative uniqueness."

As many comparative scholars have pointed out, what is needed is a set of clearly delineated research problems that can be tackled from a variety of perspectives. The research designs must include concepts whose definitions have been thoroughly hammered out so that investigators will be studying the same thing, whatever their approach. In an earlier day, the concept *development* ran into difficulty, partly because it was defined in so many ways that researchers were studying different phenomena and the results could not be made cumulative. It will be a step forward if scholars working in particular countries were to address the same questions and, where possible, standardize their categorizations so that the findings will be additive and capable of being woven into broader generalizations.

A reading of these two books suggests that progress can be made in this direction; embedded in the essays are salient research problems

that, with refinement, can be cast into comparative designs, and several of the approaches tend to converge in significant ways. Both volumes deserve the attention of comparativists who detect and are concerned about the crisis in their field.

JOHN E. TURNER

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East Germany and Detente: Building Authority after the Wall. A. James McAdams (Cambridge: Cambridge University Press, 1985. xi, 233p. \$34.50).

This is a model monograph: well written, clearly organized, informative about important events, and analytical of political processes. Although narrowly focused, its findings are applicable to general concerns. One also has the pleasure of being stimulated by disagreement with some of its assertions.

McAdams deals with a small, relatively weak, and often derided state, the German Democratic Republic (GDR). Beginning with what is widely regarded as the nadir of the GDR regime's effectiveness, the building of the Berlin Wall, he shows how the successive GDR leaderships of Walter Ulbricht and Erich Honecker strove to buttress the regime's power with authority. They had to do this while coping with problems of economic and social development (similar to those of the other East European communist states). But above all, GDR leaders have faced a difficult, changing, and nearly overwhelming external environment. More particularly, the GDR has had to make its way between the often divergent interests of the more powerful Soviet Union and Federal Republic of Germany (FRG).

Over the perspective of the quarter century since the Wall, the current status of the GDR on the European and world stages, as well as among its own population, represents a considerable success story. McAdams not only tells this story well, he explores the sources of this success and proposes reasons for it.

Between a useful introduction and a rather cursory conclusion, McAdams develops a chronological and analytical account, begin-

ning with the crisis of the Wall. There follow chapters on the GDR's post-Wall achievements, the erosion of that synthesis in the face of international change in 1967-71, the response of the new Honecker leadership to detente in 1972-78, and the GDR's management of stresses induced by the decline of detente.

Each chapter has some insightful observations on the interaction between the leadership's initiatives and the constraints on its actions. For example, McAdams places the Wall in the context of other, preferred solutions (West Berlin as a "demilitarized free city"), shows the role of economic reforms as an authority-building symbol (a point made earlier by Thomas Baylis), demonstrates how the GDR's "shell of non-relations" with Bonn collapsed after 1967 and what that meant to Soviet-GDR relations, depicts (and almost does justice to the personal drama of) Ulbricht's downfall in 1971, and shows, finally, how the current regime uses detente with Bonn to play an international role highly congruent with domestic authority building.

McAdams differentiates between tracing GDR policy back to a "legitimacy deficit," and his preferred formulation of "building authority" (see pp. 3-5). Since he does not define "authority," and authority building is forever threatened by West German flexibility, this is an uncertain distinction. McAdams makes abundantly clear how "penetrated" the GDR is by almost every move by Bonn or Moscow. Rarely has the Middle European "correlation of forces" been such as to allow East Berlin substantial leeway. The postponement, in September 1984, of Honecker's projected visit to the FRG demonstrated this very well.

Given parallels between Ulbricht's policies in 1968-71 and Honecker's in 1983-85, McAdams might have probed more deeply into Honecker's relationship to the USSR. He is surely correct in asserting (p. 151) that the key to Honecker's policy is the recognition that "socialist inequality need not mean total dependence or, perhaps more importantly, weakness."

Most of this account focuses on the international setting in which the GDR leaders pursued their goals. I would agree that this was more important than domestic forces, but it would have strengthened this book if the author had made this point explicit and sub-

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stantiated it. But this is one of the minor flaws in a very worthwhile scholarly work.

HENRY KRISCH

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Bureaucrats and Ministers in Contemporary Japanese Government. By Yung H. Park. (Berkeley: University of California Institute of East Asian Studies, 1986. xii, 192p. \$15.00).

Who really rules? The politicians? The bureaucrats? In Japan, the answer was easy for the first decade or so after World War II: the bureaucrats. They had exercised political authority before World War II. Needing them to bring about their reforms, the U.S. Occupation officials had never challenged that authority. In 1955, the Liberal Democratic party (LDP) became the majority party and formed the government. It still is the majority party. It still forms the government. Not even the opposition parties suggest that it will not be the core of future governments. In the 30 years it has been in power, the making of policy—government policy—has left the ministries and come to reside in one of the LDP organs, the Policy Affairs Research Council (PARC), composed of a gaggle of divisions, special committees, and investigative commissions. Currently, the gaggle numbers 121 committees and 144 subcommittees. Yes, it's a mess.

Scholarly writing on Japanese politics has trailed but generally reflected this change. Tokyo University professor Tsuji Kiyoaki wrote the first major postwar study of the Japanese bureaucracy. He argued that not only the bureaucracy but the junior bureaucracy exercised real political power. His book, first published in 1969 and now in its ninth printing, is still influential. Kyoto University professor Muramatsu Michio has challenged him in another powerful book on the bureaucracy, published in 1981. His thesis was that the bureaucracy could not deliver social stability and economic prosperity; these required the legislators and the parties, with the "interest groups moving in the background."

University of California at Humboldt professor Yung H. Park, the author of the monograph under review, accepts the upgrading of the politicians. Concerned with how the party

exercises its authority over the bureaucrats, Professor Park focuses on the role of the cabinet minister. In his introduction, he claims, "The cabinet minister [is] one of the most underrated institutions of the contemporary Japanese political system." And he concludes, "Any study of agency policy-making . . . that leaves out the role and place held by the agency minister or ministers will be unable to unravel the full complexities of policy-making" (p. 9).

Why has the cabinet minister been so underrated? The LDP is composed of factions. In making a cabinet, each prime minister talks of putting the right man in the right post, but each prime minister must first allocate the seats among the factions and appoint to the seats men recommended by the faction leaders. The life of a cabinet officer is short. In the 30 years of the LDP's existence, its 12 prime ministers have significantly changed their cabinets 37 times; the cabinet officer often has no time to do his thing. The making of law and policy grows increasingly complex. Each minister confronts highly skilled bureaucrats, who have clear, sometimes unshakable ideas about what is their ministry's mission and what they should be doing to fulfill that mission. Furthermore, upper bureaucrats shield lower bureaucrats, who, traditionally, have framed policy. The demands of legislature, the party, and the constituency leave a minister neither time nor energy to perform as minister effectively. These circumstances suggest that a Japanese minister is weak. Bureaucrats have primacy. This view was broadly accepted when Professor Park started to write his book.

Professor Park argues that the semipermanency of the LDP has changed these circumstances. Bureaucrats cannot remain indifferent to politics and hope to advance in their careers. The higher ranking the bureaucrat, the more partisan he becomes in his outlook. In recent years, many lower-ranking bureaucrats have left their ministries to pursue elective office. Their frequent success has politicized their former colleagues. Before becoming a minister, an LDP politician will have served with distinction for at least 12 years in posts in the party, the government, and the Diet (national assembly). Though perhaps new to the problem of the ministry, the minister is a good politician.

Chances are, though, that a minister will

have served on one of the PARC committees covering a ministry before becoming that ministry's minister. Even if he has not, he will have recourse to men in the party who are expert on that ministry's business. In fact, representing the party to the ministry and the ministry to the party will be one of the minister's principal tasks. Dealing with the special interest groups is another leg of ministerial responsibility and a source of ministerial strength. They will keep the minister informed; they will make demands. Tempering those demands will stand the minister in good stead among the officials. Satisfying those demands will stand the minister in good stead within the party. A clever minister will find a way both to temper and to satisfy. Nor will bureaucrats try to take advantage of a minister's short tenure in office because they know he will not go away when he steps down. He will take up occupancy in the PARC committee and they will have to deal with him there.

What have these developments meant to the organizational culture? They "have had a dampering effect on the career officials' sense of policy-making efficacy, making them noticeably deferential. . ." (pp. 179-80). In Professor Park's view, the politicians, not the bureaucrats, are on top.

That the bureaucrats and the politicians are contending elites in the making of policy is not a thesis unique to Japan. That this thesis represents the best way to analyze Japan has been questioned by Tokyo University professor Sato Seizaburo and LDP researcher Matsuzaki Tetsuhisa, now aide to the party president. In a recently published book, *Jimento seiken* (The Ruling Liberal Democratic Party) (Tokyo; Choukoronsha, 1986), a stunning display of political insight, they warn, "the ministries become weak to the extent that the Liberal Democratic Party becomes strong—that doesn't necessarily follow. We think it's a mistake to regard their relations as a zero-sum game" (pp. 78-79). Policy must be typed. The ministries will be stronger in reaching technical decisions. The party will be stronger in reaching political decisions. A major change between now and when the LDP was conceived is the dependence of the bureaucrats and the LDP politicians on each other.

Professor Park has worked long and hard on this monograph. In his preface, he says that

between 1978 and 1982 he conducted over 200 interviews with persons "involved in and well informed about" Japanese politics. It took him four more years to digest what he heard but the resulting manuscript is a powerful statement, sophisticated and measured in its judgments not only about the ministers and bureaucrats but about Japanese decision making. Students of comparative politics will want to read the book because Professor Park has written it so that international comparisons can be made. Students of Japanese politics must read his monograph. Professor Park has erected the mile post from which future research will be measured.

NATHANIEL B. THAYER

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La complessità della politica. By Gianfranco Pasquino (Rome and Bari: Giuseppe Laterza & Figli, 1985. xii, 228p. 25,000 Lire, paper).

This volume consists of seven essays, all previously published by Pasquino between 1983 and 1985 as journal articles, chapters in volumes, or independent monographs. The topics range widely, but the individual pieces constantly come back to the same themes. These themes are rather more broad than the title of the book actually suggests; at the very least, they carry the notion of "political complexity" to some of its logical conclusions. The major issues that, in one guise or another, are put before readers are the crisis of the welfare state (broadly defined), the adequacy of modern political institutions for coping with the problems of the late twentieth century, and, perhaps above all, the ability of the mass parties of the left to continue to perform a progressive, reformist function.

Pasquino's approach is comparative, and several essays could serve as models of how a review of literature ought to be carried out. The discussion is almost always very sharply focused on a single problem as the author explores how well various theoretical approaches (ranging from systems theory through neocorporatism and some of the most common applications of political economy) clarify the themes that have been at the center of recent debates in Italy and elsewhere.

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But while Pasquino covers a number of comparative cases, it is clear that his ultimate concern is with the reformability and adaptability of *Italian* political institutions and parties. He certainly does not underestimate the problems that have arisen since the late 1960s. Indeed, his syntheses of the difficulties facing modern democratic systems (chap. 6) and reform parties (chap. 3) are some of the most lucid I have read anywhere. But the author very strongly feels, in contrast to many of the analysts he so ably summarizes, that both politics and parties not only still matter but represent the only hope for successful resolutions of present difficulties within a genuinely democratic framework. At the same time, he acknowledges that the changes required of existing institutions will have to be extensive if there is to be any hope at all of successful reform. Unpersuaded by the conservative bias in many contemporary approaches, he nevertheless underscores the important insights of these approaches. Readers familiar with his numerous contributions to Italian political science will in fact find that these essays, taken together, not only have their own internal unity, but also illuminate why in recent years this astonishingly prolific author has felt it important to address a great number of issues in several important volumes and countless articles (on Italy's three major parties; on terrorism; on the media and politics; on voting behavior; on institutional reform; on social complexity and new social movements; and on Italy as a political system).

The book itself consists of three sections. The first two chapters primarily explore different approaches to crisis and systemic complexity within the literature of political science, both mainstream and Marxist. Here, Pasquino is mainly concerned with showing both continuity and evolution in the literature on modernization and political complexity. The next two chapters focus on the problems of a reform party and on mass media and communications, but the latter chapter keeps coming back to the impact of new modes of communication on mass parties. The final section contains three chapters that address various aspects of political systems and policies as they confront the challenges of governability and representativeness in the 1970s and 1980s. Chapters 6 and 7 also contain extended expositions and critiques of the literature, ranging

across U.S. and European contributions. Pasquino is most genuinely comparative in this section, drawing primarily on the U.S., Sweden, and Italy as cases of different states and societies confronting similar problems.

The volume suffers from the shortcomings of a collection of essays produced for a variety of other sources. Ironically, the strength of the review of the literature in some of the individual essays erodes the impact of the book as a whole. The very succinct treatment of a single author or body of work, or the abrupt shift from spelling out a problem to discussing how it is viewed in the literature, is far more suited to an article than to a longer treatment. When the reader encounters the same author, or title, on a variety of occasions in the course of the collection, he begins to wish for a bit more depth (or for a more integrated treatment). Such repetition is by no means a crippling flaw, and it does serve to underscore what the author considers most important. Yet it does weaken the impact of this particular book. Those who want more depth from Pasquino on many of the specific issues raised here will have to refer to his abundant other works. Still, the ability to sort out and make clear the central propositions concerning social complexity, fragmentation, and "governability" found in a variety of seemingly unrelated writings and writers is, as already noted, a model of synthesis. And Pasquino's discussion of both the past performance and problematic future of mass parties is as solid and insightful as one is likely to find anywhere, in any language.

STEPHEN HELLMAN

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Public Employment in Western Nations.
Edited by Richard Rose (Cambridge: Cambridge University Press, 1985. xi, 265p. \$39.50).

This volume is one of several that have recently been produced by the Center for the Study of Public Policy at the University of Strathclyde from its ongoing research into the growth of government. Its purpose is to document the extent of public sector employment in Britain, France, West Germany, Italy, and the United States since 1950. Therefore, the core of

the volume consists of 100 tables that trace the growth, nature, pay, qualifications, and functions of public employees as well as the number of people primarily dependent on government incomes in each of these nations. In large measure, the text is simply a running commentary on these tables.

In statistical terms, the book is a prodigious accomplishment. The data have been gathered carefully so as to be comparable across nations. Country specialists have canvassed a wide variety of sources for each chapter. The material is well presented, and this volume is bound to become an indispensable reference work for anyone interested in the development of the public sector. In this regard, the Italian chapter is especially impressive. Moreover, there are some startling pieces of information to be found here: in virtually every European nation, the primary income of over half the population comes from the government, and since 1951 rising public employment has accounted for almost all the net growth in employment on that continent. These facts alone are sufficient testimony to the importance of this volume.

The analytic contribution of the volume, however, rests on the categories into which the authors have decided to group public employees and their products. In line with what they term a "programme approach" to public employment, Richard Rose and his collaborators divide the activities of public employees according to whether they contribute to social programs, economic programs, or the defining concerns of government (primarily tax collection, defense, and policing or judicial functions). Their results are then said to be either "collective goods," "marketed goods," or potentially marketable goods that are "given away." Perhaps unintentionally, the effect of this system of categorization is to impress on us the extent to which contemporary governments have moved away from their "defining" functions by virtue of a great increase in the number of potentially marketable goods that are "given away." A large increase in the number of educators and health care providers over the past three decades accounts for much of this trend because they are defined as public employees in almost all of these nations.

The relative increase in health and educational personnel is a striking finding and these

categories have much to be said for them, but a few quibbles might be in order. Should doctors and other health workers paid indirectly through health insurance schemes of the sort found in France and Germany, for instance, be labelled "public employees"? Most of the funds that pay them are channelled through the state, but I suspect that many European physicians might be surprised to see themselves described this way. In this volume the term *public employee* includes people who stand in such a wide variety of relationships to the central state that it loses some of its meaning along the way. Similarly, there are those who might be inclined to consider education to be as much a "defining concern" of the modern state as defense; and there are those who (stressing its social benefits) might see education as a collective good rather than as a potentially marketable good now given away. After all, as some U.S. communities have demonstrated, even many policing functions can effectively be marketed. In short, there is room to argue with some of the broad categories into which this volume divides public programs.

Nevertheless, country specialists as well as comparativists will find a wealth of information in the chapters on Britain (by Richard Parry), France (by Edward Page), Germany (by Klaus-Dieter Schmidt and Richard Rose), Italy (by Andrea Cendali Pignatelli), and Sweden and the United States (by B. Guy Peters), as well as stimulation in the thought-provoking introduction by Richard Rose. This is an especially powerful salvo in the intensifying debate on the growth of contemporary governments.

PETER A. HALL

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Weapons of the Weak: Everyday Forms of Peasant Resistance. By James C. Scott (New Haven and London: Yale University Press, 1985. xv, 389p. \$30.00).

The Muda River valley in the northeastern state of Kedah is the largest rice-producing area in Malaysia. In the mid-1960s, the government built a large irrigation system that per-

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mitted double cropping. Combined with the new green revolution technologies, this project more than doubled rice yields within a few years. While most of the farmers were small owner-operators, a significant minority were tenants or landless workers and a few owned substantial holdings. In this familiar Asian scenario, the latter constituted a local power elite because of their wealth, their control of employment, and their links with government and the dominant Malay political party.

At first the tenants and landless workers shared in the green revolution bonanza because double cropping expanded the demand for labor. But in a few years the larger landowners introduced combine harvesters and new methods of seeding, both of which substantially reduced labor requirements with devastating effects on the livelihoods of tenants and landless workers. The specific context of Scott's study is how the marginalized peasants responded to the imposition of new technologies and the commercialization of the rural economy, which threatened both their livelihoods and their status as productive members of the village community. The larger issue is a replay of the dilemma that has confounded Marxist social scientists—Scott employs the main class-based concepts of academic Marxism—why the victims of exploitation so seldom develop a revolutionary class consciousness and why they so seldom undertake revolutionary activity.

To examine these problems from the peasants' perspective, Scott employed the methods of the participant observer. Living and working for two years in a Malay village, he faithfully recorded daily interviews and encounters with members of all classes, kinship groups, and political factions in this village society. The result is a body of authentic data about peasant responses to material opportunities, threats, and deprivations, as well as the economic and social strategies they employed to defend their interests and rationalize their behavior—stripped of the deductive inferences so common in academic treatment of the world view and motivations of disadvantaged peoples. It is not only the sensitive reporting of this rich array of data but also their sophisticated interpretation by a scholar who has mastered the literature on peasant protest and resistance that makes this book a remarkable intellectual achievement.

Among Scott's major findings are these six:

First, a continuous class struggle between owners of the means of production and those who provide only labor is waged over relative contributions to and shares in the production process, including wages, terms of tenancy, and even charitable obligations.

Second, this everyday struggle occurs simultaneously at two levels: the economic, in which the terms of material exchanges are worked out and the symbolic or ideological, where members of each class attempt to rationalize and legitimate their particular interests in terms of the social and religious norms that bind the community.

Third, while superior economic and political power confer overwhelming advantages on those who own land and capital, tenants and workers employ unremittingly the "weapons of the weak" to maximize their limited resources and bargaining power. These weapons range from slowdowns and informal labor boycotts to pilfering of rice and animals, careless threshing (so that more rice will be available for gleaners), and maligning by gossip the reputation of stingy and callous landlords. Occasionally, the resistance spills over to acts of violence, including the sabotage of machinery. With few exceptions, however, these tactics are carried out without formal organization and they avoid direct confrontation; indeed, on the surface they remain polite and deferential.

Fourth, contrary to the views of Gramsci and his numerous academic disciples, the rural poor do not suffer from "false consciousness" and are not "mystified" by the hegemonic ideologies promoted by the dominant class and its intellectual and political agents. The peasants are perfectly aware of what is happening to them, but they also recognize the limits of their ability to resist openly without jeopardizing the precarious livelihoods that are available to them and without inviting the repressive powers of local elites and the state. These peasants behave quite rationally in terms of their limited political, economic, and symbolic resources and opportunities.

Fifth, though sanctioned by religion and custom—in this case the Malay version of Islam—the solidarities of village life expressed in patron-client networks and in the obligations of the rich to ensure the subsistence of the poor were products primarily of economic

interdependency. Landed farmers needed an assured supply of reliable tenants and workers. The new technologies substantially reduce the demand for labor. As the poor become redundant and expendable in the processes of rural production, the moral economy of village life changes. Landowners abandon—and find ways to rationalize the abandonment of—their traditional obligations to tenants and workers.

Sixth, the state increasingly penetrates the rural economy, but the services it provides disproportionately benefit the rural elites directly and by preemption. Moreover, its instruments of repression unfailingly protect the status quo and deter organized protest by the disadvantaged. In Malaysia the state has become the main provider of valuable public goods but has not succeeded in replacing the rural elites as patron at the village level. Many of the material benefits distributed by the state are mediated by the official party machine, which is controlled locally by landowning families.

Like most contemporary academic Marxists, Scott is explicitly pessimistic about the future of the marginalized peasantry under collectivized as well as capitalist regimes. In states that call themselves socialist, the peasants remain exploited and powerless. The class struggle over the remuneration of labor and shares of the harvest continues. Peasants resist the exactions of the socialist state by many of the same weapons that Scott identifies among their counterparts in the Muda River valley. In capitalist economies such as Malaysia, technology in the hands of a landowning class and their new commercial allies continues relentlessly to displace and marginalize tenants and laborers. At the village level they are fighting rear-guard actions with the ineffectual weapons of the weak while the growing power of the state inhibits and where necessary crushes any revolutionary responses. Contemporary academic Marxism provides increasingly sophisticated critiques of the "contradictions" of advanced capitalism but few plausible prescriptions on how to emerge from these dilemmas. The prevailing pessimism of the non-Leninist Left, as exemplified by Scott, offers even less hope for the victims of unrestrained capitalist expansion than the reformist formulas and the standard prescriptions for capitalist growth that Marxists continue to denigrate. Ironically, academic Marxism has achieved the peak of its influence

among social scientists at the very time that its confidence in revolutionary action and in practical socialist alternatives has virtually collapsed.

The great strength of this study is its rich village-centered perspective on the unequal struggle over shares, material and social, in the wake of capitalist development. Its treatment of the state and its impact on local society, however, is fragmentary and incomplete. Though committed to capitalist development, the Malaysian state has implemented a growth-oriented economic strategy from which the majority of Malays, including many from modest village backgrounds, have benefitted in a relatively short period of time. It has retained enough democratic practices that it must be sensitive to its Malay constituency on whose votes the present ruling coalition depends (there is an active populist Malay opposition party whose role in Kedah's village life is described at some length in this study). To legitimate its rule, the present regime has taken a number of steps to improve the quality of rural life. These include the rapid expansion of such public goods as educational and health facilities, water supply and electricity. Rapid economic growth has provided alternative opportunities for livelihoods outside agriculture. The regime has guaranteed the political and cultural hegemony of the Malays, a not inconsiderable achievement of which village Malays are clearly aware. Though its machine-style rule is often heavy-handed and though benefits have been distributed unequally along class lines, the majority of rural Malays, and not only the most affluent among them, have benefitted, as Scott's data indicate. This is not to deny that the state could and should have done more—as reform-minded observers have urged—to mitigate the pace and the social costs of this rapid structural transformation.

The ability of a relatively successful capitalist state, through economic growth and the provision of public services, to distribute benefits and the prospects of benefits rather widely may help to explain the stamina of this pattern of political economy even in the face of destabilizing technological changes that produce a substantial number of losers and victims. Apart from its repressive apparatus, the relative stability of the Malaysian capitalist state to date can be explained at least in part by the material and symbolic benefits that it has

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generated for the majority of its core Malay constituents. This is the dimension of empirical reality and especially of theory that Scott underplays in this otherwise excellent study.

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The European Experience: A Historical Critique of Development Theory. By Dieter Senghaas (Dover, NH: Berg Publishers, 1985. 280p. \$26.95, cloth; \$10.50, paper).

This book describes the development experience of smaller European countries to see what light they throw on development policy throughout the world today. Countries are divided between those that are "autocentric" and those that are "peripheralized," following the Braudel-Wallerstein vocabulary, but blaming "peripheralization" on the policies of the countries affected rather than on hegemonic countries at the center. Small countries that are "associative" in the sense of producing raw materials and food for the world market are likely to find themselves in an economic backwater, although England, Belgium, Switzerland, and the Netherlands developed in that fashion, and an associative posture is satisfactory for large states. "Disassociative" development based on the dynamics of the domestic market has worked for France, Germany, and Austria-Hungary, for such state-capitalist economies as czarist Russia and Japan, and state-socialist economies as the Soviet Union, China, and North Korea. Socialist countries face the same choice as others as to whether to associate with or dissociate from other socialist countries.

What Senghaas holds to be the desirable model for developing countries is a combination of association and disassociation—export-led growth in specialized niches while keeping certain domestic markets protected, as exemplified in his judgement by Denmark, Sweden, Norway, Finland, Australia, Canada, and New Zealand. He regards free trade as a special case and views his position as neo-Listian, derived from Friedrich List's prescription for a national economy.

Senghaas's recommendations go beyond association and disassociation with the world economy. He favors especially improving pro-

ductivity in agriculture through defeudalization before setting out to build industry, wants a sharing of income and wealth as a basis for ensuring that increases in productivity spill over into consumption of domestically produced goods for the masses, not just conspicuous consumption that turns to luxury imports. He calls on developing countries to invest and innovate their own technologies and to build broadly effective infrastructures. The developing countries should stay self-reliant and decoupled as far as the Organization for Economic Cooperation and Development (OECD) world is concerned but associate collectively among their kind within regions and sub-regionally. These precepts, it is noted, do not apply to the successful newly industrializing countries (NICs) of the Pacific basin, but Senghaas is not discussing that experience.

The heart of the book is a historical account of the development experience of Scandinavia and the dominions (Australia, Canada, and New Zealand), the last presumably honorary parts of Europe. Denmark is compared with Uruguay on the (dubious) assumption that the resources of the two countries are similar, and some attention is given to the familiar Argentine-Australian comparison. On the other hand, too little attention is paid to France and Germany in Europe, the experience of which runs counter to his generalizations about an agricultural revolution and redistribution of income and wealth, or to the "semi-peripheries"—Italy and Spain. The European country that gets the most attention is Finland, stimulated in growth through paying reparations in industrial goods to the Soviet Union. Senghaas argues that the case is not exceptional but representative. The Norwegian legislation of 1905–10 limiting foreign ownership of natural resources is given critical significance as a disassociative act. Disassociative measures are called "clever" (p. 61) and "well-planned" (p. 100), with, however, little in the way of supportive evidence. I am inclined to argue that Scandinavia is not representative of European experience, with its initially weak feudalism and widespread Protestant education, plus the adventitious British repeal of the Corn Laws, the timber duties, and the navigation acts. While the Sandberg thesis of Sweden as an "impoverished sophisticate" is mentioned, no attention is paid to the financial institutions that constituted the core of Sandberg's thesis.

To those immersed in the Anglo-Saxon literature, the book is of great interest in calling attention in the footnotes to a lively and abundant literature on problems of economic growth, especially by Ulrich Menzel, who has worked with Senghaas as well as on his own, by Andreas Buro, Silvio Horner, the Swiss economist, and others.

The book, however, is hard going. Either Senghaas or his translator has a weakness for making up words. *Autocentric* and *peripheralization* are bad enough. There are, in addition: *dynamification*, *horizontalization*, *hierarchization* (and *de-hierarchization*), *hypertrophically*, and the like. Mouthfilling sentences of German structure abound. There are many meaningless but good words: *coherent*, *intermeshed*, *homogeneous*, *well-proportioned*, *balanced*, *broadly effective*, and the like, as well as their bad opposites: *heterogenous*, *disproportionate*, *stagnative*, *stunted*, *top-heavy*, *crippled*, *lame*. Italics protrude from practically every page to give a breathless quality to the prose, and the number of exclamation points exceeds the narrow limits I allow. Repetitive lists of conditions necessary for growth lead one to infer that economic growth is overdetermined with more equations than there are unknowns. The author uses *free-trade imperialism* and *staple theory* in ways that differ from normal practice among economic historians. There is no index.

Having said so much, I must admit that the book, while incoherent and badly proportioned, has some interesting ideas and refers to some interesting literature. It is especially refreshing that developing countries are not asked to metamorphize themselves into little Japans.

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Three Civilizations, Two Cultures, One State: Canada's Political Traditions. By Douglas V. Verney (Durham: Duke University Press, 1986. 454p. \$35.00).

This is a fascinating study of a number of what are clearly the most significant issues in Canadian politics in modern times. Verney investigates the manner in which Canada's current political culture and contemporary politi-

cal institutions have been influenced by three different civilizations and seeks to explain not only how a number of uniquely "Canadian" practices and institutions have developed but as well what the implications of these practices and institutions will be for the future of Canadian politics.

Verney begins by drawing a rather unusual and important distinction between civilization and culture. Canada and the United States share a North American civilization; they do not share a culture. Canada has been "in the front line in the conflict between civilization and culture," he asserts, and each of three successive civilizations—the French, the British, and the U.S.—has "left a different residue" (p. 11) that has contributed to Canada's culture. Verney demonstrates how Canada has retained some cultural aspects of each civilization it has experienced, but yet avoided being absorbed by each of these cultures, seeking instead to develop its own culture.

There is a great deal of material here. Verney examines Canada's "philosophical federalism," and shows how Canadians were both drawn toward and fought against U.S. "scientific empiricism." He traces the manner in which Canadians, while attracted to British "Westminster" political institutions, "reconciled" (modified) these political institutions and practices such as parliamentary supremacy so that they would work within the Canadian federal political arena. He discusses the differences between Canadian and U.S. applications of the federal concept and describes the evolution and development of the concept of judicial review in the Canadian political arena. As well, he treats the French political heritage of Quebec with an eye to understanding how "dualism" responded to the French Canadian perception of "compact theory" and the consequent need for biculturalism: dualism was "the very basis on which French Canadians had consented to Confederation" (p. 218).

In its broader view, this book is concerned with the types of political structures appropriate for Canada today and in the future. There is much discussion of the federal model of government here and of how Canadian federalism might be modified in the future. Verney concludes that Canada's federalism today might more appropriately be described as "pseudofederalism." He argues that what is needed is "the replacement of pseudofederalism by a truly constitutional federalism with

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institutions appropriate for a balanced constitution" (p. 392).

This is an interesting and well-written book, which draws upon a number of different bodies of literature and should, accordingly, be of interest to several different groups of scholars. Verney's discussion of the Canadian response to "scientific empiricism" includes not only a discussion of the manner in which many U.S. intellectual predispositions differed from comparable Canadian attitudes but includes a well-written discussion of emerging Canadian political theory in its own right. His explanation of some of the implications for Canadian federalism of the British principle of parliamentary supremacy gives the reader a real understanding of both systems, as well as an appreciation for some of the more subtle problems involved in the "marriage" of different political systems. His analysis of "cultural dualism" and the French Canadian perspective on contemporary political issues does an excellent job of pulling together the many major themes of this body of literature, and clearly articulates the French Canadian position in federal-constitutional debates.

It must be noted that in one sense much of what is here is not new material. There are already substantial bodies of literature dealing with the issues of cultural dualism, biculturalism, assorted theories of Canadian federalism, the threat of U.S. cultural domination, and the like. What this book offers that is different from these other studies is the manner in which Verney synthesizes the material he presents. He brings together a number of different bodies of literature in a way that is systematic and theoretically meaningful. The material is impressively documented, and the book includes both a substantial bibliography and an index that appears to be very well done.

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Welfare Politics in Mexico: Papering over the Cracks. By Peter Ward (London: Allen & Unwin, 1986. ix, 152p. \$24.95).

In what ways and why does the Mexican state provide social welfare to low-income groups? Peter Ward argues, as the subtitle of his book suggests, that welfare allocations are

designed to cover up underlying systemic failures. The state, in essence, addresses poor people's needs for its own institutional reasons. Mexican governments have no intrinsic commitment to the country's poor, even though the governing class is elected to office by a populace that, in its great majority, is impoverished.

Ward focuses empirically on the providing of land, urban services, and health care to Mexico City's poor since 1970. He bases his analysis on semistructured interviews with top officers in relevant state agencies and on a household survey of some 600 families in six low-income settlements. His methodology reflects his analytical approach. He seeks to understand the politics of social welfare by focusing not merely on the poor but also on citywide and national institutional dynamics. He rightly argues that grassroots welfare provision must be understood in the context of macro political and economic forces.

Analytically, he claims that welfare state policies are not the outcome of pluralistic interest group bargaining. Nor are they a by-product of who runs the state apparatus, as instrumentalists argue. Rather, he supports Poulantzas's view that the state often acts against the short- and medium-term interests of dominant groups to ensure that social stability be maintained and to guarantee the reproduction of the labor force. Accordingly, social welfare allocations are costs that the state bears in order to serve the general interests of capital, and he rightly shows how politically managed such allocations are.

In conjunction with political management, Ward argues that bureaucracies can be dominated by one of two rationalities: rational-technical or partisan-political. He claims that decision making in general and social service policy making in particular have been "technocratized" since 1970. Agencies are increasingly staffed by technocrats. Emphasis is placed on planning that makes use of technical-rational considerations, and technical-rational bureaucracies are characterized by autonomy, stability, accountability, and objectivity. By implication (though it is never explicitly stated), "technocratization" has evolved because it serves the state's concern with the long-term interests of capital accumulation.

Although his political analysis of social wel-

fare allocations at the local level is interesting, his macro argument remains unconvincing. For one, his antiinstrumentalist thesis implies that the technocratic background of bureaucrats is no evidence that state office is used for technical-rational ends. According to structuralists, bureaucrats serve state interests, irrespective of their socioeconomic background. Second, he presents extensive data to show how politics *interferes* with the effective *implementation* of technical-rational plans and how planning initiatives are quashed. Although elite and mass support for the state is waning, it actually is the *persistence* of political bureaucracy that explains why Mexico has thus far escaped the food riots, mass demonstrations, and civil strife plaguing so many of the country's southern neighbors. Third, Ward focuses on the period in Mexican history when the state's ability to address accumulation concerns have been at a record post-World War II low. Mexico, as is well known, is suffering its worst fiscal crisis since the consolidation of the revolution in the 1930s. The reduction of budgetary allocations

in recent years to low-income groups and the recent allocation of welfare benefits (in particular, the legalization of squatter land claims) in a manner that increases governmental revenue through the concomitant imposition of property taxes speak to the short- and medium-, not the long-term, class interests in behalf of which the state rules. There is no evidence—given the country's enormous public debt, the contraction of the economy, and the extensive capital flight—that whatever technocratic reforms have been implemented serve the long-term interests of capital. What needs to be explained is the *failure* of the technocratic model and the state's role in promoting structures that have undermined capital's as well as poor people's interests for the foreseeable future. Ward has obviously bought the ideology and world view of the technocrats he interviewed; a "structuralist" should do better than that.

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INTERNATIONAL RELATIONS

OPEC: The Falling Giant. By Mohammed E. Ahrari (Lexington: University of Kentucky Press, 1986. 256p. \$25.00).

OPEC and the Third World: The Politics of Aid. By Shireen Hunter (Bloomington: Indiana University Press, 1984. xvi, 320p. \$27.50).

Mohammed Ahrari's *OPEC: The Falling Giant* is a useful and timely study of the rise and demise of the Organization of Petroleum Exporting Countries (OPEC) as the dominant actor in the international oil market. Based on the assumption that OPEC's actions can be understood largely through the analysis of underlying market conditions, Ahrari's book examines both the events that led to the 1973 oil embargo and the subsequent disarray within OPEC that produced the decline in the price of oil after 1981. Ahrari's analysis is based largely on published sources; conse-

quently, little new material is presented in this book. The main value of the book lies in its careful, exhaustive analysis, especially of the post-1973 demise of OPEC. Unfortunately, the book was completed before the recent collapse of the oil market, rendering it somewhat out of date. A large number of statistical tables are included in the book, making it a useful reference source.

One point of criticism that can be leveled at Ahrari's study is that his insistence on the primacy of market forces rather than political preferences as determinants of OPEC's behavior prevents him from examining how economic and noneconomic factors have interacted in OPEC policy making. An exclusive focus on market forces cannot explain crucial issues such as the timing of the 1973 oil embargo, OPEC's efforts to use its oil wealth to influence the North-South debate, and the failure of OPEC members to agree on common

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policies regarding price and production levels. Market forces have certainly affected OPEC decision making on these issues. However, within the constraints imposed by market forces, considerable leeway has always existed for OPEC members to pursue nonmarket preferences in areas such as security and ideology. Ahrari avoids delving into the complex tangle of political factors bearing on OPEC policy making, stressing instead the role of market forces. By adopting an overly economicistic approach, Ahrari fails to explain fully the complexity of OPEC policy making and misses an excellent opportunity to untangle the complicated relationship between politics and economics in the making of foreign policy.

Shireen Hunter's *OPEC and the Third World: The Politics of Aid* is a careful and exhaustive study of the various foreign assistance programs undertaken by OPEC members since the early 1970s. Because OPEC's aid giving has declined considerably since the price of oil began to fall in the early 1980s, and because she has compiled a wealth of data on the subject, Hunter is able to present a comprehensive assessment of the strengths and weaknesses of these programs. Moreover, the broad conception of foreign aid adopted by Hunter enables her to examine the general policy of the OPEC countries toward non-oil-producing Third World countries and toward the North-South debate. Hunter's book is therefore a study both of OPEC aid giving and of OPEC's attempts to influence the Third World and North-South relations.

Much of Hunter's book consists of extensive descriptions of the motivations, instruments, and specific content of the various OPEC aid programs. Since most OPEC aid giving has been bilateral rather than multilateral, much of the book therefore consists of country-specific case studies. Especially valuable are studies of foreign aid giving by Iran and Saudi Arabia, the two largest OPEC donors. Hunter's main conclusion is that OPEC aid giving has not substantially advanced Third World interests, either at the national or at the international level. Economic development in the recipient countries has not been greatly affected, and the hope that oil might prove useful in restructuring the international economic order has not materialized. Hunter argues that the ineffectiveness of OPEC aid giving has been due to many factors, including the heterogeneity and

conflicts of OPEC's members, the institutional character of OPEC, the divergence of interests between OPEC and non-oil-producing Third World countries, and the international political and economic dependencies of certain OPEC countries. OPEC aid giving has therefore shown many similarities to aid giving by the developed countries. Inasmuch as OPEC's price increases have adversely affected many Third World countries, Hunter concludes that the overall effect of the OPEC price revolution has not been positive for the Third World.

The wealth of data that Hunter presents tends to overwhelm the reader, although it also makes the book a useful reference source. Hunter's data are too disaggregated in certain areas, making it difficult to relate them to the broad conclusions presented in the final chapter. Summary tables showing the distribution of OPEC aid in all its forms to individual recipient countries would have been useful in demonstrating the degree to which OPEC aid has been concentrated among a few recipients. These criticisms do not, however, detract from the overall quality of the study.

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African Crisis Areas and U.S. Foreign Policy.

Edited by Gerald J. Bender, James S. Coleman, and Richard L. Sklar (Berkeley: University of California Press, 1985. xiv, 373p. \$40.00, cloth; \$9.95, paper).

In attempting to reconcile scholarship and policy, this volume seeks to present "the so-called Africanist case" with respect to U.S. foreign policy toward Africa (p. xiii). This "Africanist" perception emerges most clearly in a sustained criticism of the U.S. tendency to perceive African issues from the perspective of an all-encompassing East-West conflict—what is described as a "globalist" orientation. Contributors on South Africa, Angola, Ethiopia, Zaire, the Western Sahara, and Chad converge in their criticism of the U.S. tendency to view Africa through the prism of U.S.-Soviet relations. In doing so, they underline a valid point not accepted in current administration circles in the United States. Rather, these authors argue, U.S. and African interests will best be served by emphasizing the regional sources of

African problems (i.e., a "regionalist" view). Such an orientation provides us with a basis for assessing the costs of present policy directions; however, as this argument is set forth, it provides only limited insight into the process of U.S. decision making and into the fine gradations of choice open to U.S. policy-makers under current circumstances.

African Crisis Areas and U.S. Foreign Policy is a useful sourcebook containing some well-researched and perceptive essays on contemporary U.S. policy toward Africa. Nonetheless it leaves a number of critical issues unresolved. For one thing, the authors do not treat the relationship between globalist and regionalist perceptions in a systematic manner, leaving unclear the circumstances under which globalism might be legitimate and/or complementary with regionalism. As Colin Legum asserts in this volume, a global approach is indispensable to the task of coping with the great economic problems besetting Africa (p. 283). If Africa requires extensive external support in terms of debt relief, appropriate economic aid and investment, and technology transfer, the problem of economic development cannot be limited to an exclusively regionalist view but requires a balanced perspective. To quote David Abernethy in an excellent concluding chapter, prudent policy entails reducing "the tension between American global and regional goals in a manner most consistent with what one defines as the interests of Africans" (p. 333). A stance reconciling constructive global concerns and regional self-determination must be found—and soon—or the interests of the United States and the African countries will be adversely affected by rising hunger, poverty, frustration and instability.

The book's focus upon African "crisis" areas opens up, as the editors tell us, a broad range of "comparative concerns" (p. 1). This raises the readers' expectations to high levels. However, two interconnected aspects soon become apparent. The crises themselves differ noticeably in terms of time horizons, the nature of the protagonists, and the intensity of conflict. And these crises are not compared in their origins, domestic and external political actors, processes of escalation, efforts at crisis management, and so forth. Perhaps the explanation for this lies with the inappropriateness of the term "crisis" itself. Are the time horizons of the sharply escalating Ethiopian-

Somali encounter really comparable with the long festering struggle that besets the authoritarian and corrupt regime of Zaire? What are the thresholds between tension and crisis (p. 1), and can the term be applied to economic deterioration as well as to political/military encounters involving external as well as internal actors? (pp. 1, 323). These methodological concerns cannot be avoided if the book is to achieve its central objectives.

Other matters seem pertinent as well: the impact of conflicts within the U.S. bureaucracy upon policy formation, the significance of various U.S. interest group pressures, the contacts between U.S. nongovernmental organizations and African governments, and the role of other nonglobal actors in shaping the relations between the United States and the African countries. Certainly, this volume is a useful addition to our current materials on U.S.-African relations. I hope that it will encourage others to continue research on these subjects, in terms of generalizable and theoretical issues and in terms of more specific policy questions.

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The Reagan Defense Program: An Interim Assessment. Edited by Stephen J. Cimbala (Wilmington, DE: Scholarly Resources, 1986. xx, 215p. \$35.00, cloth; \$11.95, paper).

Strategic Nuclear War: What the Superpowers Target and Why. By William C. Martel and Paul L. Savage (New York: Greenwood Press, 1986. xx, 249p. \$35.00)

The Future of Nuclear Deterrence. By George H. Quester (Lexington, MA: Lexington Books, 1986. xiv, 333p. \$24.95).

Quester's excellent book demonstrates the value of systematic and sustained thinking, studying, and writing about nuclear deterrence. A collection of previously published or circulated thoughts on nuclear deterrence, this volume does not avoid examining difficult questions for which there are no easy answers. Some of the specific issues Quester analyzes are the causes of war and how these are tied to defense program decisions; extended deterrence; nuclear winter; Soviet military doctrine;

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substituting conventional for nuclear weapons; strengthening the defense; the future of the U.S. commitment to NATO; and trends in arms control. Analysts on the far left and the far right of the political spectrum will be unhappy with Quester's approach. He concludes that the fundamental tenets of nuclear deterrence remain unchanged, and he warns against radical new alternatives such as the Strategic Defense Initiative (SDI), deep cuts in strategic offensive forces, the adoption of a no-first-use policy, or the development of a nuclear war-fighting posture. Put succinctly, "the world must try to overcome its moral and practical problems on nuclear deterrence, for the simplest of arguments: that there is currently nothing better" (p. 119).

Attacks on the technical underpinnings of deterrence began in the early 1970s. The development and deployment of multiple independently targeted reentry vehicles (MIRVs) and the dramatic improvement in missile accuracies have eroded strategic stability because they endanger the survivability of retaliatory forces on both sides. Quester points to the dramatic improvement in missile accuracies, in particular, as leading to a renewed interest in the development of counterforce options and to a reassessment of U.S. targeting strategy, as confirmed in such documents as Presidential Directive 59. The programmatic implications of Quester's analysis are clear. Both sides should move away from MIRVs and emphasize the survivability of their missiles, rather than increased accuracy. Defense programs should concentrate on improving permissive action links (PALs), command and control of nuclear systems, reconnaissance satellites, and the security of second-strike retaliatory forces.

Martel and Savage examine the consequences when deterrence fails. In a logical and systematic fashion, they analyze the results of U.S. and Soviet counterforce attacks on their respective ICBM fields; bomber bases; submarine bases; submarines on patrol; and command, control, and communication centers. They also detail the effects of U.S. and Soviet countervalue nuclear attacks against each other's urban-industrial centers. Martel and Savage conclude that the U.S. lacks a hard-target kill capability—defined in terms of the number of available Soviet hard targets—and that the United States should reorient its plan-

ning and the configuration of its strategic nuclear forces to match Soviet counterforce capabilities.

The underlying assumption of the Martel and Savage book is that U.S. and Soviet nuclear strategies have converged around counterforce options and preemption. Like Quester, Martel and Savage attribute this convergence to technological developments that have created new targeting options. But, whereas Quester sees these trends as destabilizing, Martel and Savage argue the United States should restrict the Strategic Integrated Operational Plan (SIOP) to strategic counterforce targets and reconfigure its forces to make counterforce a realistic option. For Quester, ensuring the survivability of the retaliatory forces on both sides strengthens nuclear deterrence. For Martel and Savage, in contrast, matching Soviet counterforce capabilities achieves the same result. Thus, the programmatic implications of Martel and Savage's analysis are quite different from Quester's. They argue the United States should exchange the triad for a dyad of submarine-launched ballistic missiles (SLBMs) and bombers, concentrate its hard-target kill capability on the least vulnerable leg of the dyad—the SLBM force—and construct deep underground command posts.

Martel and Savage have done a commendable job of laying out the results of all-out strategic nuclear attacks by the Soviet Union and the United States. However, important questions about how the United States and the Soviet Union might fight a strategic nuclear war remain unanswered. For example, the authors do not address the critical issue of how a nuclear war might begin. Would a conventional war in Europe escalate to the use of tactical and theater nuclear weapons, and ultimately to a strategic nuclear exchange? Would surrogates draw the United States and the Soviet Union into a conflict in the Middle East or Persian Gulf? Would the United States be the target of a "bolt-out-of-the-blue" attack? Would the United States, under a not too incredible scenario, be the first to use nuclear weapons? How nuclear war begins is a critical question because it influences the kind of attack (counterforce or countervalue) contemplated by both sides, the scope of the attack, and the process of target selection.

Not all nuclear war scenarios are equally

likely, yet Martel and Savage treat their four cases (U.S. counterforce, Soviet counterforce, U.S. countervalue, Soviet countervalue) with equal detachment. In fact, the two most likely and perhaps most interesting scenarios (a Soviet countervalue attack on the United States after a U.S. counterforce attack on the Soviet Union; a U.S. countervalue attack on the Soviet Union after a Soviet counterforce attack on the United States) are given short shrift—each is accorded only four pages in the approximately 50 pages devoted to the countervalue cases.

There are some other problems, as well, that illustrate the authors' neglect of the broader military and political dimensions of strategic targeting decisions. For instance, there is not a single mention of British, French, or Chinese nuclear forces and how these might complicate Soviet planning and targeting. The role of Pershing 2 and ground-launched cruise missiles (GLCMs) for the West is ignored; for the Soviets, on the other hand, the backfire bomber is assumed to operate only in a strategic role. Second, the authors assume that the United States and the Soviet Union would launch counterforce attacks against the same prioritization of targets, and they ignore conventional targets entirely, which could be of special importance to the U.S. Third, some of the remedies proposed (e.g., move all U.S. ICBMs and bomber bases to Maine, New Hampshire, Vermont, and New York) ignore political, economic, and social realities.

The title of Cimbala's edited collection is misleading, because only four of the eight chapters address the Reagan defense program *per se*. Oliver and Nathan provide a good overview of the administration's first term, and address whether the Reagan defense program represents change or continuity at a higher cost compared with the Carter program. They conclude that the Reagan program represents a change in declaratory policy but continuity in operational policy. Lawrence Korb contributes an excellent analysis of the Reagan defense manpower programs—perhaps the administration's clearest success. Ballistic missile defense and President Reagan's SDI are covered admirably by Don Snow. Cimbala's own chapter on strategic offensive modernization is the weakest of the group.

The other four chapters, two of which are excellent, review various aspects of U.S.

national security policy during the Carter and Reagan administrations. Vince Davis offers a fascinating account of the defense decision-making organization under President Reagan and suggests that changes in management style influenced the substance of the administration's defense program. Alan Sabrosky considers how U.S. presidents have dealt with the war powers resolution. He concludes that Presidents Nixon, Ford, Carter, and Reagan have been quite prepared to act in their capacity as commander-in-chief and that Congress generally won't take issue with the president if the action is successful. Sam Sarkesian's chapter on special operations forces makes brief reference to the Reagan program, but focuses primarily on the general problem of unconventional war for the United States and the pre-Reagan history of special forces. The Gellner and Voas piece on arms control, in what must be a "first" in the treatment of this issue during the Reagan administration, does not mention the bureaucratic politics of the problem that have deeply colored the administration's policy.

What's missing from the Cimbala volume are important chapters on conventional force modernization (land forces, naval forces, and tactical air forces) and procurement. The goal of a 600-ship navy is mentioned tangentially in the Oliver and the Nathan and Davis chapters, but otherwise the major thrust of the administration's program to "rearm America" is ignored. In addition, the critical issues of procurement, spare parts, and the elimination of waste, fraud, and mismanagement are not mentioned. And what about the readiness and sustainability accounts, which may have been given less than their due by the administration? Finally, the editor should have included a summary chapter that ties together the issues of defense organization and leadership, program and budget, and relates these to the overall strategic philosophy of the administration.

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Searching for World Security: Understanding Global Armament and Disarmament. By Curt Gasteiger (New York: St. Martin's Press, 1985. xii, 216p. \$27.50).

Curt Gasteiger's new book is marked by a deep and ultimately unresolved tension. On the one hand the author makes repeated, impassioned pleas for some check on what he calls "the uninhibited militarization of our world" (p. 3). On the other, he puts forward a range of arguments for believing that no real deceleration (let alone wholesale disarmament) is even remotely likely.

In 12 central chapters, Gasteiger offers a review of recent trends globally (in terms of aggregate measures of military spending and manpower), regionally (the superpower competition, developments in Europe, Asia and the Third), and functionally (the expanding trade in arms, the prospect of further nuclear proliferation, the spread of chemical and biological weapons, the possible impact of new military technologies, and so on).

Much of what is said here seems quite sensible and it is accompanied by an array of useful tables and diagrams. Gasteiger notes, for example, that "in developing countries the use of force . . . still holds enough promise of gain and insufficient risk of punishment . . . to be considered a valid, useful, and hence legitimate political instrument" (p. 95), and he points out that "whether or not countries eventually decide to 'go nuclear' is fundamentally a political question" (p. 121).

In evaluating efforts to restrain the worldwide accumulation of armaments, the author is also, for the most part, admirably realistic. He remarks on the fading from serious discussion of calls for "general and complete disarmament" (p. 24), notes the failure of efforts at nuclear disarmament and touches briefly on the problems involved in even the narrower forms of arms control (pp. 29-30). The difficulty in each case, Gasteiger argues, is one of misplaced emphasis; "disarmament follows rather than precedes an easing of tensions . . . it is not possible in a world of tension and distrust" (p. 185).

How then are the conditions for arms limitations of any sort to be achieved? Here the author can do little more than recommend a "revolution in politics" to match the recent "revolution in technology" (p. 3). Such a trans-

formation would depend among other things on efforts to promote "common rules of [international] conduct" (p. 195), to "improve communication, information and transparency in security-related matters" and to "reduce the role of military power as an instrument of foreign policy" (p. 198). Professor Gasteiger argues that an accumulation of such steps would amount to "'disarmament without tears,' i.e., disarmament without any sacrifice of national sovereignty" (p. 202), but it would appear instead to be a call for anarchy without anarchy, for the preservation of a system of independent nation-states somehow bleached of the most essential characteristics of all such systems.

In the end, as the author recognizes, his proposals for disarmament depend on states being convinced "that the alternative, i.e., continued armament, is likely to leave them in a much worse situation" (p. 203). But is this in fact the case? Professor Gasteiger at one point makes the familiar assertion that few countries (not including, presumably, either of the two superpowers) can feel any more secure today than they did 30 years ago (p. 20). In 1956, however, the Soviet Union was vulnerable to U.S. nuclear attack and had virtually no means at its disposal for direct retaliation. Today the Soviets have good reason to hope that their strategic forces are sufficient to deter such strikes, even if they might appear to be the only effective means of responding to a Russian invasion of Western Europe.

Some measures of the Soviet attitude on these matters is revealed in a remark attributed to former defense minister Marshal A. A. Grechko: "The more military power the Soviet Union has, the more tranquil the world will be" (p. 182). No doubt. But if the Western democracies prefer continued independence to Soviet-style tranquility they would be well advised to look to their own defenses while they pursue what meager arms limitation agreements they can and wait for Professor Gasteiger's "revolution" to arrive.

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Roots of Failure: United States Policy in the Third World. By Melvin Gurtov and Ray Maghroori (Westport, CT: Greenwood Press, 1984. ix, 224p. \$27.95).

This is a timely book. A new interventionist spirit in the United States and the recent demise from power of two of the close allies of the United States, Ferdinand Marcos of the Philippines and Jean-Claude Duvalier of Haiti, require us to look afresh at U.S. policy in the Third World.

Roots of Failure examines the history of U.S. intervention in Iran, Nicaragua, and Vietnam to show the continuing dominance of realism and containment in U.S. foreign policy from the earliest stages of the postwar period to this day. This analysis of U.S. foreign policy covers familiar ground and draws upon the substantial political science literature on ideology, bureaucratic politics, national perceptions, and arms race theories. The three case studies of Iran, Nicaragua, and Vietnam demonstrate the U.S. proclivity from Truman to Reagan to seek military rather than political or diplomatic solutions to Third World problems and generally to back Third World right-wing authoritarian regimes in order to counter the totalitarian left.

As this book demonstrates, the postwar pattern is a familiar one. The Shah of Iran, Anastasio Somoza of Nicaragua, and various pro-U.S. regimes in South Vietnam declared an anticommunist stance in their foreign policies, requested and received vast amounts of U.S. aid, and provided the U.S. with bases, diplomatic support at international and regional forums, and investment opportunities for U.S. multinational corporations. The negative costs of these foreign policy "successes," however, were that these regimes proved to be unstable in the long term, alienated their own people and radicalized domestic opposition as a result of excessive corruption, malfeasance and human rights violations. Such regimes became liabilities rather than assets, did not serve the long-term U.S. national interests, and the United States found itself making last-ditch efforts to dissociate itself from its former friends and allies. Historically, such policies produced disastrous results as the new regimes proved to be either hostile to the United States, as in Ayatollah Khomeini's Iran, or to be Marxist-Leninist, as in the Sandinistas's Nicaragua.

The authors argue for the need to develop alternative foreign policy goals that take humanitarian needs into account. In particular, they contend that there is an urgent need for a new globalism that "would seek to get beyond national self-interest by resetting the conditions and content of international exchange, reversing and ultimately ending the arms race, and putting teeth into national and international human rights legislation—changes that could only be meaningful with the active cooperation of the United States." Drawing upon the already substantial World Order Studies literature, the authors call for substantially strengthening the authority of global institutions over global problems and for sweeping changes in the principles and practices of U.S. policy towards the Third World.

This reviewer shares the concerns of the authors of *Roots of Failure* and generally supports their recommendations. However, as the authors concede, the transition to a more humane U.S. foreign policy and to a new world order promoting human rights, basic needs, and nonintervention will not be easy. It will require radical alterations in the ideology, values, and attitudes of the foreign policy establishment. Thus, although *Roots of Failure* serves as a useful critique of U.S. foreign policy, the reader is still left wondering how to achieve a more humane world. What are needed are concrete proposals to tell us how to shift the conduct of international politics out of the hands of hard-line realists and into the hands of those who advocate the inclusion of humanitarian principles.

G. D. LOESCHER

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Economic Sanctions Reconsidered: History and Current Policy. By Gary Clyde Hufbauer and Jefferey J. Schott, assisted by Kimberly Ann Elliott (Washington, DC: Institute for International Economics, 1985. xvi, 753p. \$45.00).

Academics, practitioners, and the public at large have had a long love-hate relationship with the whole issue area of economic sanctions in international relations. The love part stems from the fact that economic coercion appears to occupy that halfway house between

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doing nothing and using some form of military force. The hate part comes from perceptions of sanction failure, usually in the most spectacular of cases.

Interest in economic sanctions also appears to wax and wane, going up when international situations of tension arise where "something" must be done but where military or quasi-military efforts are impractical, morally unacceptable, or both. The current renaissance of general interest in economic coercion is largely the result of sanction talk (and some action) against South Africa for apartheid and against Nicaragua for a wide array of perceived sins. The past year has seen the publication of two major books on economic sanctions. The first was David Baldwin's *Economic Statecraft* (Princeton University Press). The second is the book under review here, *Economic Sanctions Reconsidered: History and Current Policy*, by Hufbauer, Schott, and Elliott. Interestingly, the two books make excellent companion pieces. The Baldwin book is largely conceptual and historical, seeking to put more rigor and less passion into the *intellectual* study of "economic statecraft." The Hufbauer, Schott, and Elliott book is much more empirical and policy-oriented. It is important for a variety of reasons, not the least of which is an attempted methodological step forward in the study of economic sanctions.

When initially hefted, the Hufbauer, Schott, and Elliott book is daunting indeed, 753 pages. Rather unconventionally, however, the "main" part of the book is only 95 pages; appendixes comprise the remainder, more than 650 pages of which are case summaries. Methodology, statistical analyses, and case compilation have been moved to the appendixes, leaving the authors stylistically and substantively free to summarize and draw conclusions in the main part of the book. Given the need for most academically oriented presses to seek wider markets, we should expect that this pattern will be increasingly common.

According to the authors, the purpose of their book is to answer three central questions (p. 1):

1. What factors in a sanctions episode—both political and economic—usually result in the achievement of foreign policy goals?
2. What are the costs of sanctions to both target and sender countries, and to what extent do they influence policy decisions?

3. What lessons can be drawn from this experience to guide the policy maker on the use of sanctions in the future?

For practitioners, the heart of the book is chapter 1 and chapter 5. Chapter 1, a brief introduction, puts the use and study of economic sanctions in historical perspective and explains where economic sanctions fit inside a foreign policy option continuum. Chapter 5, the conclusion, summarizes the results of the statistical analyses of 103 sanction episodes and offers the "surprising" conclusion that sanctions were successful in 36% of the cases. Chapter 5 also offers policy makers "nine commandments," the do's and don'ts of economic coercion: (1) keep sanction goals realistic; (2) pick on weak targets; (3) target "friends" for coercion, because allies are more vulnerable than longtime adversaries; (4) apply all available economic sanctions fully, not incrementally; (5) apply sanctions quickly; (6) beware of domestic costs of sanctions; (7) be cautious about using parallel forms of coercion—"covert action, quasimilitary measures, or regular military operations" (p. 89)—because they do not improve the chances of success; (8) do not count on multilateral sanctions, because they are difficult to organize and are probably not worth the effort; and (9) plan carefully.

For academics, the heart of the book is chapters 2 through 5, but especially the appendixes. It is occasionally on the academic side that the book is problematic, and the point on which criticism will be focused will probably be the definition of the "success" of a sanction attempt. The authors devise two simple index systems, each scaled from one to four. The first assesses "the extent to which the policy outcome sought by the sender country was in fact achieved" (p. 32), ranging from (1) *failed* through (2) *unclear but possibly positive* and (3) *somewhat successful* to (4) *successful outcomes*.

The second index reflects assessment of "the contribution made by sanctions to a positive outcome" (p. 32), ranging from (1) *zero-to-negative* through (2) *minor* and (3) *modest* to (4) *significant contributions*. The two scores on any case are then multiplied to obtain a 1-16 "success score" (p. 33). That is about all the explication offered.

Other indexes are created for the variables "international cooperation" with the sender

(pp. 34-36), "economic health and political stability" of the target (pp. 36-37), and "prior relations between sender and target" (pp. 37-38). The authors also make a noteworthy attempt to assess the solely *economic* costs of a sanctions episode for both target and sender (pp. 38-39, 60-69, and appendix B). So much, including conclusions drawn from the key multiple regression analysis, depends on the "success score" that the brief discussion leaves this reviewer uncomfortable. It also brings up the issue of using case studies as the source of the data.

For the basic data, the book relies on analyses of 103 sanction cases from 1914 (U.S. versus Germany) to 1985 (U.S. and others versus Grenada). This approach, however, is a classic two-edged sword. On the positive side is the fact that the authors have satisfied a basic methodological tenet. They have compiled a respectable number of case studies, asked a consistent set of questions of each and statistically analyzed the results. On the negative side, the case evidence is highly uneven in quality, being more suspect obviously in recent cases. That is, the basic building blocks of the statistical analyses differ in size, shape, and quality of material. I foresee a significant number of dissertations focusing on interesting but inadequately documented "sanction episodes" among the 103 cases. The authors must be commended, however, for actually doing what most others simply talk about, turning case studies into data.

In addition to a too-abbreviated discussion of "success" and of methodologies in general, the book does not deal with two dimensions of economic coercion that probably represent the next generation of scholarship in this area: (1) the deterrent utility of economic sanctions and (2) the role of positive sanctions (rewards) in foreign policy success. Of course, both issues pose methodological difficulties that make those of Hufbauer, Schott, and Elliott pale by comparison. Nonetheless, conceptual and empirical analyses on these two issues are vitally necessary to round out exploration of economic sanctions in international relations.

On balance, *Economic Sanctions Reconsidered* is provocative and should be read by anyone interested in the role of economic coercion in international relations. Practitioners will find it informative. Academics will take their shots at conceptual definition and meth-

odology, but Hufbauer, Schott, and Elliott must be recognized as having provided a courageous point of departure for rigorous empirical work on economic sanctions.

RICHARD STUART OLSON

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African Regional Organizations. Edited by Domenico Mazzeo (Cambridge: Cambridge University Press, 1984. ix, 265p. \$42.50).

Famine, military coups d'état, and rhetorical posturing seem to be the staples out of which the mass media (aside from Hollywood historical dramas with colonial settings) fashion popular images of Africa. Notwithstanding the threnodies about the continual violence occurring in the black townships of South Africa, African politics can be quite placid and unglamorous, even in many government ministries and bureaus. This particular work provides a needed corrective to fuzzy and incomplete popular writing about African states and their relations with African and extra-African states. The editor and his nine colleagues, who teach in U.S., Canadian, and east and west African universities, have furnished an exceptionally well-arranged analysis of the transnational organizational politics of sub-Saharan Africa. All of the contributions are original essays, and all are admirably linked through an exemplary bibliography of books, articles, and unpublished materials in English and French dealing with African regional organizations.

Thanks to a smooth, articulate editor, the reader need not be an expert in either the African politics or international organization fields. Professor Mazzeo surveys the post-World War II decolonization era as it relates to Africa in order to demonstrate why the early federal approach (favored most visibly by Kwame Nkrumah of Ghana) was displaced by the functional school of international organization. Africa, he notes, was receptive to extracontinental ideas of cooperation given the interest of African leaders in the results of Western European economic, political, and security cooperation, if not integration. This receptivity was an indicator of the extent to which these leaders had become devotees of the creed of pan-Africanism, a creed that

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offered the prospect of pooled strength from individual national weakness. This collection of essays, avers the editor, will "provide the reader with a better grasp of the possible complementarities between continental and subcontinental cooperation as instruments for building and strengthening the national capacity for development and for helping restructure the international system" (p. 9).

Four authors consider organizations at the continental level (the United Nations, the Organization of African Unity (OAU), the African Development Bank, and the Economic Commission for Africa). Leon Gordenker observes how skillful African diplomats are in using the UN "as a negotiating theater" (p. 24), while he correctly emphasizes the welter of differing concerns that split the African group, especially once they have dispensed with the unifying issue of African majority rule in Southern Africa. The OAU is an even weaker and more impoverished ensemble of states. K. Mathews suggests that OAU members were captivated with "the accelerated struggle for national identity, [and] the struggle for . . . consolidation of internal political power and gaining political influence in relation to their neighbours. Consequently, there seemed to be little time and energy left for such remote concerns as the OAU" (p. 78).

Five of the authors consider the record of regional organizations at the subcontinental level. These organizations are the defunct East African Community, the Customs and Economic Union of Central Africa, the Economic Community of West African States, the francophone African and Malagasy Common Organization, and the recently inaugurated Southern African Development Coordination Conference (SADCC). It is at this level that one observes less flamboyance as well as marked foreign economic intrusion, especially in francophone and Southern Africa. Peter Meyns suggests that "the prospects for the viability of SADCC within the framework of what it can realistically hope to achieve . . . seem quite good" (p. 219). In light of the intense pressure on South Africa's trading partners to implement economic sanctions, interest in the viability of the SADCC is nothing less than prudence. This loosely structured group of none internationally recognized states is attempting to diminish dependence upon South Africa in an incremental, sectoral

fashion, while withstanding South African attempts at destabilization.

RICHARD DALE

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Cyrus Vance. By David S. McClellan (Totowa, NJ: Rowman & Allanheld, 1985, ix, 194p. \$22.95).

Henry Kissinger: Perceptions of International Politics. By Harvey Starr (Lexington, KY: University Press of Kentucky, 1984. xiv, 206p. \$23.00).

David McClellan's *Cyrus Vance* provides us with an overview of the man, his education, early law career, and experience as a diplomatic troubleshooter. The logical choice for secretary of state, Vance shared the president's desire to promote a better world order and was perceived as a team player who would fit well into Brzezinski's notion of a balanced leadership in the area of foreign affairs.

Yet, as the details given in this work suggest, Vance was never to penetrate into the president's inner circle. Carter's moralism, as McClellan argues, left him impervious to the constraints that would undermine some of his most ambitious projects, while Vance's temperament and earlier diplomatic experience led him to face these limitations. This difference, plus Vance's dislike of abstractions, undermined his influence with the president, who liked grand plans. There were fundamental conflicts, too, with Carter's national security advisor. Zbigniew Brzezinski relished confronting the Soviets, while Vance was deeply committed to finding common ground with them so that an arms limitation agreement could be signed and the general level of hostility reduced. Thus, Vance opposed the deep cuts in nuclear war heads in the president's first arms limitation proposals to the Soviets against the advice of Brzezinski, who seemed to relish the thought that the Soviets would reject them. Later, Vance's diplomatic efforts to isolate problems due to Soviet involvement in the Horn of Africa from the strategic arms limitation talks were undercut by Brzezinski's public statement linking Salt II to the issue. Beyond this, Brzezinski saw radical nationalistic movements as working to the Soviet advantage, while Vance believed that nationalism

would override ideology and that the national interest need not lead the United States into opposing radical movements throughout the world. Brzezinski was inclined, also, to opt for the use of force whenever he thought U.S. interest was endangered, while Vance was much more inclined to opt for diplomatic means.

Yet, Vance's tenacity and attention to detail were crucial components in two of Carter's great foreign policy accomplishments: the Panama Canal treaties and the Camp David negotiations on the Middle East, with the subsequent Israel-Egyptian Peace Treaty. Vance, urging a country-by-country approach, also brought to Carter's human rights policies a balanced and prudent appreciation of the limits of U.S. power and wisdom.

Overall this is a well-written, well-argued book that accomplishes what it sets out to do, namely, provide a concise overview of the contribution of one secretary of state. The sources are mainly memoir materials, published articles, and a few interviews with Washington influentials, including Cyrus Vance.

There are some matters, however, that McClellan does not deal with that this reviewer would like to have seen. Somewhat surprisingly, given the author's emphasis on Vance's character, is his nearly complete neglect of the decision-making process that led to the Iranian hostage rescue operation and Vance's extraordinary resignation from office. Indeed, one can obtain a better feel for the moral courage of the man from Hamilton Jordan's recollections of the secretary of state's opposition to that venture, *Crisis* (Putnam, 1982) than in this work. McClellan could also have better explained the factors limiting Vance's influence on Carter if he had gone beyond face evaluations of Carter's personality and his relationships to others. When one looks at some of the decisions that created political problems for Carter—the grandiose proposals to the Soviets for deep strategic arms cuts, the 180-degree turn on the neutron bomb, his initial stubborn insistence on the withdrawal of troops from South Korea—one finds not an excess of morality, but Carter's extraordinary pride—in other words, his desire to do very important things and not be shown wrong. Vance, emphasizing limits, did not fit in well with those proclivities.

Finally, a closer analysis of Carter's earlier career and rhetoric would suggest that in fact—if not in presidential rhetoric—Carter was really more predisposed to follow Brzezinski's tough line against the Soviet Union than Vance's more moderate approach. As I have shown in *Jimmy Carter: In Search of the Great White House* (Norton, 1980), Carter had been a leader in the "Anyone But McGovern" movement in 1972, and in his speech nominating Henry Jackson for president, he had called for a U.S. arms buildup. Carter had even run advertisements in the South during the 1976 campaign, attacking the Ford-Kissinger detente policies and aligning himself explicitly with Ronald Reagan. Carter's failure to back Vance against Brzezinski's public Soviet baiting, his tilt towards playing the China card, and his linking Salt II to events in the Horn of Africa—all were in tune with the deeper instincts Carter had suppressed in his efforts to win the presidency in the post-Vietnam mood of 1976.

In *Henry Kissinger*, Harvey Starr delineates the impact of Kissinger's personality and operational code on his attitudes towards the USSR and the People's Republic of China. To do this he uses a variety of techniques. In part 1, he draws from traditional biographies, psychobiographies, and Kissinger's own writings before taking office to delineate Kissinger's personality and basic world view (i.e., *operational code*). As he shows quite convincingly, the traumas of Kissinger's early life experiences—the flight of his family from Nazi Germany when he was fifteen and the need to make his own way in an unfamiliar U.S. culture with no father to pave the way for him—led him to a view of the world in which disorder is always threatening to overwhelm order and power must be obtained to prevent that from happening. An extraordinarily able young man, he found patrons to help him attain that power and his subsequent historical and political analyses were elaborations of his basic premise that the preservation of order is the highest political value.

In part 2, Starr concentrates on one aspect of Kissinger's belief system, his images of the enemy. Employing quantitative techniques (first developed by psychologist Charles Osgood) to Kissinger's public statements from 1970 through 1976, Starr shows that Kissinger had a relatively open belief system. Unlike Dulles, to whom Ole Holsti earlier applied this

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technique (D. J. Finlay, O. R. Holsti, and R. R. Fagan, eds., *Enemies in Politics* [Rand McNally, 1967], pp. 25-56), Kissinger did not see an insatiable and evil USSR facing the U.S. His anti-Soviet statements came after Soviet forays into the Horn of Africa and were a response to their challenge to the principle of order. As for the People's Republic of China (PRC), Kissinger's perceptions were inversely related to his perceptions of the USSR. As he came to distrust the Russians, his friendliness towards China increased, belying his earlier statements that the U.S. should never try to play off one country against another in a triangular balance.

The relevance of Kissinger's perceptions of the USSR and China to U.S. policies is analyzed by relating these perceptions to events data as measured by the Azar Sloan scale for the period from 1972 to 1974. Starr concludes that there is no clear relationship between Kissinger's negative perceptions of the USSR and U.S. policies. Perceptions have an indirect relationship to behavior, he concludes, the latter being influenced by a number of other variables. Finally, Starr shows that no important changes in Kissinger's perception of the USSR or the PRC were related to changes in his role as he moved from being the national security assistant to his position as secretary of state under Nixon, then Ford. From this finding, Starr speculates that idiosyncratic factors may be more influential than role for a high-level decision maker.

Through the use of quantitative techniques, Starr gains a precision in his analysis and an ability to make comparisons with greater certainty than would be possible with a more qualitative study. It is one of the best studies in this genre that has been produced in recent years. When the book is compared to McClellan's, however, it is clear that in the quantitative section of this work, Starr sacrifices, to some extent, contextual variables, facility of expression, and breadth of judgement. For the qualitatively oriented scholar, it sometimes appears that he uses extraordinarily complicated scientific techniques to arrive at conclusions that could be more simply demonstrated. Moreover, there is a problem in his research design. His conclusion that Kissinger's views of the USSR did not have much of an impact on policy may very well be due to the time frame he used (which is prior to Soviet

involvement in Angola) and the short (two-month) lag between event and perception. Certainly, Kissinger's negative assessment of Soviet involvement in the Horn of Africa contributed to a political climate that by 1976 would make detente a dirty word in U.S. politics. Finally, Starr's speculative generalization about the relative unimportance of role is not persuasive to those who take a broader historical perspective. There were no really important differences in the powers he had in each role. A comparison between Kissinger and Vance as secretary of state would be more instructive. One man operated in a political setting that gave him primacy in the formulation of foreign policy, the other in one in which there were strong competitors, including the president.

Both of these works deal with an important subject too often neglected in political science, namely, the importance of the individual in foreign policy making, and both do well what they set out to do. Each shows the strength and weakness of its genre. The McClellan book covers more material and excels in facility of presentation. The Starr book goes further in its attempts to develop both general theory and the quantitative techniques that permit greater certitude of research results. A broader historical perspective would have increased the sophistication of each author's interpretations of one or two significant matters.

BETTY GLAD

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Verification and Arms Control. Edited by William C. Potter (Lexington, MA: Lexington Books, 1985. xiii, 266p. \$29.00).

This is one of the first works to address the problems of arms control verification—a particularly pertinent issue area now, given the Reagan administration's emphasis on it. The book is the product of a conference held in 1984 and brings together chapters that fall into two general areas: the capacities and limitations of verification technologies and the political implications raised by both verification capacity and its results.

Chapters addressing verification capacity include Warren Heckrotte's discussion of the monitoring requirements for the various test-

ban proposals, William Durch's consideration of Antisatellite Satellite (ASAT) control and its monitoring difficulties, Jeffrey Richelson's elaboration of U.S. verification technology, and Dean Wilkening's chapter on monitoring cruise missiles and bombers. Each of these areas demonstrates the limits of verification capacity, ranging from modest (in the test-ban treaty area) to serious (in the ASAT area, due in part to the number of potential ASAT weapons, and the bomber and cruise missile categories). These problems suggest that arms control efforts in such areas might profit from the suggestions found in James Schear's chapter on cooperative measures, even though these measures offer only limited benefit.

The political implications of verification include such things as compliance standards, superpower cooperation on verification issues and results, and the impact of domestic political change. On this last point Michael Krepon discusses changes in the verification issue in the U.S. from the looser "adequate" verification of 1963-1969 and the tougher "effective" verification standards of the Reagan administration, coupled with a more public airing of suspected Soviet violations. Dan Caldwell notes how this shift has taken place on Standing Consultative Commission issues, claiming that until recently it performed a useful role in clarifying and resolving arms control compliance issues. Mark Lowenthal and Joel Wit note, though, that some of these changes come from vague verification standards conflicting with more stringent verification requirements brought about by increasingly complex arms control agreements.

It is easy to simplify verification difficulties—some charge that the Reagan administration uses them to legitimize its disinterest in arms control, while others claim that the Soviets will cheat on arms control provisions whenever they can. But, as Allan Krass notes, there are fundamental differences in the ways the two sides view verification issues, even to the extent of how they are sequenced in arms control negotiations. Each side fears that verification permissiveness will simply enhance the other side's ability to carry out espionage. And in some cases there are practically no agreed-upon means of verifying compliance with agreements. F. R. Cleminson notes this with respect to limits on chemical and biological weapons.

None of the chapters is excessively technical—a boon for the reader who is not so inclined. Of course, this means that some of the more interesting technical details of the monitoring systems are omitted, which may leave some readers wondering just how the systems work and how reliable they really are. Security classification may be responsible here.

The editor might have provided a more comprehensive introduction, linking the political-strategic with the technical aspects of verification, thus integrating more fully the disparate chapters in the collection. But Potter can be commended for bringing together a distinguished group of specialists whose contributions collectively illustrate both the limitations and opportunities for verification as a part in the arms control process. He has chosen a fairly homogeneous group. It would be fair to characterize the contributors as arms controllers, who would probably be uncomfortable with the present administration's absolutist approach to verification questions. They collectively make a good case, though, that there is sufficient complexity in the entire issue area to prevent strict judgements as to either compliance or violation of arms control agreements.

DAVID S. SORENSEN

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Camp David: Peacemaking and Politics. By William B. Quandt (Washington, DC: Brookings Institution, 1986. xvi, 339p. \$32.95, cloth; \$12.95, paper).

William Quandt, then a senior staff member of the National Security Council, played an important role in shaping the Carter administration's Middle East policy and was an active participant in all the events leading toward and occurring during the Camp David negotiations.

In addition to his own observations, Quandt consulted relevant, still-classified documents at the Carter library and he carried out extensive interviews. The result is a detailed and judicious assessment on the process of making Egyptian-Israeli peace although the book also provides a broader history of Middle East policy during the first half of the Carter presidency. The Camp David talks are of the great-

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est interest not only as an important political breakthrough in the Middle East context but also as a high point in the management of the policy process itself—with successful coordination of all parts of the executive branch—and as a case study in diplomacy equivalent to the Cuban missile crisis or the Nixon administration's opening to China. Since the book is so clearly written and each stage of the developments is so well explained, it will be of tremendous benefit for courses on international affairs and contemporary Middle East politics.

After an excellent background discussion on the pattern and timing of presidential interventions in the Middle East, Quandt sketches the views of the main U.S. policymakers. This survey demonstrates particularly the administration's eagerness for diplomatic progress in the Middle East and its willingness to devote so much of its time and resources to the issue. But such energy in and of itself did not suffice. Attempts to reconvene the Geneva conference tended to become an end in itself. An initiative designed to win PLO acceptance of UN Resolution 242 used much of the White House's political capital and (like later efforts in this direction) got nowhere in the face of the PLO's intransigence.

President Anwar al-Sadat's dramatic announcement of his willingness to visit Israel broke the logjam, showing the necessity of the local states taking the lead in making any progress on the Arab-Israeli conflict. But the administration also soon concluded that the initiative could succeed only with further U.S. involvement. "For Egypt and Israel, it is fair to say that peace was possible, but not inevitable, after the October 1973 war" (p. 4). Of course, Carter realized, in Quandt's words, "the traps and dangers of playing the role of intermediary between two highly suspicious adversaries, one of whom had a strong constituency in the United States, and the other of whom was rapidly becoming a media star of consequence" (p. 117).

While the talks themselves were conducted very well, the U.S. side did not fully understand the regional context, believing, Quandt notes, that "Egypt was the key to war and peace in the Middle East. If Egypt chose peace, other Arab states would eventually follow" (pp. 1-2).

The problem of the aftermath of Camp

David is that an Egyptian-Israeli agreement could not bring about the participation of other elements, including the Palestinians, Jordan, or Saudi Arabia. The point was that peace was only possible—no matter how energetic and innovative U.S. involvement might be—when the parties involved were ready to make peace. These latter Arab forces were not then prepared to do so and they have remained unable or unwilling to take the necessary steps despite such efforts as the Reagan plan (1982-1983) and King Hussein's attempts to assemble a Jordanian-Palestinian joint delegation (1985-1986). While, as Quandt argues gently—and other critics more vociferously—Carter might have better handled his follow-up campaign to the agreements, Arab unwillingness or inability to step forward and the continuing veto exercised by the PLO and Syria against direct negotiations, have been the main barrier to progress.

There seems to be a peculiar U.S. attitude toward diplomacy that is seen with particular frequency in discussion of Middle East negotiations. Rather than perceiving long-range trends and efforts that build upon one another, observers argue that any initiative or step that does not bring an instant breakthrough is, in fact, a failure. This kind of thinking has led to some dismissal of the progress achieved at Camp David. Nonetheless, as Quandt concludes, "the two agreements reached at Camp David marked an important watershed in the peace negotiations, but a long road remained to be traveled before peace would actually be achieved. Along the way there would be pauses, detours, some backtracking, and many dead ends" (p. 259). This well-written and judicious book will probably remain the definitive work on one of the most important landmarks on that road.

BARRY RUBIN

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The Ties That Bind: Intelligence Cooperation between the UKUSA Countries. By Jeffrey T. Richelson and Desmond Ball (Boston: Allen & Unwin, 1986. xviii, 402p. \$29.95).

During the investigation of the federal intelligence agencies by the Senate Select Committee on Intelligence (the Church Committee) in

1975, senators often wished they knew more about how intelligence worked in other democracies, the better to evaluate how it ought to work in our own. Yet, little information was available to answer this question. Now a solid comparative analysis has been undertaken by Richelson and Ball.

Part 1 of the book instructs us in the organization and mission of the intelligence agencies in Britain, Australia, New Zealand, Canada, and the United States—combined, one of the largest bureaucracies in the world with a budget of some \$16–18 billion and authority to use severe coercive powers. With these “wiring diagrams” in place, the authors proceed to the central question of how the agencies operate both at home and within the U.K.-U.S.A. (UKUSA) relationship. Separate chapters on signals intelligence and ocean surveillance illustrate the impressive—some would say frightening—capacities of these agencies to track the movements worldwide of friend and foe.

For political scientists, the key portions of this study are those which focus on the discord and collaboration among these agencies. We have long known that the U.S. intelligence agencies often resemble embattled fiefdoms, warring among themselves over budgets and missions; here we learn that—small comfort—the intelligence establishments of other democracies are as fragmented as our own. As for relations between the UKUSA nations, the authors state that, despite much sharing of valuable information, “there is also an enormous amount which is not fully shared among the five countries” (p. 168). In some cases, the need-to-know principle excludes certain countries (CIA paramilitary operations in Nicaragua are of little relevance to New Zealand). Agencies also refuse to reveal, even to friendly nations, sensitive *modus operandi* or the identity of agents. And, yes, Virginia, the allies spy on one another (though with more restraint than against enemies), and, obviously, they do not disclose these operations either.

The authors, in a word, provide a unique survey of the hidden side of five democratic governments. Sometimes the study reads too much like a survey, a description of each item in the pantry, when one might hope for more evaluation, even theorizing. We are told, for example, that the President’s Foreign Intelligence Advisory Board (PFIAB) “has the

authority to review the performance of all agencies . . . [and has] a full-time staff and consultants to conduct special inquiries” (p. 130). This leaves one with the impression that this panel really plays a significant role. Harry Howe Ransom’s evaluation of PFIAB as more “a polite alumni visiting committee than a vigorous watchdog” (*New York Times Magazine*, 21 May 1961) remains closer to the mark. The inevitable error or two also creeps into the work. The late William C. Sullivan of the FBI is charged with writing an anonymous letter to Rev. Martin Luther King, Jr., encouraging his suicide (p. 294). This despicable event in the Bureau’s history certainly happened, but whether Sullivan was the perpetrator was never established with certainty by congressional investigators. These are minor distractions, though, from a work that is commendable for its scope and thoroughness of research in a field where information is difficult to obtain.

The authors are sensitive to the danger these powerful agencies pose to liberty. They write that “many members of the UKUSA security services are unable or unwilling to distinguish between protest and dissent on the one hand and subversion and disloyalty on the other” (p. 306). To guard against this danger, the authors advocate “firm, continuous and responsible oversight and direction” (p. 307) and a citizenry “fully apprised of the nature and operations of these agencies” (p. 309). The authors have made a significant contribution toward this laudable goal of an informed citizenry.

LOCH JOHNSON

University of Georgia

Lawmaking and Co-operation in International Politics. By David Sanders (New York: St. Martin’s Press, 1986. xi, 161p. \$22.50).

It is perhaps ironic that, at a time when realism (in its classical or neorealist form) has been generally reaffirmed as the dominant paradigm in international relations, international law—a subject so closely associated with the idealist paradigm—has been enjoying a modest revival. Of course, Kaplan and Katzenbach long ago showed how international law, rather than representing a pie in the sky, Pollyannish

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approach to world order, was not only compatible with realism but was grounded in it. More recently, Gilpin and others who have explored the "hegemonic stability theory" of regime change have likewise shown how international politics and international law can be viewed as two sides of the same coin.

David Sanders has produced a monograph that also seeks to examine international law in a realistic fashion, accepting the realist proposition that international politics is driven primarily by *realpolitik* considerations but arguing that international law can also play a role in shaping interstate politics. In particular, his "reconstructed idealism" hypothesis posits that the more frequently countries engage in law making (treaty making), the more likely they are to develop a degree of mutual trust that will reduce the possibility of their resorting to armed force against each other in the future. It is the law making or treaty making process, more than international law itself, that is a source of peace. He submits this hypothesis to empirical testing through both an elaborate data analysis of dyadic treaty making and war making patterns in the interwar period (1920-1942) and a detailed case study of Anglo-Turkish relations during the interwar years. The findings provide partial support for Sanders' thesis, his major conclusion being that "in certain limited contexts in the interwar period . . . in situations where disputes between nation-states were less intense and less in immediate need of resolution by force than they were at the (European) core of the international system, the employment of the treaty making process did indirectly reduce nation-states' tendency to use force as a means of settling the conflicts of interest between them" (p. 67). The author found that the relationship between treaty making and war avoidance was strongest in those cases where pairs of states were distant geographically, culturally, and in terms of power. Anticipating the criticism that his reconstructed idealist hypothesis is tautological and given to spurious correlation, Sanders conducts a variety of tests to demonstrate that the proclivity of states to enter into treaties can be treated as a causal factor in their subsequent war avoidance rather than being dismissed merely as a manifestation of already trustful and peaceful relations.

Sanders is to be commended for adding to a growing literature in recent years that, depart-

ing from the heavily philosophical, legalistic, and moralistic tradition of the idealist school, has sought to subject international law to rigorous and systematic empirical analysis in order to increase understanding of the actual as opposed to the hoped-for role of international institutions. Notwithstanding the author's obviously considerable skills at quantitative and historiographical methodology, however, the reader is left unpersuaded by both the theoretical argument and the evidence. As a theory of how habits of cooperation can be learned and trust can be developed among states, leading eventually to peace between them, Sanders' thesis sounds strikingly reminiscent of notions advanced by the likes of Mitrany and Haas (functionalism), Deutsch (transactionalism), and Osgood and Rapoport (game theory), but is not nearly as well developed and sophisticated. Although the study assumes that "the most obvious and potentially fruitful 'non-fatal if unsuccessful' co-operative strategy available to nation-states in the contemporary international system is the bilateral . . . treaty-making process" (p. 2), why should one think that treaty making is a particularly key engine for building trust and peace, as distinct from the cooperative dynamics represented by international organization membership, regime compliance, expanded transaction flows, or Graduated Reduction in Tensions (GRIT)-type confidence-building diplomatic initiatives? As an attempt to provide a stronger empirical foundation for "reconstructed idealism," the study is too narrowly couched either to validate idealist thought or to qualify the validity of realist thought. In any case, one does not have to be a "fanciful optimist" or to have read this book to believe that "cooperative strategies in international politics are at least worth trying" (p. 1).

J. MARTIN ROCHESTER

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The Vietnam War: A Study in the Making of American Policy. By Michael P. Sullivan (Lexington: University Press of Kentucky, 1985. 198p. \$20.00).

Michael Sullivan is the author of a well-known book surveying the contributions of empirical political science to the study of inter-

national relations. In this book he continues his quest for a more scientific, theoretical approach to international relations by examining the Vietnam War. Although he does have policy goals and interests as well—to help focus attention on what he sees as the neglected lessons of the war for U.S. foreign policy—his primary goal is to use the war as a case study to test several theories about U.S. foreign policy. Still more broadly, by applying the social scientist's method of systematic empirical analysis to the Vietnam War, he wishes "to demonstrate that the study of American foreign policy need not be limited to the traditional diplomatic historian's analysis" (p. 6). Finally, the study is intended to bridge the gap between what the author considers to be two styles of analysis: historical examinations of single cases with no theory or quantitative analysis on one hand and highly theoretical and empirical analysis of many cases on the other.

These are worthy and ambitious goals, but the results are disappointing. For example, one chapter focuses on the well-known dispute over whether the Vietnam War is best explained in terms of the "incrementalist" or the "rational" model of decision making: did policy makers inadvertently stumble into a quagmire or did they knowingly choose what plausibly were the best means to achieve worthy goals (however badly it all turned out)? Surprisingly, the author brings scarcely any new empirical evidence to bear in his effort to shed new light on this puzzle. Sullivan's argument is obscure and lame. While leaning toward the incrementalist

model, he seems mainly to be concerned that policy makers not be blamed for their human frailties and that social scientists maintain "as broad a perspective . . . as possible" and avoid "imposing one rigid model of decision making" on the war (p. 86).

The central empirical focus of the book is devoted to testing the applicability of the famous Klingberg thesis to the Vietnam War and its aftermath: does U.S. foreign policy follow alternating 20-25-year cycles of isolationism and expansion, or "introversion" and "extroversion"? Making use of public opinion data, Sullivan concludes that the Klingberg thesis has been confirmed, as the post-World War 2 mood of extroversion gave way in the late 1960s to a broad introversionist consensus that persists today. However, this analysis is questionable on at least two grounds. First, despite the Reagan administration's difficulties in getting public support for military intervention in Lebanon and Central America, it is not clear that there is really a consensus on introversion or isolationism comparable in breadth to that prevailing between the first and second world wars. Secondly, to the extent that the overall national mood of the country did shift in the 1960s, it would seem more plausible to explain this as a direct result of the unpopular Vietnam War and its disastrous conclusion rather than as a generalized, predictable long-cycle phenomenon presumably unconnected with any specific event.

JEROME SLATER

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POLITICAL ECONOMY

The Political Economy of U.S. Import Policy.
By Robert E. Baldwin (Cambridge: MIT Press, 1985. xi, 238p. \$22.50).

Decisions taken by the U.S. government on whether or not to try to impede the flow of foreign-made goods in the world's largest single national market are significant determinants of resource allocations in the world

economy and of the state of political relations between the United States and its trading partners. Import policy involves the pursuit of perceived economic rationality as well as the exercise of political power to determine who produces what; as such, it is of importance both to political scientists and economists, neither of whose disciplines by itself can fully explain the phenomenon of foreign trade policy making.

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Both disciplines will be enriched by the relatively brief study of Professor Baldwin, an economist with experience in Washington's foreign trade apparatus. Nevertheless, academic reactions to this book are likely to fall into a broad spectrum between criticism and praise in accordance with readers' attitudes towards the modeling of bureaucratic behavior.

On the one hand, the author has successfully achieved his stated objective of contributing "to a better understanding of the manner in which U.S. import policies are formed and implemented" (p. 175). On the other hand, he has failed to do the seemingly impossible: to divine either a single theoretical construct of the import decision-making process or a finite set of criteria to predict flawlessly which of several models will determine behavior in a given policy-making exercise. Such a spectacular achievement may be demanded by some as the determinant of a successful new study of this subject.

Such an accomplishment is a long, long way off, and Baldwin has resisted the tack of latching onto a master theory and then selectively choosing from hundreds of case studies to allegedly verify its utility in explaining past actions. Instead, he has reached a very realistic conclusion that will disappoint those who want to bottle certain slices of human behavior into a neat quantitative formula. Baldwin calls for an "eclectic approach" to understand import policy behavior: at least until the various models are "differentiated more sharply analytically and better empirical measures for distinguishing them are obtained" (p. 180).

The book's methodology begins with a brief recitation of five models developed within the framework of public choice theory, a body of thought useful to economists trying to discern why political leaders stray from economic theory when making decisions. These models are repeatedly related to decisions examined in the next three chapters, the main body of the study. Baldwin separately examines actions in the three main institutions of import policy decision making: Congress, the International Trade Commission (ITC), and the senior levels of the executive branch. Efforts at linkage are supported by original and useful quantitative efforts to determine important variables in decision making. The author's repeated admissions that his statistical efforts have failed to

produce unambiguous results will please the realists and frustrate zealous behavioralists.

Economists will not like the absence of macroeconomic considerations (e.g., large federal budget deficits and a very overvalued dollar exchange rate) in explaining recent U.S. import policies. And stylists will take issue with Baldwin's propensity for repetition in this brief book (especially the themes of his five models) and his excessive fascination with the detailed workings of the ITC.

But criticisms should not prevent acknowledgement of this very useful contribution to understanding the complex, dynamic import policy making equation that consists of different institutional dynamics, economic considerations, political pressures, career ambitions, personal ideology, and ethics. By this original linkage of a synthesis of public choice theory with an empirical review of import policy decisions, Baldwin has helped both economists and political scientists to better understand why U.S. foreign economic policy is as it is.

STEPHEN D. COHEN

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Introduction to the Theory of Social Choice.

By John Bonner (Baltimore: Johns Hopkins University Press, 1986. 205p. \$25.00, cloth; \$8.95, paper).

Rational Politics: Decisions, Games, and Strategy. By Steven J. Brams (Washington, DC: Congressional Quarterly Press, 1985. 233p. \$16.95, paper).

These two books provide disparate introductions to the microfoundations of social (political) choice. Despite a common goal, these books address fundamentally different audiences. John Bonner, an economist, successfully guides the reader through the basic tenets and problems of social choice in a noble effort to provide a comprehensive but introductory overview of a difficult subject. Steven Brams's *Rational Politics* is cast in game theoretic terms and is more appropriately read as a supplemental text by those seeking to make the leap beyond the axioms of social

choice to applied examples within the political arena.

The unabashedly economic orientation of Bonner's book should be of no concern to a political science audience. The seminal works of Kenneth Arrow and Amartya Sen, familiar to economists and political scientists alike, underpin most of the theory to which the reader is introduced. The generic nature of the problem of social choice is underscored by the organization of Bonner's book. In consecutive chapters the author considers political and economic solutions to the inherent paradox confronting the designers of social choice mechanisms.

Because a true paradox has no solution per se, discussions of social choice mechanisms often lead to normative statements about the relative merits of competing alternatives. To his credit, Bonner maintains a steadfastly impartial stance throughout his presentation. While this adherence to neutrality is an admirable quality for an introductory text, it is also partially responsible for one shortcoming of the book. In his systematic presentation of the attributes and drawbacks of various social choice mechanisms Bonner fails to discuss why, in equilibrium, we observe the coexistence of mechanisms with distinctly different normative implications. Such a discussion would serve as a useful concluding or summary chapter, a feature notably absent from the book.

The relatively abrupt end of the book is characteristic of most of the individual chapters which otherwise follow in a logical, well-conceived progression. The first three chapters carefully establish the microeconomic foundations of the subsequent social choice analysis. These early chapters cover a deceptively large amount of material and should be read with care. Bonner provides short, annotated bibliographies at the end of each chapter. The inclusion of these references mutes the inevitable criticism of omission easily directed at introductory works.

The transition from Bonner's *Introduction to the Theory of Social Choice*, to Brams's *Rational Politics* requires something akin to a leap of faith. Nominally, both have an interest in explicating the process of political or, more generally, social choice. Practically, the books differ to the point of being noncomparable. Where Bonner's book easily stands alone as an

introductory text, Brams should ideally be read by someone with prior exposure to social choice and game theory.

Rational Politics holds, as its basic premise, that much observed political behavior can and should be explained as the result of a perfectly rational calculus on the part of politicians or other relevant actors. To illustrate this point, Brams uses elementary game theory to expose the rationale for the outcomes of prominent political confrontations (e.g., the Cuban missile crisis). As the book progresses the reader is presented with a succession of topics, including social choice theory, that relate to rational choice models of political behavior.

My concerns with Brams's book relate to elements of both the organization and substance of the material he presents. While making a strong argument for the use of a hypothetico-deductive approach to political analysis in his opening chapter, Brams proceeds to ignore his own advice in the organization of the remaining chapters. Deductive reasoning is fundamentally axiomatic and yet it is not until the fourth chapter that any effort is given to a rigorous analysis of individual preferences, an essential prerequisite for rational choice models. The absence of such analysis prior to the presentation of spatial games in the third chapter is disconcerting.

A related concern is with Brams's development of game theory. Again we note the absence of an initial overview of game theory principles. Instead, such principles are presented over the course of the book. As a consequence, the reader must wait, for example, until chapter 6 before being introduced to basic terminology such as *normal* and *extensive form* games. The inclusion of such relatively formal terminology suggests incorrectly that *Rational Politics* is meant to serve as an introduction to game theory in addition to its role as a general introduction to rational choice political models.

My most serious concern rests with the substance of the game theory that Brams presents. As a single precautionary example, consider Brams's characterization of single-period Nash games. To claim, as Brams does, that such games are inherently myopic testifies to a profound misunderstanding of their specification.

Rational Politics works well as a testimony to the broad applicability of rational choice political models. The reader is cautioned, how-

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ever, before using the book as an introduction to relevant methodology.

BRIAN E. ROBERTS

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Liberty, Market, and State: Political Economy in the 1980s. By James M. Buchanan (New York: New York University Press, 1986. ix, 278p. \$45.00).

James Buchanan is a brilliant, imaginative, and prolific scholar. He is especially adept at defining terms and constructing models that reach the conclusions prefigured by his conservative ideological position, a talent amply evident in this collection of papers about the 1980s. His work is motivated and channeled by his intense dislike of the twentieth-century liberal, welfare, interest group state and by his desire to restrict both the economic role of government and its intellectual rationale, particularly in what he perceives to be redistribution. His self-expressed desire is to create an intellectual fortress supportive of what he calls the minimalist, protective, or night watchman state. For Buchanan, traditional economics is too readily interventionist. He prefers a political economy led by his "public choice" theory in providing (in the phrase of his mentor, Frank Knight) a propaganda for economic freedom.

Buchanan would prefer a world in which (1) collective political action is constitutionally limited; (2) there is minimal legal change and that, especially involving redistribution, subject to a unanimity rule (even if only implicit or conceptual); (3) there is exercised a necessarily fundamental (however limited) economic role of government, which he recognizes will channel economic outcomes; (4) the minimalist state is given constitutional protection; and (5) the constitution is necessarily subject to reform (notably, in the present world, a balanced-budget amendment). He prefers, in short, a world in which ostensibly spontaneous—rather than political—order is achieved, largely through markets, albeit within an appropriate but stable legal and institutional framework which is itself not the continuing object of change by political entrepreneurs. These are the main themes that reverberate throughout these papers.

The book is divided into five parts, covering 24 papers that are diverse and wide-ranging and not readily summarized in a short review. The papers in part 1 treat alternative perspectives on social order. Here he surveys the development of the public choice perspective and its juxtaposition, as political economy, to mainstream economics. The most important papers, first, argue that insofar as politics is perceived as constituting a quest for truth there will be a potential for the tyranny of coercion and political absolutism; and, second, make the case against various opposition arguments to constitutional reform. A unifying theme is that in both markets and politics there is no independently determined or testable efficient or correct result independent of the determining process itself. Part 2 continues the theme of "order defined in the process of its emergence" (the title of chapter 7) and explores the processes of institutional reform, especially through politics; the irrelevance (contrary to Ronald Coase) of transactions cost; and the distinction, for Buchanan, between moral community, order, and anarchy. Part 3 presents the case for a contractarian minimization of the welfare state. Part 4 presents his case against government deficits and debt and for the balanced budget amendment. He laments the putative destruction of capital values and constraint of future choices. The emphasis is on deficit financing per se rather than the size of government. Part 5 presents informal models of individual and collective choice and of democracy, all in support of the minimization of the economic role of government and legal change. Among other things, he opposes social engineering and politics conducted as the search for truth and participated in by scientists (utilizing arbitrary social welfare functions or benefit-cost analysis) in the belief that they are politically relevant truth specialists. He affirms arrangements in which voluntary exchange can predominate.

Buchanan is to be applauded for certain points that other economists, as well as political scientists and others, would do well fully to appreciate and adopt: (1) that economic performance is inexorably governed by legal and other institutions, with performance specific thereto, and that legal rights of economic significance are logically prior to the market; (2) that there is no independent basis for determining maximization: that order,

utility functions, and economic performance are worked out through the processes of their determination and are not ontologically determined truths and that politics, like markets (themselves political institutions), is a valuation process, not the search for an independent truth; (3) that equal access to influence, open franchise, regular rotation of political agents, and avoidance of gross bundling of separate collective choices are useful for *ex ante* political equality; and (4) that much work by social scientists is a combination of preaching and social engineering, utilizing the mantle and forms of science for purposes of persuasion.

Buchanan's conservative position aside, there are numerous tensions within and problems with his arguments. While he opposes science as social control, his approach, he recognizes, also constitutes social control (p. 38). Why are his political entrepreneurship, his rational constructivism and his social engineering preferable to others? His answer would be that he wants to create institutions in which voluntary agreement predominates. But this is only a different agenda for government, and economic performance (including distribution) would be channeled by it. Individuals can always exercise voluntary (actually volitional) choice within the extant legal framework. He would argue that this is the minimalist state, but this is illusory: his state, for example, in its structuring of private power, is no less fundamental. He would minimize legal change by largely subjecting it to a unanimity rule. But it is illusory too to think that government is normatively important only in matters of legal change of legal rights. His minimal government prescription is itself but a strategem in the system of power play governing the use of government. Far from eliminating predatory politics, it would be its tool. Buchanan himself may be a romantic constructivist, perhaps even a moral anarchist (p. 119), with all the dangers of such positions.

Apropos of the view that status quo institutions and outcomes are efficient. Buchanan attempts to rebut the criticism thereof by emphasizing the possibility of institutional change. But using a greater-than-majority rule grants decisional power to a small majority and fortuitously operates in favor of the status quo (but not fortuitously for those to whom his ideas are gospel and useful). He wants to limit politics to mutual gains and does not

want to interpret politics exclusively as the clash of conflicting interests. But, as laudable as that may be, social and economic conflicts exist and to ignore them is to reinforce their established resolution. For all its unattractive features, politics is a conflict resolution process. That it deals with choices that involve mutually exclusive (rather than Pareto-optimal) results is due to the existence of conflict. Politics is due to conflict, not vice versa, although the form that the conflict takes is influenced by politics.

Buchanan in these matters may be normatively attractive but he is so often substantively narrow or wrong that one wonders about the degree to which both wishful thinking and his conservative ideology limit and channel his analysis. His analysis is both brilliant and (in the best high-priestly tradition) casuistic—casuistic, for example, in its rejection of preferences that do not accord with his. To give him credit, their holders could in principle veto constitutional change, but he does not recognize that constitution making is itself a vehicle of power play and that unanimity rules do not in practice apply to it. Indeed, he both denies and affirms the ideological stance of public choice theory, a motivation that is a strength and weakness both of Buchanan and of the theory itself. One weakness is his own brand of absolutism; one strength is the positive hypotheses to which his ideas have given rise. The great normative strength is that he is basically right regarding the freedom-enhancing nature of competitive markets (even though here, too, he equivocates, first rejecting the structural definition of competition and later invoking it). Moreover, the book is so heavily laden with wishful thinking in a corporate system, that the tendency is for his analysis to mask and obfuscate the reality of private power concentrations and their implications for "voluntary" (actually only volitional) agreement.

All in all, this volume is conservative polemics packaged in philosophy and social science, but with much good sense and suggestive analysis for positive work. The polemics are those of Ronald Reagan; for example, the idea of the budget balance constitutional amendment. Indeed, if a constitutional convention were called on behalf of this amendment, Buchanan may well be its intellectual or spiritual patron. If such a convention

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attempted major overhauling of the document of 1789, it would likely give constitutional effect to his emphasis on markets and to other items on the conservative agenda, probably without the restraint required by his affirmation that politics (even constitutions, which should deal with rules and not outcomes) is in search not of truth but of conflict resolution. Indeed, pursuit of the balanced budget amendment may be the trojan horse with which to gain the opportunity for larger revision.

WARREN J. SAMUELS

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Rivalry and Central Planning: The Socialist Calculation Debate Reconsidered. By Don Lavoie (New York: Cambridge University Press, 1985. 192p. \$34.50).

For neoclassical economists, the market socialist controversy is an important *shared example*, to use the terminology of Thomas Kuhn. When, in 1920, Ludwig von Mises argued that rational economic calculation was impossible under socialism he precipitated a 20-year exchange that now constitutes one of the few episodes in the history of economics discussed in standard textbooks. Reading that episode as customarily presented, the student learns that a socialist economy must solve the same basic problem as under capitalism. That problem is one of efficiently allocating resources to appropriate uses, given tastes and technology known to the pertinent agents. Therefore, to prove Mises wrong, all one need do is to imagine socialist managers imitating capitalist entrepreneurs and to envisage a central planning board mimicking the free market. This shared example teaches the budding economist to disregard the constraints imposed on economic life by institutions and to assume away all difficulties connected with the diffusion of information in society.

Reading Don Lavoie's *Rivalry and Central Planning*, one can clearly see why the market socialist debate is such an instructive example for the teaching of neoclassical economics. Lavoie shows how the arguments of Mises, Hayek, and their followers in the Austrian tradition were reinterpreted in a way that would make them easily answered within the

neoclassical framework. In this tradition, according to Hayek's famous 1945 *American Economic Review* article, "The peculiar character of the problem of a rational economic order is determined precisely by the fact that the knowledge of the circumstances of which we must make use never exists in concentrated or integrated form, but solely as the dispersed bits of incomplete and frequently contradictory knowledge which the separate individuals possess." The Austrian world is one of constant change, where new conditions arise every day and where the efficiency of the economic process is determined by the ability of the economic system to use the new information generated by the changing conditions. In contrast, in the economic models that are central to the neoclassical tradition, information is constant, already possessed by the relevant actors, and efficiency is defined relative to a given stock of information. Neoclassical economics simply could not be brought to bear on Mises' arguments concerning the relative efficiency of capitalism and socialism. The shared example not only instructs the student how to proceed but also gives guidance on which questions it is permissible to ignore.

Lavoie's book is the most complete record written to date of this debate. Although Lavoie writes from an avowedly Austrian perspective, his book is, with one important exception to be discussed later, a fair history of the debate. In addition to the historical record, the author provides a valuable discussion of the differences between the neoclassical and Austrian paradigms. The book is well written and the presentation is always clear. It can be highly recommended for all historians of economics, those interested in the theory of socialism, and those wishing to learn about Austrian economics.

Although recommending the book, I do believe that it has two important flaws, which some readers might find critical. First, Lavoie accepts too uncritically the Austrian view of capitalism—that unbridled entrepreneurs and unfettered markets will naturally produce the most efficient outcome. Work conducted within the Austrian paradigm may contain penetrating insights into the limitations of neoclassical theory, but that work does not constitute a rigorous alternative body of theory. Lavoie could certainly have subjected the arguments of Mises and Hayek to the same type of

penetrating criticism that he reserved for Lange and the neoclassicals.

Secondly, Lavoie's book is primarily a recapitulation and reinterpretation of the historical record, not an explanation of why the debate took the course that it did. The debate is presented purely as a clash of paradigms. There is no attempt to explain why the Austrian challenge was rejected by the overwhelming majority of economists. Underneath the surface of market socialist debate, there lies the fascinating question as to why the Austrian challenge was unsuccessful even though, as the Keynesian revolution attests, the profession was open to the consideration of new alternatives. Lavoie provides the raw material on which to build an answer to this question but not the answer itself.

Despite the reservations, the book is highly recommended. But I fear there is little chance that it will be read by economic theorists. As Whitehead once remarked, "A science that hesitates to forget its founders is lost." Economics knows its way and history does not lie on the route. This is unfortunate because Lavoie's book provides an instructive lesson for the present. In the early 1970s, there began to be much debate on the difficulties that informational issues presented for neoclassical economics. But at present these issues are being sidestepped. The advent of the rational expectations revolution once more means that economists are happy to assume that all agents are adequately informed. Any study of the process of information acquisition is eschewed. If economists were to read Lavoie's cautionary tale, perhaps they would be more prepared to ask themselves whether economics is still evading the informational issues that Mises raised over 60 years ago.

PETER MURRELL

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The Rhetoric of Economics. By Donald N. McCloskey (Madison: University of Wisconsin Press, 1985. xx, 209p. \$21.50).

Sure to be important in economics, this book should be read by political scientists interested in economics, in methodology, and in the philosophy of the social sciences. Yet the book's message and the uncommon grace, wit,

and learning of its argument are such that the readers who relish it most may be those who are most skeptical about economics, methodology, and scientism.

McCloskey dubs his *bête noire* "modernism," the belief that the only real knowledge is that tested by "scientific," rigorous skepticism. Modernism prizes only the observable, the quantifiable, the reproducible. It aims for prediction and control and would strictly separate scientific knowledge from oughts. McCloskey draws on Kuhn and others to show that modernism is not practiced in the natural sciences. He then examines the work of influential economists and finds that they persuade not through "official" modernist means but through an "unofficial" rhetoric that uses analogy, metaphor, appeals to authority, and other methods that Cartesian doctrine should not find persuasive at all. He finds that important techniques, like simulation, are about affirming, not about falsifying. He is especially telling in his criticism, "Unexamined Rhetoric of Economic Quantification" (pp. 138-173), arguing that statistical significance should be a minor tool in the economist's box. He also offers sensible suggestions about how to arrive at usable standards of economic significance.

McCloskey attacks not just modernist methodology but all rule-bound methodology. His quarrel is not with modest, small *m* methodology—statistical theory and practice, accounting conventions, and the like. And he joins Habermas in praise of the conversational norms of civilization: "don't lie," "pay attention," "be open-minded," "explain yourself when asked" (p. 24). The problem is with capital *M* Methodology that pretends to know the route to future progress: "the modern methodologist is a Red Queen (Normative argument: off with his head), and the Gods are snickering behind their hands" (p. 20).

For McCloskey, Habermas's conversational civility aside, "there are no rules and regulations for being reasonable" (p. 52). We should be more respectful of probable argument and plausible conclusions. The standards for good reasons, warrantable belief, and plausible conclusions come "from the conversations of practitioners themselves" (p. 29). And those conversations should be open to evidence from questionnaires, introspection, the humanities, and other sources scorned by the modernist in economics. "You are more strongly persuaded

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that it is wrong to murder than that inflation is always and everywhere a monetary phenomenon" (p. 45). The ethical arguments for such propositions are sometimes "more persuasive, better, more probable" (p. 46) than for scientific propositions.

To those who say the abandonment of modernism opens the door to irrationalism and the mob, McCloskey says to the contrary. The "lamentable split of fact and values" (p. 124) leaves the modernist believing that reason and science do not permit him to make arguments that speak to the mob's concerns. And in the world of science the "lamentable split" makes the scientist feel that it is not quite right to try to persuade. Thus "the Announcement, the more bald, unargued and authoritarian the better, is the favored form of scholarly communication" (p. 124). Since the results are supposedly objective and statistically significant, those not persuaded must be dishonest or ideological or stupid. Modernism does not combat dogmatism and venom; it nourishes it.

Though McCloskey is usually persuasive, his desire to tweak the modernist nose sometimes leads him to overemphasize the least scientific parts of rhetoric. When, for example, he suggests that economists should defend the metaphor of the selfishly economic man as calculating machine "because of its prominence in earlier economic poetry or because of its greater congruence with introspection than alternative metaphors" (p. 82), he must realize that such arguments will not persuade non-economists, nor should they. McCloskey's book focuses on economic rhetoric on the frontiers more than on why economists agree on much of importance. If, for example, he had set out to explain why a recent poll showed that only 2% of economists disagreed with the assertion that "a ceiling on rents reduces the quantity and quality of housing available" (J. R. Klear et al., "A Confusion of Economists?" *American Economic Review* 69[2]:28-37 [1979]), I think the importance for economics of theory, prediction and evidence would have been given their due along with the neglected forms of argument McCloskey rightly emphasizes.

McCloskey thinks economics a "successful" (p. 56) science, one whose "brilliance" deserves more influence in "civilization at large" (p. 180). But his criticism of his colleagues is merciless. The "typical" economist is "harshly

if unreflectively dogmatic" (p. 184). "Many are bored by history, disdainful of other social scientists, ignorant of their civilization, thoughtless in ethics and unreflective in method" (p. 7). The journals are filled with "pseudoscientific ceremony" (p. 159) and "exercises in deriving the obvious" (p. 179). Even one of the book's heroes, Robert Fogel, is praised for creating a rhetoric that brought into being a new type of reader, one "less attractive than the more common one in successful academic prose" (p. 135). And McCloskey says Fogel's rhetoric is similar to his own (p. 117)! A puzzle. One senses that McCloskey has the Habermas virtues, that he will be pleased by such questions and is intelligent enough to have some answers. We need more scholars like him and more books like this one.

STEVEN E. RHOADS

University of Virginia

The Economist's View of the World: Governments, Markets, and Public Policy. By Steven E. Rhoads (Cambridge: Cambridge University Press, 1985. xiv, 331p. \$39.50, cloth; \$12.95, paper).

The influence of economics in policy making inspires various combinations of wonder, envy, and disgust among some noneconomists. In this book, Professor Steven Rhoads assesses the strengths and weaknesses of micro-economic policy analysis, that is, the application of the economic theory of individual behavior and welfare to the problem of evaluating economic institutions and the policies that affect them. Along the way, he provides an excellent, detailed nontechnical explanation, with powerful examples, of some key ideas in modern economics, such as opportunity cost, externalities, the marginalist perspective, and the importance of the incentives that are created by different types of markets, organizations, and policy instruments. The book is rich in illustrations drawn from several areas of public policy, and especially good sections deal with the economics of environmental policy and income distribution policy.

The central mission of the book is to identify the weaknesses and limits of the use of economics in policy making. It considers many of

the criticisms of microeconomic analysis that have been voiced by economists and non-economists alike and in the end adopts a view that is more sanguine than that of most of the critics, as, for example, exemplified by Steven Kelman. Nevertheless, Rhoads strongly attacks several aspects of economics: the doctrine of consumer sovereignty (the presumption that individual preferences are normatively compelling), the self-interest axiom (individual behavior is motivated largely by selfish objectives), and the reigning methodology (heavy reliance on sophisticated mathematical and statistical tools to the exclusion of moral philosophy, history, and hands-on, real-world experience, as through field work).

In my view the clearest weaknesses of the book stem from its central conception, which is to deal with economists as a group of people holding shared and distinctive values, rather than with the discipline of economics as a universally applicable theory of human behavior. This approach produces several highly dubious inferences and conclusions. First, it leads to regarding anything written by any economist that is not a critique of the discipline as an expression of shared beliefs, no matter how ridiculous and ethically impoverished its content. Thus, because one economist once argued the point, all economists are held to believe that dueling ought to be legalized (because people duel voluntarily and there are no externalities to duels), that the direct net benefits of stealing money are zero (because the gains to the thief exactly equal the loss of the victim), and that the willingness to pay for addictions has the same normative content as the willingness to pay for prosaic goods like food and shelter.

Once one adopts this point of view, the foolishness of economics requires a "theory of economists" to explain how they could be so myopic. Rhoads offers the view that economics attracts Philistines and materialists because its content focuses on markets and financial matters. He does not tell us why this is more plausible than the view that at least a few people enter the profession because they want to use the powerful force of economic incentives, as vividly described in the book, to make meaningful progress in solving environmental problems or in improving the lot of the materially deprived. In fact, until about 1970, economists were more likely to be criticized for

being iconoclastic liberals than for exhibiting conservative materialism, and one wag advised that the route to success in economics was to travel to Cambridge and turn left.

Political scientists are likely to find especially interesting the final section of the book that deals with the application of microeconomics to the study of political processes. Here the reader should be warned that Rhoads's characterization of the economics literature is idiosyncratic, for his coverage is limited essentially to the public choice school of James Buchanan and Gordon Tullock. While major figures in the economics literature try to develop a positive theory of government, I find it quite misleading to place such heavy stress on their work while all but ignoring, say, Kenneth Arrow, who not only approaches the subject in a far different way, but who also won a Nobel prize for this work. Even more misleading is to weave together the positive economic theory of Buchanan and Tullock, and, to a lesser extent, George Stigler, with their political philosophy. The first error is that their philosophy is more conservative than most of the writings on the topic that are uncited, but the bigger error is the implication that conservative political philosophy is inextricably linked to an economic theory of politics. The book elsewhere contains some of the evidence on these points in its summary of the economic literature on environmental policy and income redistribution, much of which can hardly be classified as politically conservative.

In discussing the positive economic theory of political behavior, Rhoads takes issue with the core assumptions that politicians attempt to maximize the probability that they will hold office, that political participation is motivated by self-interest, and that one should be highly skeptical of assigning benefits to actions that attempt to impose one person's preferences on another. All these issues need to be confronted, of course, but the argument in the book is incomplete in some respects. Although Rhoads is correct in identifying consumer sovereignty as the soft underbelly of benefit-cost analyses that are based on the willingness-to-pay of citizens, the book does not explore whether this has any implications at all for political decision processes. The core issues are whether consumer preferences are manipulable and whether a society that bases public decisions on selfish preferences thereby must

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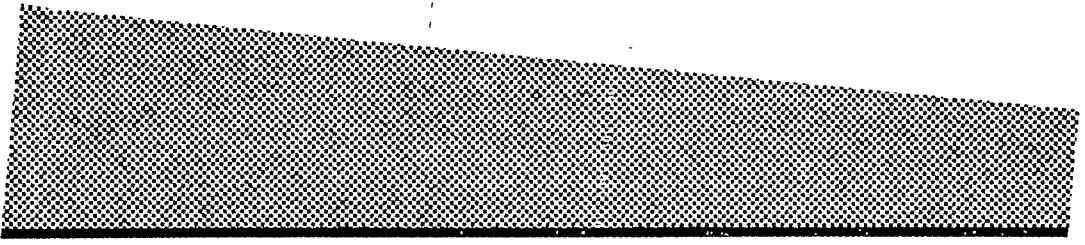
encourage materialism among its citizens. The problems seem obvious. Political preferences, too, may be manipulable. And political actors, not economists, decided to institutionalize economics into government decision processes, which some might see as an *effect* of the materialistic nature of a society, rather than a *cause*. In any case, the extensive analysis of how government officials ought to make decisions serves to confuse the positive and normative aspects of economics. Benefit-cost analysis is normative, in that it leads to the recommendation of one course of action over another. The economic theory of political behavior is positive, in that it yields testable conclusions about the effects on public policy of changes in the economic environment and the political institutions that make policy. It is, of course, quite incorrect to say that the perspective *recommends* that politicians maximize reelection prospects or that voters evaluate can-

dicates on the basis of the personal benefits the candidate will provide them.

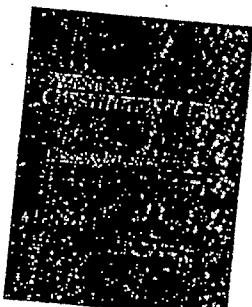
While most any economist will certainly find these and other faults in Rhoads's work, the book nevertheless is a valiant attempt to attack a difficult issue. It is tough enough to come to a deep understanding of another discipline, but to have something sensible to say about its strengths and weaknesses is a herculean challenge. And many of the ideas and observations in the book are quite penetrating. It is definitely a book that a person who tries seriously to undertake multidisciplinary work between economics and political science ought to confront, for it usefully indicates the many ways one can display narrowness or overlook important causal forces in such endeavors.

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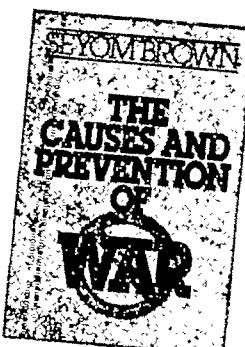
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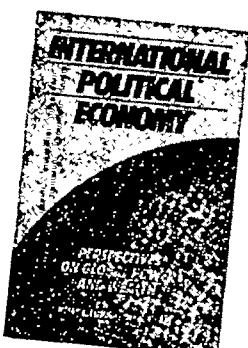


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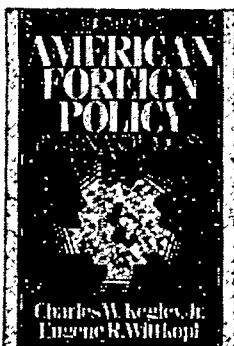


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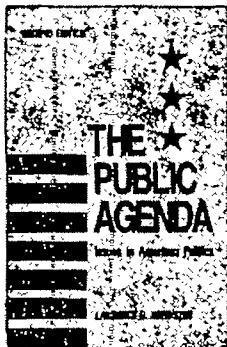
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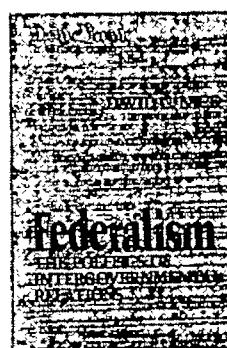


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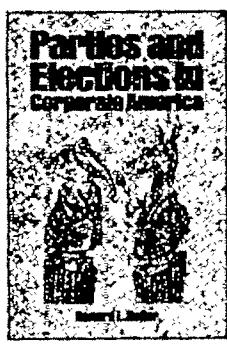


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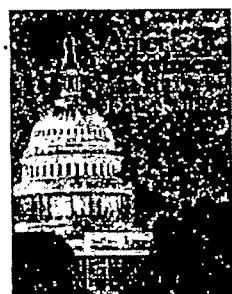
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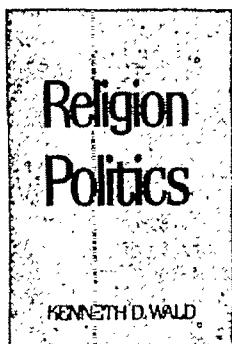
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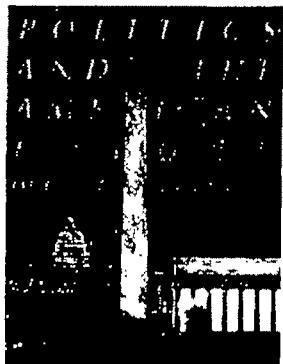
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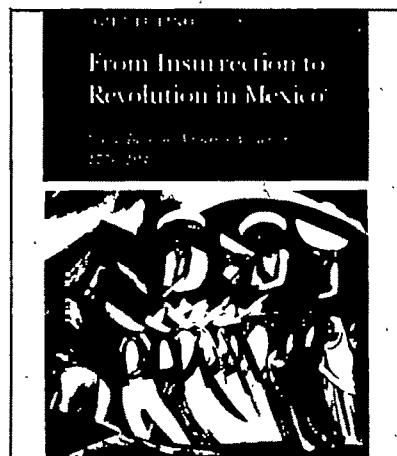
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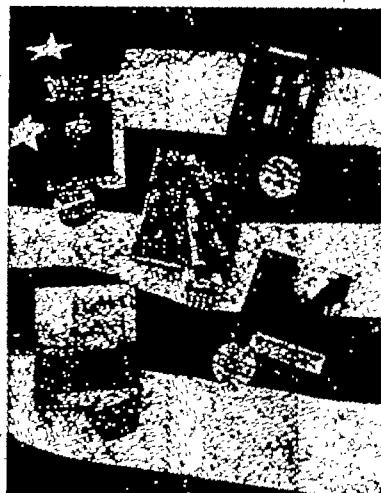
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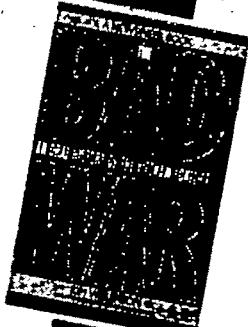


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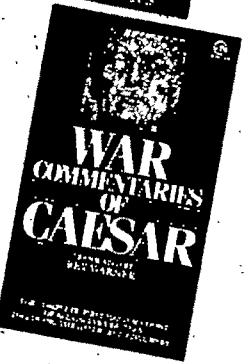
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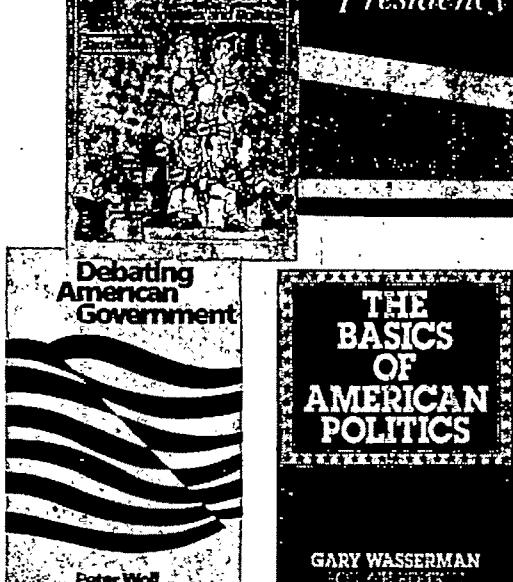
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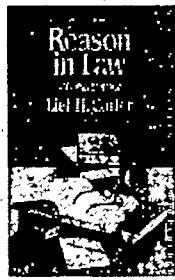
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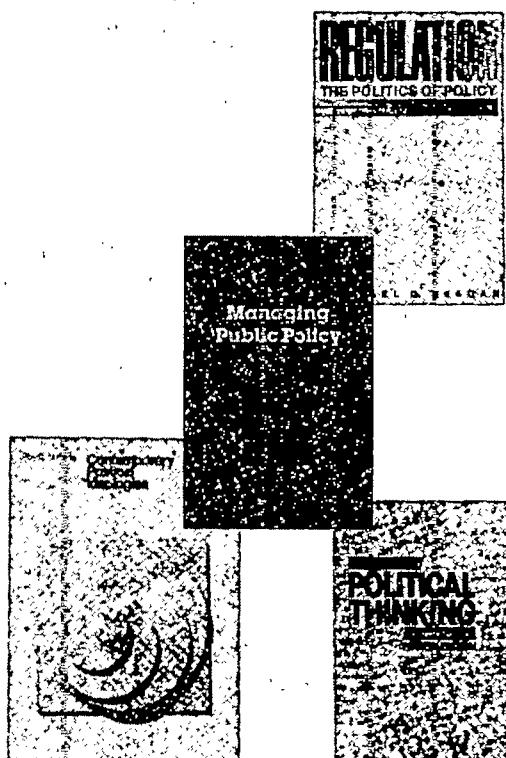
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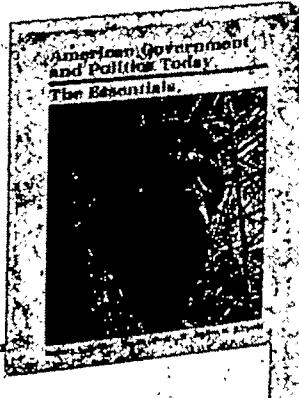
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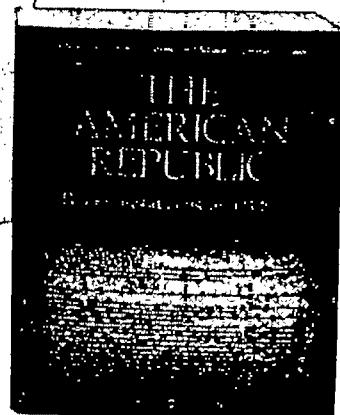
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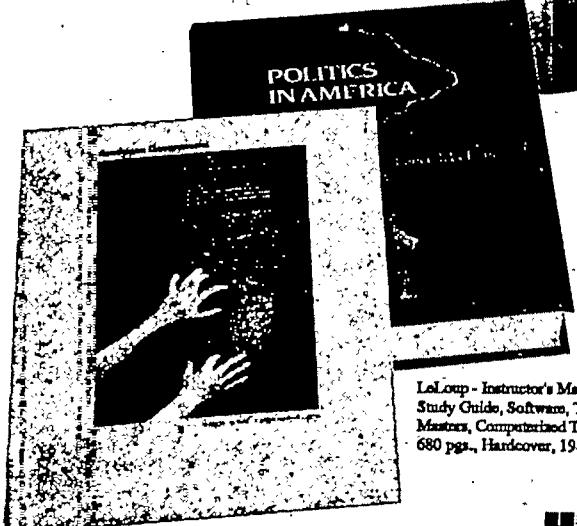
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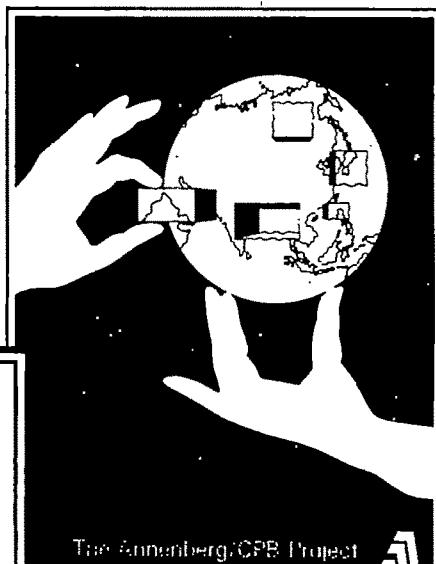
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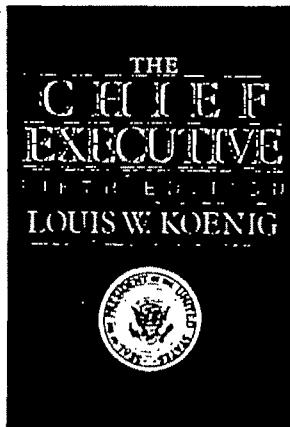
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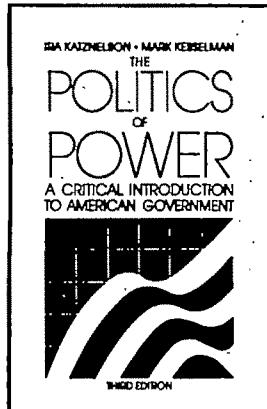
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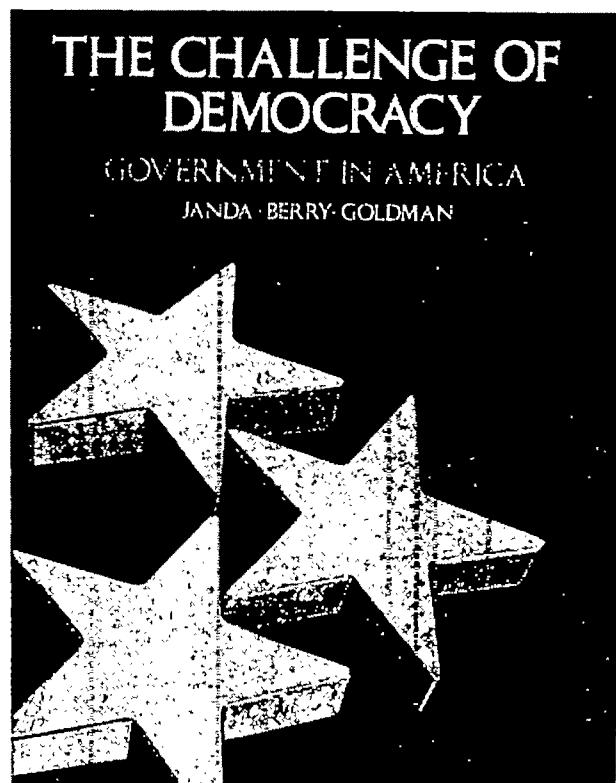
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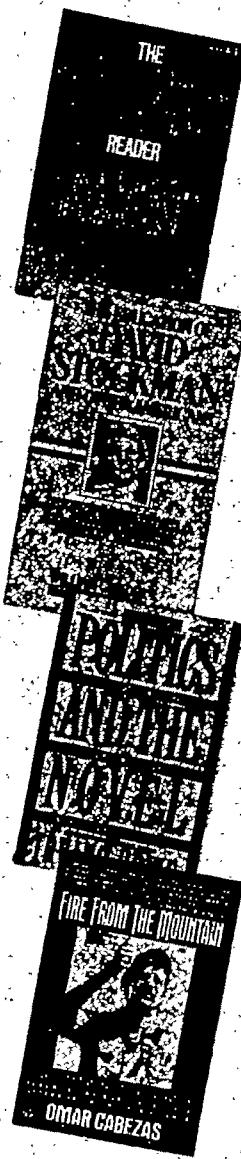
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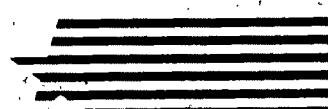
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AMERICAN POLITICAL SCIENCE REVIEW

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ARTICLES

INTIMIDATION AND THE SYMBOLIC USES OF TERROR IN THE USSR

DONNA BAHRY
New York University

BRIAN D. SILVER
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Theories of regime-society relations in Communist states stress the central role of coercion in maintaining political control. Based on a survey of Soviet emigrants, we examine whether Soviet citizens are deterred from nonconformity by the punitive actions of the KGB (individual deterrence), a perception of the KGB's coercive potential (general deterrence), or mistrust of other people. We find that few respondents were directly coerced by the KGB (and those who were had engaged in the most serious kinds of nonconformity); that those who had punitive contacts with the KGB in the past were not deterred from subsequent nonconformity; that the KGB's competent image was a general deterrent; and that trust in other people facilitated both nonconformist and compliant political activism. Those who came of political age under Khrushchev and Brezhnev were more likely to be involved in both kinds of activism than those who came of age under Stalin.

The feature of the Soviet political system that is most often said to distinguish it from industrial democracies is the use of state terror—physical coercion or the threat of it—to assure compliance with the regime and to remake the nature of man. In his characterization of the Soviet system under Stalin, Merle Fainsod wrote, "Terror is the lynchpin of modern totalitarianism" (1961, 384). In their classic book, *Totalitarian Dictatorship and Autocracy*, Carl Friedrich and Zbigniew Brzezinski (1965) described a terrorist police as one of six key features of the "totalitarian syndrome." Totalitarian states are said to atomize society so that people become isolated and mistrustful of one another and hence unable to concert their efforts in organized political activity (cf. Kornhauser 1959). They fear the *stukach* (stool pigeon) and the possibility that even an

unguarded or innocent remark could be used against them.

The actual use of state-sponsored terror as an instrument of control has unquestionably decreased, however, since Stalin's time. The reduction is reflected not only in the decline in the use of physical force for political ends but also in reduced autonomy and authority of the secret police agency itself. Even though in the late 1970s the status of the KGB (*Komitet gosudarstvennoi bezopasnosti*, "Committee on State Security") was upgraded in visible ways and the number of dissidents arrested increased (Knight 1980; Reddaway 1983), overt mass terror is much less common now than in Stalin's time.

Whether a Soviet system with limited overt use of terror should be classified as something other than totalitarian has exercised the minds of many scholars.

Although few question that the Soviet state has reduced its overt use of force for political ends since Stalin's day, most recognize that the main instruments of political control remain in place. For example, Bialer (1986) has argued that there are three main reasons why the Soviet system survived the abolition of terror: the inhibiting effect of the memory of the Great Terror among the generations brought up under Stalin; the maintenance of a political police that uses more measured and predictable, but nonetheless effective, measures of control; and the use of additional means of nurturing mass support, such as material incentives.

This suggests that it is the style of coercion that has changed in the Soviet Union, not the essential commitment to use it to enforce political orthodoxy. The argument is not simply that the Communist party has cultivated other means of generating mass compliance, such as material incentives, but that a mature coercive regime does not need to engage in constant harassment of every person who strays from the orthodox line. It can tolerate unorthodox thoughts and a certain amount of unorthodox behavior, while resorting to coercion only when an unorthodox activity intensifies or challenges the authority of the regime.

More important, an efficient system of controls can rely on a perception of its coercive potential to inhibit deviation from prescribed political norms. As Jeremy Azrael has observed, in the post-Stalin era more of the activities of the secret police are "designed to have a 'prophylactic' instead of a 'punitive' effect" (1962, 10). From this perspective, it is important that the state be perceived as having the capability and will to uncover and to act against dissident or unorthodox activity, whether or not it actually does so in every instance.¹

An equally important instrument of collective coercion against the individual according to the totalitarian model is the

individual's lack of trust in others, a sense that risks lurk not only in the overt activities of the agencies of coercion but also in one's most ordinary contacts with co-workers, bosses, friends, and even relatives. It is this set of perceptions—of the coercive potential of the political police and of other people—that we call the *intimidation factor*.

We use information provided by former Soviet citizens to study both perceptions of the KGB and interpersonal trust and to test propositions about the relation between these perceptions and political behavior. Did the experience of direct intimidation by the KGB deter the individual from engaging in subsequent unorthodox behavior? What is the KGB's image? Is the KGB held in higher regard than other Soviet institutions? How prevalent is social atomization? Were those who were less trusting of others likely to take fewer chances and to engage less in behavior that the regime considers unorthodox?

We focus on the KGB as the primary agency of coercion in the USSR. Other institutions, of course, also play a role in intimidation. We study the image of the KGB because it represents the most severe coercive agency. Those who are not intimidated by the secret police are unlikely to be intimidated by other, less coercive institutions.

The Soviet Interview Project General Survey

The data for this study come from the Soviet Interview Project (SIP) general survey of 2,793 former Soviet citizens. Most of the questions on the survey centered on the respondents' "last period of normal life" (LNP) in the USSR—the five years prior to the major disruption associated with their decision to emigrate. For most survey respondents, this was when Leonid Brezhnev was general secretary of

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the Communist party and Yuri Andropov was the head of the KGB.² The respondents arrived in the United States between 1 January 1979 and 30 April 1982 and were between the ages of 21 and 70 on the date of their arrival. They were interviewed between March 1983 and January 1984.³

Can the results of the SIP general survey yield valid insights about mass politics in the USSR? We would not suggest that either the overall distributions or summary statistics from a survey of Soviet emigrants be generalized directly to the Soviet population, especially because the respondents come overwhelmingly from large cities and are (by definition) emigrants and because about 85% of them are Jewish by some definition.

Our concern, however, is with the pattern of relations among variables, such as the relation between people's image of the KGB and their compliance with the Soviet model of a politically mobilized citizen (cf. Zimmerman 1987). And our ability to generalize is limited to the Soviet adult population from large Soviet cities, the "referent Soviet population" (Anderson and Silver 1987a). Previous research using SIP data confirms that such findings do reflect Soviet reality (Bahry 1987). For instance, the connections between socio-economic characteristics and political activity essentially match those reported in Soviet surveys. So, too, does the pattern of early recruitment into the Komsomol for different generations and social groups. Friedgut's (1979) study based on Soviet emigrants supports a similar conclusion.

In addition, previous research shows that most of the responses to questions on political attitudes and behavior are not sensitive to whether the respondents were Jews or to whether they had especially traumatic experiences associated with emigration. Thus, although one can never prove that all the patterns in the responses

reflect the attitudes and experiences of the referent Soviet population, the results are not sensitive to some of the obvious candidate sources of bias.⁴

Theoretical Background

Conceptual Insights from the Theory of Deterrence

The literature on deterrence, particularly as it has been applied to understanding law enforcement, provides a useful conceptual framework for understanding the effects one should expect from government coercion, though we do not wish to press the analogy between ordinary crime control and the activities of political police.⁵

When agencies of the state resort to force against people, their usual immediate objective is to suppress the activities themselves. By arresting or punishing people, they try also to affect their future behavior or that of others. First, they can *incapacitate* people—remove them from society—by detaining, imprisoning, exiling, or killing them. Second, they can seek *individual deterrence*, by retaliating against nonconformists and making them hesitant in the future to repeat the same or other undesirable actions or by inducing them to behave in more appropriate ways in future. Third, they can try to foster *general deterrence*, by making other people hesitant to become involved in undesirable activities. General deterrence depends on shaping peoples' perceptions of what could happen to them if they were to overstep the boundaries of acceptable behavior.

Individual Deterrence. The shift in the KGB's role in Soviet society from the 1930s to the present involves a shift from relying heavily on incapacitation to relying more on individual and general deterrence. The literature on law enforcement suggests that the individual is deterred

from straying because of both the certainty and the severity of punishment. In general, however, high certainty of punishment is a more effective deterrent than high severity of punishment. The logic, supported by some empirical evidence, is that increasing the penalty is not likely to deter deviant behavior unless people think they are likely to be caught (see Piliavin, et al. 1986).

By this same logic, the KGB does not need special tribunals and a vast archipelago of labor camps to be an effective political police. But it does need to make people believe that if they do step out of line, retribution will be nearly certain. This logic applies to both individual and general deterrence.

General Deterrence. For general deterrence to work, the agencies of coercion must appear to be effective and the punishment meted out must be visible, publicized. Or the memory of past activities of the agencies must be strong. A perception that some activity might be risky is only a deterrent if reprisal is very likely.

Evidence among SIP general-survey respondents supports this distinction. Often, people who were aware of the risks of engaging in unconventional behavior nonetheless did it. For example, 30% of the respondents reported that they read *samizdat* or *tamizdat* (unauthorized materials in the USSR);⁶ of those who did not read it, less than 1% refrained from doing so explicitly because it was too risky. On the other hand, of the 30% who did read *samizdat*, 84% thought it was risky to do so, and 40% of the latter classified it as very risky.

In fact, however, only 1 of the 280 respondents who claimed to have read *samizdat* during their LNP reported being punished for doing so. Thus, this form of unconventional behavior was not deterred by the perception of risk alone. Those who did not engage in it seldom volunteered its riskiness as a reason; they

were much more likely to mention that *samizdat* was not available or that they were not interested in it. At the same time, those who did engage in the behavior did so despite perceiving the potential hazards.

The relation between the perception of risk and type of *samizdat* that people read is instructive. Those who read literature about human rights and the regime's political abuses regarded the reading of *samizdat-tamizdat* as much riskier than those who read nonpolitical materials, even though punishment was rare in both cases. Ironically, politically oriented materials may send an unintended message that reinforces the image of the potency and ruthlessness of coercive agencies. Materials that chronicle state abuses remind readers of the state's power and the heavy costs to be paid by those who fail to comply. And articles and books critical of the vast majority of compliant citizens tacitly underscore the social isolation of political dissidents (Jahoda and Cook 1954).

Random Terror. Scholars writing on terror in the USSR have emphasized the importance of random terror as a method of totalitarian control. According to this idea, it is not the predictability and certainty of punishment that deters people from engaging in unconventional behavior, but instead its unpredictability and uncertainty. Brzezinski puts it thus: "It is this selective, unfathomable character of the purge which gives it its effectiveness" (1956, 28). Alexander Dallin and George Breslauer note that "it is this element of arbitrariness—or, from the vantage point of the citizen, the unpredictability—in the use of terror that is [terror's] distinguishing mark" (1970, 4).

We think the appearance of randomness or arbitrariness with respect to the targets of terror is most effective at given stages in the development of a regime, in what Dallin and Breslauer (1970) refer to

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as the *takeover* and *mobilization* stages. Random terror is especially relevant to a regime that is committed to undermining the established social order. As Barrington Moore has observed, "Capricious or arbitrary use of terror undermines the essential basis of social organization" (1954, 170).

At a later stage of development of the regime, however, not only are other mechanisms of achieving compliance available, but utter arbitrariness in the application of coercion punishes people who are supportive of the system. Furthermore, it is not efficient for the police either to try to achieve 100% compliance or to punish every transgressor of official norms.

An analogous argument has been presented in the context of the strategy of two-person games. Assuming that agencies of coercion must be selective in the application of coercion, it is better to be unpredictable than not.⁷ In this way, it is more difficult for those who wish to violate the established norms to work the system to their own advantage, like knowing when the cop on the beat makes his rounds. Under uncertainty, the prospective wrongdoer will be deterred from acting because the risk of punishment becomes incalculable.

The usual objective of the person who threatens coercion is to modify the behavior of the other person in a specific way, not to terrorize him into total inaction. For this reason, enforcement should not be random or unpredictable with respect to the types of behavior that are sanctioned. That is, which individuals will be caught and punished may not be highly predictable; but what kinds of activities will be punished should be more certain.

Following this line of argument, one would not expect to find certain and severe punishment of all transgressors of official Soviet political norms. Nor would the lack of universal and harsh enforcement of official norms of behavior be a

sign of a lack of capability or will of the political elite. But one would expect actions that are more threatening to the regime to be more consistently punished than those that are not. And leaders of unsanctioned activities should be more likely to be punished than those who are only participants.

Social Atomization. A related component of deterrence that has been especially emphasized in theories of totalitarian control is the deliberate undermining of social bonds among individuals. Society itself thereby becomes an instrument of coercion: the memory of mass terror, the elimination of autonomous intermediary groups between state and individual, and the continued reliance on informers breed an atmosphere of social intimidation that undermines any collective activity not officially sanctioned by the state. Moore outlined the process well:

The regime deliberately seeks to sow suspicion among the population, which to a marked extent results in the breakup of friendship groupings, in the work situation and elsewhere, and the isolation of the individual. . . . Terror ultimately destroys the network of stable expectations concerning what other people will do that lie at the core of any set of organized human relationships. Dissent may thus be deterred not so much by the direct threat of punishment by the KGB (although it looms in the background) but by a generalized fear or mistrust of others, a lack of faith in people (1954, 158, 164).

Thus, based on the scholarly literature on deterrence and on terror in totalitarian systems, there is reason to ask both when and how coercion comes into play, and whether the actual or expected application of negative sanctions works as either an individual or a general deterrent. How likely is it that people will in fact be punished? And does punishment or the anticipation of punishment actually deter people from political nonconformity?

Results

Is Coercion Random?

Previous research based on SIP general-survey data has shown that only a small proportion of the respondents engaged in overt political nonconformity.⁸ By and large, they were quite conventional, judging by both the standards established by the Soviet government and the patterns of political participation found in other developed countries.

Because of the small number of respondents to the survey who engaged in specific unconventional political activities, we must be cautious about interpreting the patterns of regime response to each kind of noncompliance. In addition, we do not have information about the frequency with which individuals engaged in each of the behaviors. Usually, we know whether they engaged in different types of behavior in their LNP, but not how often.

We have better evidence about the level of involvement of the individual in various kinds of unconventional activity, that is, whether the respondents were leaders in the activity or were only participants in it. And for those activities, we can assess whether involvement led to punishment by the authorities.

As shown in the top panel of Table 1, those who were leaders of unconventional activities were consistently more likely to be punished for their activity than those who were merely participants. Table 2 shows that, of all nonconformist leaders, 50% were punished.⁹ The probability that they were punished increased with the variety of activities that they led. In contrast, of all those who were participants (but not leaders) in the same kinds of activities, only 7% were punished, though the proportion penalized also increased with the number of different types of activity in which people participated. Reprisals were especially likely for those who engaged in more serious,

public threats to the regime, for example, strikes and overt protests.

From this evidence, one would conclude that punitive action by Soviet authorities is very predictable.¹⁰ The question remains, however, whether those who were not involved in overt unconventional behavior during the LNP were also targets of the KGB. If the use of coercion were random with respect to the kinds of activities in which people engaged, the secret police would have intervened directly in the lives of many people who did not knowingly engage in any overt unconventional behavior.

Only 2% ($N = 58$) of the SIP general-survey respondents reported such contact with the KGB during their LNP.¹¹ One-fourth of this small group were asked by the KGB to help it conduct its own business: the KGB solicited the respondents as witnesses in an investigation or asked them to provide information about other people. Other encounters with the KGB related to the respondent's activities: 24% concerned contact with foreigners; 17%, illegal economic activities; 12%, other restrictions on the respondents' freedom of speech or movement (e.g., the respondents' reading of *samizdat*, complaining to the government, "saying the wrong thing," visiting a place where they were forbidden to go); 5%, ethnic reasons or engaging in religious observances; and 17%, unidentified reasons.

Direct contact with the KGB was therefore minimal for those who refrained from unconventional activities in the LNP.¹² Not surprisingly, the KGB was far more likely to intervene in the lives of those who publicly threatened the established political order: For example, out of approximately 20% of all respondents who reported not voting during their LNP, the only ones punished for it were individuals who also took part in overt unconventional activity.

A similar pattern holds with respect to

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regime responses to individual demands and complaints (see Table 1). The vast majority of citizen-initiated contacts with Party and state agencies, mostly concerned with pocketbook issues and public services, evoked either a positive response

or no response from the agency contacted. Only a handful led to reprisals, and these were for contacts that were more overtly political, such as complaints about the state itself or its handling of citizen rights.

**Table 1. Punishment by Authorities for Political Activities in the LNP,
by Type of Activity**

Activities	Percentage Punished ^a	Base Number of Cases
Unconventional political activities		
Unofficial study group		
Participant	4.2	166
Leader	18.8	16
Distribution of <i>samizdat</i>		
Participant	8.8	102
Leader	23.1	13
Overt protest		
Participant	23.1	26
Leader	100.0	5
Strike		
Participant	13.0	23
Leader	55.6	18
Other self-defined activity ^b		
Participant	16.7	6
Leader	64.3	14
Other unconventional activities		
Nonvoting		
Sometimes	1.6	506
Always	1.7	292
Attending religious services	1.1	888
Read <i>samizdat-tamizdat</i> ^c	.4	262
Citizen-initiated contact with authorities^d		
Contacted government or party official		
Material reason	2.4	405
Administrative, civil matter	2.8	69
Protest	28.0	25
Contacted media		
Material reason	5.1	59
Administrative, civil matter	.0	8
Protest	27.8	18

^aPunishment is any official action taken against the respondent for the particular activity the respondent reports.

^b"Other" unconventional activities are defined by respondent. The most frequently mentioned were teaching/study of Hebrew/Yiddish; involvement with dissident movement; and illegal economic activity.

^cThe question about reading of *samizdat* or *tamizdat* was asked only of a random one-third of the respondents.

^dThese items included citizen-initiated contacts of all types, and are recoded by the authors into the categories listed. Many such contacts evoked positive responses, and many evoked no response.

Table 2. Punishment by Authorities, by Intensity of Involvement in Politically Nonconformist Activities

Number of Activities	Participant		Leader	
	Percentage Punished ^a	Base Number of Cases	Percentage Punished ^a	Base Number of Cases
1	5.7	176	44.7	38
2	16.4	55	50.0	8
3	38.9	18	100.0	3
4	20.0	5	100.0	1
5	50.0	2	—	0
1 or more	7.2	256	50.0	50

Note: *Political nonconformity* includes participation during the last normal period in an unofficial study group, distribution of *samizdat*, overt protest, strike at work, or any other unsanctioned political activity as defined by the respondent.

^aIn order for a person to be *punished* for any of the activities defined here, he or she must have been either a participant or a leader. Hence, no individual in this tabulation can have engaged in 0 unsanctioned activities.

Individual Deterrence

Since most questions in the SIP general survey focused on events and attitudes during the last five years of a respondent's life in the USSR before it was disrupted by plans to emigrate, it is difficult to determine whether people who were punished for unconventional behavior were deterred from doing it again. Fortunately, we do have some critical additional information: whether respondents had been contacted by the KGB earlier in their lives (before the beginning of the LNP), and whether they had a prior history of family arrest (whether someone in their family was arrested before the respondent ever got into trouble for some activity during the LNP). We would expect that if individual deterrence comes into play, those with early contact or family arrest should be less involved in nonconformity.

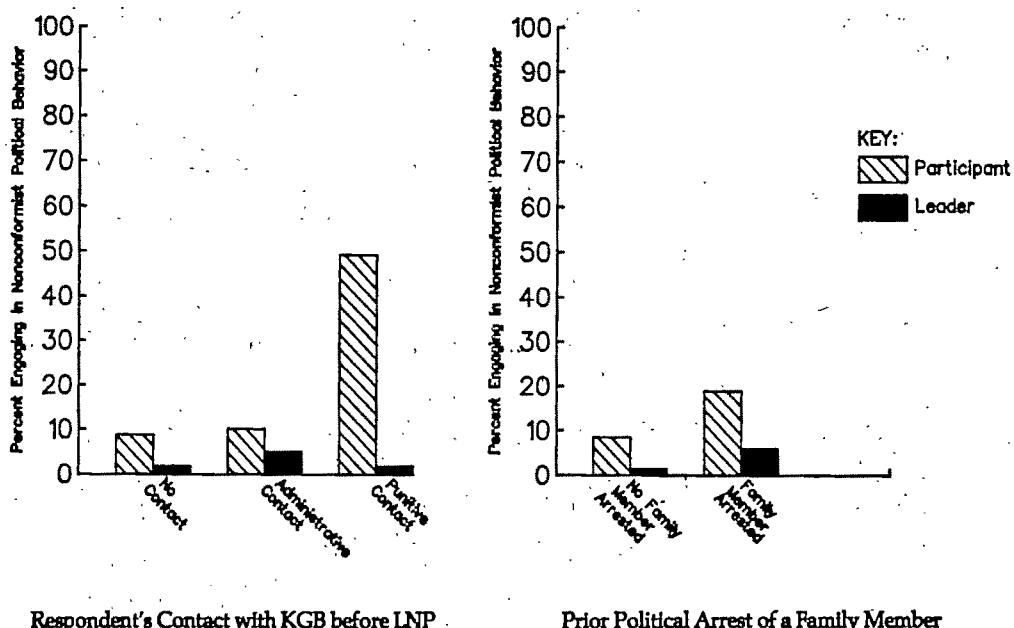
The data in Figure 1 reveal that such earlier experiences did not necessarily deter further involvement. Individuals penalized earlier by the KGB were almost four times as likely as other respondents to engage in unorthodox political activity during the LNP, although they were not more likely to be leaders.¹³ And those

who came from families that experienced a political arrest turned out to be twice as likely as others to engage in unconventional activity during the LNP. Evidence on the number of family members arrested provides additional support for this conclusion: the higher the number, the more likely the respondents themselves were to engage in unorthodox political activity (the data are not shown here).

Two explanations for this pattern seem plausible. First, whatever it is that impels people to engage in unorthodox political activity does not necessarily disappear once the person has been punished by the regime. Calling unorthodox political actors to account may be punitive but not rehabilitative or corrective.¹⁴ Second, once individuals are branded as troublemakers, their opportunities for political redemption may be limited. They face continuing extra scrutiny by persons in authority at work, by neighbors, and by social organizations. They may be isolated from and ostracized by others who do not want to be judged guilty by association. At the same time, they may receive social support from others who are similarly stigmatized by the authorities as political deviants, and perhaps

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Figure 1. Relation between Early KGB Contact and Political Nonconformity



Respondent's Contact with KGB before LNP

Prior Political Arrest of a Family Member

from Western news media, intellectuals, or politicians.

This does not mean that being punished makes someone more likely than before to engage in unconventional political activity. But the balance of incentives and disincentives to be compliant or nonconformist shifts once the person is identified by the authorities as a nonconformist.

Perceptions of the KGB: Competence, Honesty, and Other Dimensions

The analysis to this point shows that individual deterrence does not necessarily work. For the majority of SIP respondents, however, who did not have direct contact with the KGB, the effect of the KGB on political behavior may depend more on the KGB's image of competence. As Moore has written, knowledge of how many people the regime has arrested is not the only factor shaping compliance

and nonconformity: "Particularly in the case of such a phenomenon as terror, popular beliefs about its impact are likely to be more influential in determining actual conduct than the objective facts themselves" (1954, 156).

Accordingly, we now examine two dimensions of the intimidation factor that may serve as a general deterrent on unorthodox behavior: perceptions of the competence of the KGB and of the trustworthiness of other people. A KGB that is perceived as competent is likely to be more of a general deterrent than one perceived as incompetent. People who do not trust one another are also more likely to be deterred from unorthodox behavior than people who have greater faith in other people. In the final section of the analysis, we will test for the effects of these perceptions on the incidence of unorthodox behavior.

Competence and Honesty. Judging from

answers to questions about the competence of leaders of eight key Soviet institutions, the KGB is viewed as highly capable: 57% of all respondents said that almost all or most of the KGB's leaders were competent (*kompetentnyi*) or did their jobs well (see Figure 2).¹⁵ The KGB ranked third in competence among the eight institutions. The highest ratings were given to the leaders of the Academy

of Sciences and of the military; 66% of all respondents stated that almost all or most of the leaders of these institutions were competent.

Industrial-enterprise managers were rated fourth, the Politburo fifth. The leaders of local political institutions—the *militsiya* (local police), the local party, and the local government council—were rated much lower. Thus, the responses to

Figure 2. Perceptions of Competency of Leaders in Major Soviet Institutions

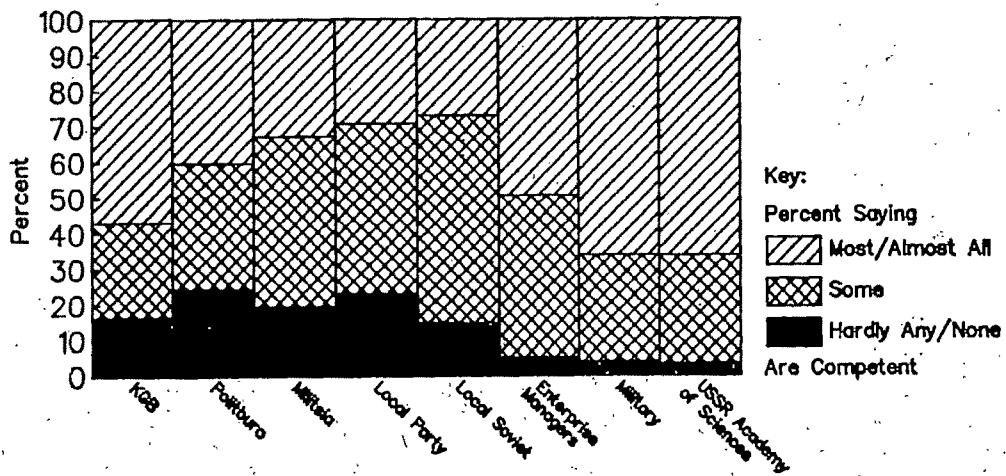
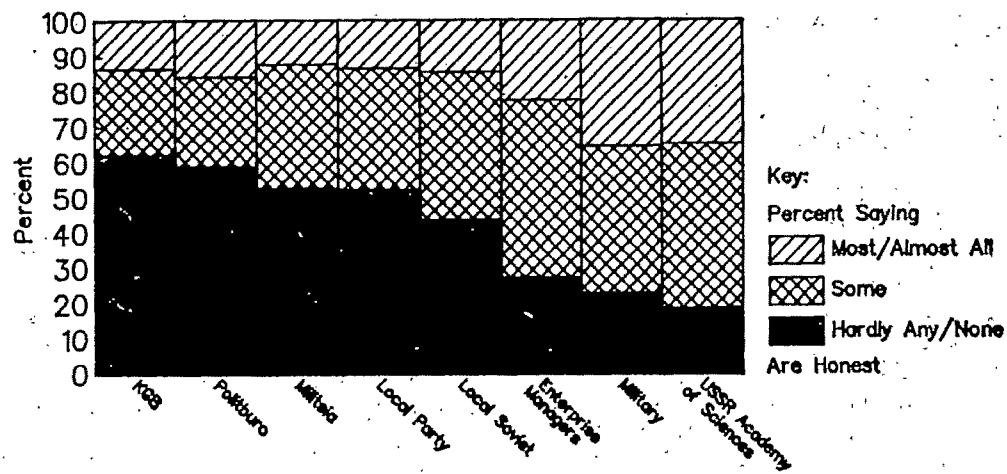


Figure 3. Perceptions of Honesty of Leaders in Major Soviet Institutions



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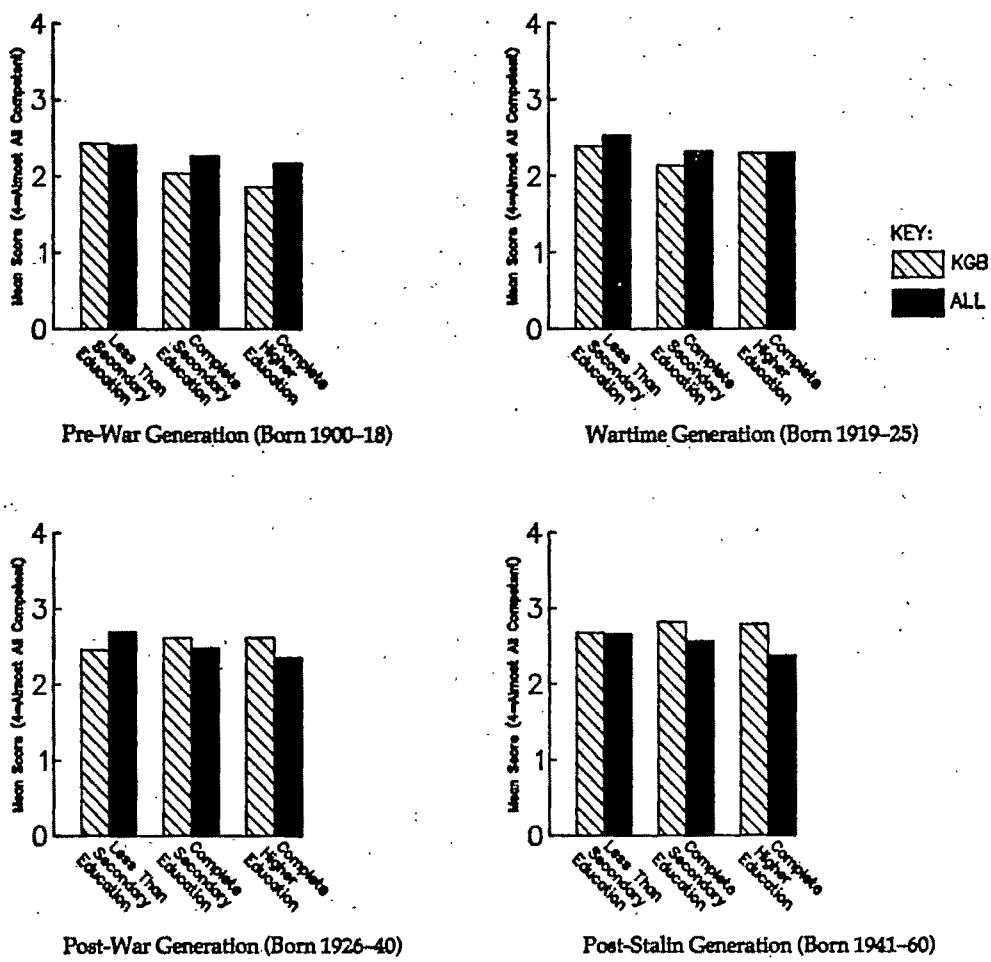
the questions on competence vary considerably, and within the range of variation, the KGB was rated quite highly.

It is possible that the answers to the questions about the competence of Soviet officials reflect a more general evaluation rather than an evaluation of competence per se. To determine whether this was so, we examined responses to questions about how honest or honorable (*chestnyi*) the leaders of the eight institutions were. If the responses to the questions about competence and honesty were substantially

different, then we could be more confident that the responses to the questions about competence are judgments of competence rather than of more general qualities of the institutional leaders.

When the questions related to honesty, the leaders of major Soviet institutions were judged much more negatively (compare Figures 2 and 3). The gap between the average competence and honesty ratings for each institution implies that the responses to the two sets of items do not derive from a single underlying

Figure 4. Perceived Competence of Leaders of the KGB and All Major Institutions, by Level of Education within Generations



dimension of evaluation.¹⁶ The leaders of the Soviet military and the Academy of Sciences were again the two most highly rated. Enterprise managers were third. Depending on what part of the distribution one focuses on, the leaders of the KGB rank either sixth or eighth—last—on the *honesty* dimension.

Thus, leaders of major Soviet institutions have a split image. They are viewed as generally capable, but not very honest. Even for the two agencies for which a large majority of the officials were judged to be competent, barely a third of the respondents judged them to be honest. The largest relative gaps between their image of competence and their image of honesty appeared for the Politburo, the KGB, and the *militsiia*. The smallest relative gaps appeared for leaders of the Academy of Sciences, the military, and industrial enterprises.

It is important to emphasize that neither dimension of evaluation should be interpreted as a measure of affect for the leaders of the institutions, particularly of the KGB. One does not admit that one actually likes a hangman when one acknowledges the efficiency with which he manipulates the rope.

In the remainder of the analysis, we examine factors related to the image of the competence of institutional leaders. Our main concern is to study perceptions of the capability or competence of the KGB, and to determine whether these perceptions are related to political non-conformity.

Determinants of Perceptions of Competence. To understand the factors that affect the evaluation of the secret police in particular, it is important to understand first what affects evaluations of authorities more generally. The basic reason for this is that respondents who evaluated the leaders of one institution as competent were also likely to evaluate leaders of the other seven institutions as competent.

Table A-3 (see Appendix) shows the correlations between the evaluations of the eight institutions, with the responses to the questions on competence transformed into numeric scores by assigning values to the response categories, ranging from zero (lowest evaluation) to four (highest evaluation).¹⁷ The correlations range from moderately to highly positive.

Initially, one might hypothesize that both the level of education and political generation will affect individual perceptions of leaders of major Soviet institutions. Previous research (Silver 1987) has shown that increases in education have tended to undermine support for two Soviet "regime norms," state control of the economy and collective or social control at the expense of individual rights.¹⁸ Also, younger respondents were less likely to support the norms than older ones.

The responses to the questions on competence of the authorities bear out only part of the pattern. As shown in Figure 4, the perceived competence of institutional leaders declines as the respondent's education increases. This is true for all eight institutions (see Table A-1), and within every generation. But generational effects move in the opposite direction. Younger cohorts rate the competence of Soviet leaders more highly than do older ones, even after adjusting for educational differences (see Figure 5 and Appendix Table A-2).

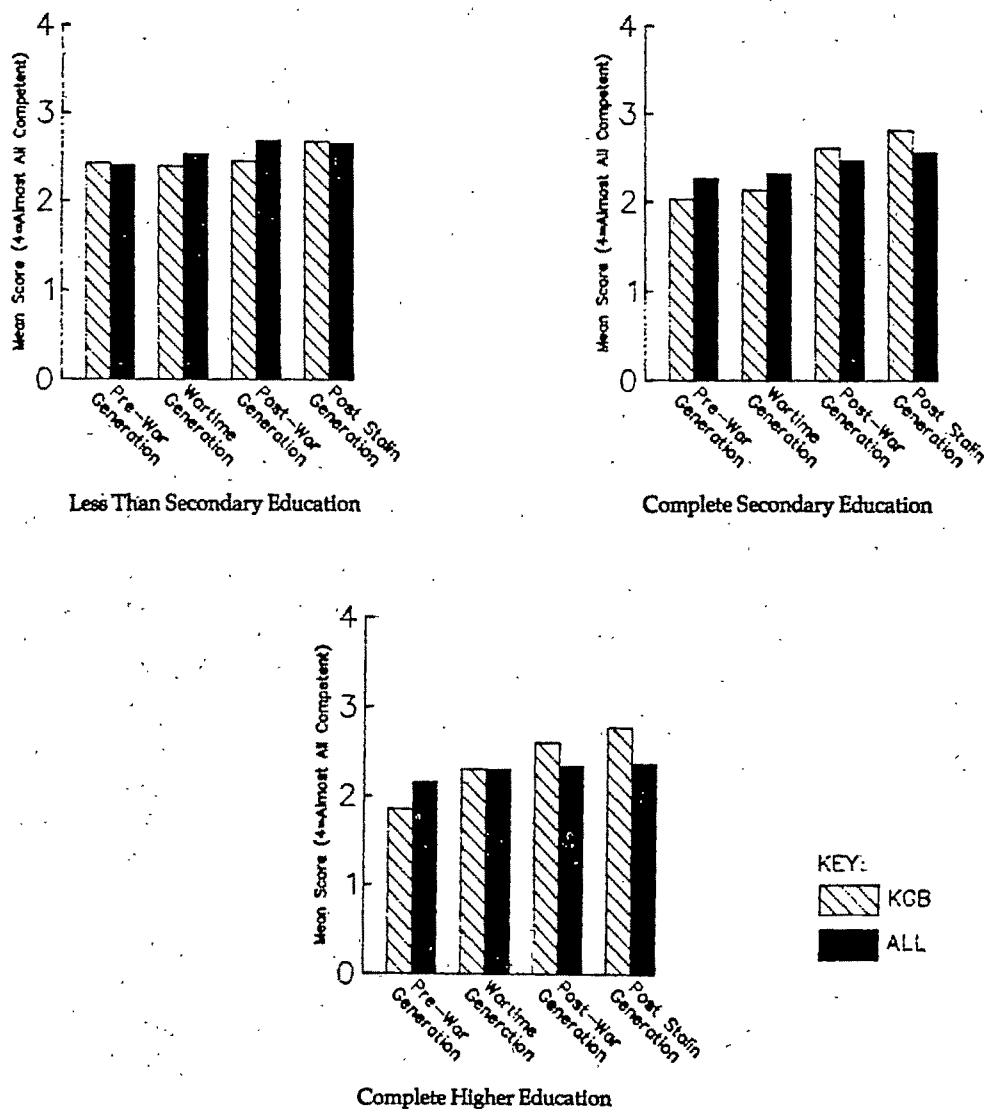
In light of these findings, it is important to examine whether other factors known to be related to support for regime norms are also related to the evaluation of the competence of Soviet institutional leaders. Based on earlier research, one might expect people who were more satisfied with their material status (housing, job, standard of living, provision of medical care) to evaluate the abilities of Soviet officials more highly (Silver 1987). Those more supportive of regime norms (established institutional practices) should also be more likely to evaluate the leaders of

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the institutions positively. On the other hand, one might expect persons who were more interested in political affairs to have a more negative evaluation of Soviet leaders. Finally, people who have experienced direct punitive contact with the KGB prior to their LNP ought to hold a more negative view of the authorities.

The results of a multiple regression analysis assessing the effects of all of these factors are shown in Table 3. The age categories are those used by Bahry (1987), and are based on Hough's (1980) classification of Soviet political generations. As expected, people's satisfaction with their material welfare is positively related to

Figure 5. Perceived Competence of Leaders of the KGB and All Major Institutions, by Generation within Levels of Education



their evaluation of the competence of the leaders of key Soviet institutions. In addition, the stronger people's support for regime norms, the more positive their image of institutional leaders, over and above the effects due to their satisfaction with material conditions. As shown in column 2, this relation holds up even after adjusting for the effects of the other factors.

Neither interest in politics, the number of privileges the respondent enjoyed, nor whether the respondent held a Party *nomenklatura* post¹⁹ showed a statistically significant relation to the evaluation of the institutional leaders.

Consistent with the data in Figure 4, persons with higher education held Soviet authorities in lower esteem than those with less education, even after other factors are taken into account. Consistent with the results in Figure 5, respondents in the postwar generation (born 1926-40) and the post-Stalin generation (born 1941-60) held a more positive image of the competence of Soviet authorities than did earlier generations.

The comparatively positive assessment of the competence of the authorities by the younger generations and the comparatively negative assessment by the older generations thus persist even after a large number of additional respondent characteristics are taken into account. It seems likely that this pattern of evaluations is a generational rather than an aging phenomenon. That is, it reflects the historical experiences of differing cohorts, not a typical pattern of change in political beliefs through the life cycle. One indication is that the cohort differences are especially sharp in the evaluation of the competence and honesty of the most politically coercive agency, the KGB. The prewar generation (born before 1919, reaching age 18 before 1937), whose formative political experiences occurred during the Great Terror, has a particularly

negative evaluation of the leaders of the KGB.

Table 3 also shows whether, after adjusting for other factors, people's direct experiences of being punished by the KGB affect their evaluations of the authorities. Respondents who were in families where another member was arrested for political reasons had a lower evaluation of the competence of officials. But those who themselves had previous punitive contact with the KGB (prior to the LNP) were not affected by this in their evaluation of the authorities (after adjusting for the effects of other factors).

In the latter case, it may be that most of the effect of people's experience with the KGB is captured by the measure of their subjective satisfaction. This is supported by additional analysis (not shown). Individuals who had been sanctioned by the KGB were much less satisfied with their material status in the LNP than those who did not have those experiences. Hence, via that mechanism, they had a lower estimation of the competence of the authorities in general.²⁰

How do subjective satisfaction and social background affect evaluations of the KGB in particular? To answer this question, one must take into account that over half (56%) of the evaluation of the competence of leaders of the KGB can be accounted for by people's evaluations of the competence of leaders of the other seven institutions.²¹ This means that if ratings of the KGB's competence were used directly as dependent variables in the equations for Table 3, the factors affecting evaluations of the KGB would work in much the same way as the factors affecting the average evaluation of all institutions. But the similarity in the results would be an artifact of the respondents' evaluation of the competence of institutional leaders in general.

To obtain a measure of the evaluation of the KGB that is distinct from the eval-

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Table 3. Regression of Average Competency Ratings on Subjective Material Satisfaction, Support for Regime Norms, Measures of Status, and Experience of Direct Coercion

Independent Variable	Average Competency ^a	
	b (1)	b (2)
Subjective material satisfaction ^b		
Average satisfaction	.199**	.202**
Support of regime norms ^c		
State control	.032**	.078**
Collective control	.082**	.037**
Interest in politics ^d		
Very interested		-.079
Somewhat interested		-.065
Slightly interested		-.054
Status measures		
Number of material privileges ^e		-.055
On Party <i>nomenklatura</i> ^f		-.097
Highest education ^g		
Complete secondary		-.064
Complete higher		-.095*
Generations ^h		
Wartime		.077
Postwar		.395**
Post-Stalin		.317**
Punitive experience before LNP ⁱ		
Punished by KGB		.045
Family member arrested		-.087*
Constant	2.014**	1.951**
Adjusted R ²	.111	.129
Number of cases	2,167	2,207

^aA single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

^b*Subjective material satisfaction* is a summated scale based on responses to questions concerning the respondent's satisfaction with five material conditions of life during his last normal period of life (LNP) in the USSR. *housing, job, standard of living, medical care, and availability of goods*. The scale ranges between 2 (most satisfied) and -2 (least satisfied), with 0 as a midpoint. For further details, see Silver 1987.

^cScores on the *state-private control* measure and the *collective-individual* measure range from 7 (greatest state-collective control) to 1 (greatest private-individual control). Each measure is based on responses to three questions. For details, see Silver 1987.

^dDummy variables are used to represent the respondents' stated degree of interest in politics. The question was "During your last normal period [of life in the Soviet Union], how interested were you in politics and public affairs—were you *very interested, somewhat interested, only slightly interested, or not at all interested?*" The omitted category is those who said they were *not at all interested*.

^eThis is a count of the number of privileges the respondent claimed to have received during the LNP, from a list of three: *access to a closed medical clinic, access to special shops, and use of an official car*. Scores range from 0 to 3.

^fDummy variable: 1 if the respondent held a job controlled by the Party *nomenklatura*, 0 if not.

^gDummy variables representing highest diploma or degree earned. The omitted category is *less than complete secondary education*

^hDummy variables: *wartime*, born 1919-1925 (reached age 18 in 1937-1943); *postwar*, born 1926-1940 (reached age 18 in 1944-1958); *post-Stalin*, born 1941-1960 (reached age 18 in 1959-1978). The omitted category is persons born before 1919.

ⁱDummy variables: Did the respondent have an encounter with the KGB that led to some kind of negative sanction *before the beginning of the LNP*? Was any family member arrested for political reasons at some time prior to any arrest the respondent may have experienced, or prior to the LNP?

uation of the other agencies, we calculated a *KGB differential*: the difference between the respondents' actual (reported) evaluation of the competence of KGB leaders and their expected evaluation of KGB leaders given their evaluation of the competence of the leaders of the other seven institutions.²² Positive KGB differentials mean that the respondents rated the KGB as more competent than would be expected from their rating of other authorities; negative KGB differentials mean that the respondents rated the

KGB as less competent than would be expected.

Table 4 replicates the analysis in Table 3, but with the KGB differential for competence as the dependent variable. The question answered by this part of the analysis is, Do the explanatory variables have a distinctive effect on the evaluation of the competence of the KGB's leaders over and above their effect on the average evaluation of the leaders of all institutions? The answer depends on the variable considered.

Table 4. Regression of Differences between Actual and Expected Ratings of KGB Competence on Material Satisfaction, Support for Regime Norms, and Measures of Status and Experience of Direct Coercion

Independent Variable	Rating of Competence of KGB Minus Expected Rating ^a	
	b (1)	b (2)
Subjective material satisfaction		
Average satisfaction	-.057**	-.010
Support of regime norms		
State control	-.053**	-.036**
Collective control	-.031**	-.015
Interest in politics		
Very interested		.156**
Somewhat interested		-.031
Slightly interested		-.029
Status measures		
Number of material privileges		.025
On Party <i>nomenklatura</i>		-.078
Highest education		
Complete secondary		.025
Complete higher		.051
Generations		
Wartime		-.066
Postwar		.130*
Post-Stalin		.264**
Punitive experience before LNP		
Punished by KGB		.035
Family member arrested		.146**
Constant	.308**	-.095
Adjusted R ²	.024	.067
Number of cases	1,607	1,607

^aA single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

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One finding is that variation in people's subjective material satisfaction does not affect the size of the KGB differential. The KGB neither derives any distinctive benefit from increases in material satisfaction nor suffers any distinctive loss in reputation from decreases in people's material satisfaction.

In contrast, support for regime norms, particularly the norm of state control, is related differently to the evaluation of the KGB than it is to the evaluation of other institutional leaders. Persons more in favor of state control of the economy (state ownership of industry, state control of agriculture, state provision of medical care) give lower ratings to the KGB than to other institutional leaders, even after adjusting for the effects of age or generational differences.

Given that a high estimate of the competence of Soviet leaders is correlated with greater support for state control of the economy (see Table 3), the results in Table 4 can also be interpreted as showing that persons who have relatively positive evaluations of institutional leaders *other than the KGB* are more likely to favor state control. Additional statistical tests (not shown) sustain this interpretation: support for regime norms is directly related to the evaluation of the competence and honesty of institutional leaders as a whole (excluding the KGB) but inversely related to evaluations of the competence of the KGB in particular. Hence, the more competent or intimidating the KGB appears, the lower the support for regime norms.²³

Also noteworthy are the effects of early punitive experience with the KGB. Respondents who were punished for unorthodox political activity did not have a measurably higher or lower estimate of the KGB's competence than those who were not punished. But those who had family members arrested had higher estimates of the KGB's competence (net of their estimate of the competence of other

institutions) than those who did not.

These results do not present strong evidence that punitive contact with the KGB affects people's evaluation of the KGB. However, it should be recalled that there is a link between people's punitive experience and their subjective material satisfaction. Those who were punished for political reasons were less satisfied with their material welfare than those who were not, and lower material satisfaction is linked to more negative evaluations of the authorities in general.

Differences by generation also stand out in Table 4. Once again, the KGB receives a much higher rating on both competence and honesty by the postwar generation and the post-Stalin generation than by the prewar and wartime (born 1919-25) generations. It is especially notable that the generational differences in the evaluation of the competence of the KGB are so sharp even after we control for the effects of generalized attitudes towards institutional leaders and of several other factors. Not only are the younger generations likely to rate the authorities in general more highly than the older generations, but they are especially likely to rate the KGB more highly.

These results are consistent with an earlier finding of a lack of historical sense among younger respondents (Bahry 1987). A significant minority of the young, particularly the less educated, were likely to report that the KGB was more powerful in Brezhnev's time than in Stalin's.

Avoiding Trouble with the KGB. The SIP general survey also included questions designed to tap perceptions of the randomness of the KGB's actions and the pervasiveness of its network of informers. The questions are given in the notes to Table 5. The answers are instructive.

Almost half of all respondents stated that it was "easy or very easy" to avoid

Table 5. How Easy Is It To Avoid Trouble with KGB?
 How Easy Is It to Tell Who Is an Informer?
 (by Generation within Levels of Education)

Generation within Levels of Education	Respondents Who Said It Was Very Difficult or Somewhat Difficult	
	Percentage	Base Number of Cases
To avoid trouble with KGB		
Less than complete secondary		
Prewar generation	60.8	120
Wartime generation	54.4	68
Postwar generation	58.6	99
Post-Stalin generation	56.5	23
Complete secondary		
Prewar generation	64.8	108
Wartime generation	61.6	99
Postwar generation	49.8	247
Post-Stalin generation	52.6	470
Complete higher		
Prewar generation	66.0	53
Wartime generation	51.1	45
Postwar generation	43.4	318
Post-Stalin generation	34.7	398
To tell who is an informer		
Less than complete secondary		
Prewar generation	81.1	122
Wartime generation	77.6	67
Postwar generation	79.8	94
Post-Stalin generation	78.6	28
Complete secondary		
Prewar generation	92.2	115
Wartime generation	87.9	107
Postwar generation	83.1	267
Post-Stalin generation	85.8	471
Complete higher		
Prewar generation	88.3	60
Wartime generation	94.6	56
Postwar generation	85.6	361
Post-Stalin generation	85.5	421

Note: The two questions that were asked were: (1) "Some people say that it is easy for most Soviet citizens to avoid trouble with the KGB. Others say it is difficult to avoid trouble with the KGB. What did you think during your last normal period [of life in the Soviet Union]—was it *very easy* to avoid trouble with the KGB, *fairly easy*, *fairly difficult*, or *very difficult*?" (2) "Some people say that it is easy to tell who is a KGB informer or undercover KGB agent among one's coworkers, fellow students, or neighbors. Others say it is very difficult to know for sure. During your last normal period, did you think it was *very easy* to tell, *somewhat easy*, *somewhat difficult*, or *very difficult*?"

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trouble with the secret police, hence that the actions of the KGB were predictable. But a much larger proportion—close to 80%—felt that it was somewhat difficult or very difficult to tell who might be a KGB informer among co-workers, fellow students, or neighbors.

The answers to these two questions taken together suggest yet another split image, between an organization with eyes and ears everywhere and one that is predictable and thus avoidable. The gap is especially pronounced for the younger generations and the highly educated. As Table 5 shows, virtually all the respondents agreed that it was difficult to know who was an informer or agent. Even so, the college-educated among the postwar and post-Stalin generations stand out as much more confident about the possibility of staying out of political trouble.

Further analysis (not shown) reveals that the respondents' sense of being able to calculate the odds, to avoid the secret police, is correlated with a more general sense of trust in other people. The lack of avoidance ability may thus be part of a more general syndrome of social atomization. On the other hand, the capabilities of the KGB to enforce social orthodoxy seem to be captured in the notion of the KGB's competence, and that is what we rely on as a measure of general deterrence in the analysis below.

Social Atomization

To understand how the intimidation factor might work to deter unorthodox political behavior, it is just as important to examine perceptions of other people as it is to examine perceptions of the KGB. To measure atomization, we rely on two items in the SIP questionnaire taken from Morris Rosenberg's (1956, 1957) widely used "misanthropy," or *faith-in-people*, scale. These questions were asked of a random one-third of the respondents. We combine the answers to the items to

derive our measure of *faith in people*, which ranges in value from zero (least faith in people) to three (most faith in people).²⁴

Although we hypothesize that those less trusting in other people will be less likely to become involved in unorthodox political activity, faith in people should be positively related to both unconventional and conventional political activism. This would be consistent with the findings in Almond and Verba's (1963) Five Nation Study, in which faith in people was important in accounting for cross-national differences in people's sense of political competence. Thus, trust of others may be an important political resource that facilitates participation in both unconventional and conventional political activity.

Table 6 shows that, within political generations, there is very little difference in faith in people across educational levels. In contrast, generational differences are very sharp. Within each educational level, those who came of political age before or during World War II had much less faith in people than those who came of political age after the war.

The low correlation between education and faith in people among SIP respondents contrasts with Almond and Verba's (1963) finding that trust in others increased with education in the five nations that they studied. The pattern among SIP respondents indicates that social atomization in the Soviet Union is not confined to persons of a certain social status. Instead, the level of interpersonal trust varies much more systematically by generation than by social status.

The evidence suggests for the older generation something of a "burnt-finger-tips" phenomenon, a wariness about letting down one's guard; while the younger generations, which came of political age in a period when the overt use of terror was much less common, were less wary of trusting others. Thus, the atomization and isolation of people from one another

Table 6. Faith in People, by Education and Generation

Education and Generation	Mean	Standard Deviation	Number of Cases
Level of Education within generations			
Prewar			
Less than complete secondary	.91	1.11	53
Complete secondary	.81	1.06	52
Complete higher	.82	.83	22
Wartime			
Less than complete secondary	.96	1.11	29
Complete secondary	1.00	.99	45
Complete higher	.83	1.13	18
Postwar			
Less than complete secondary	1.46	1.14	58
Complete secondary	1.18	1.12	107
Complete higher	1.24	1.08	133
Post-Stalin			
Less than complete secondary	1.50	1.17	15
Complete secondary	1.22	1.13	187
Complete higher	1.31	1.84	147
Generation within levels of education			
Less than complete secondary			
Prewar	.91	1.11	53
Wartime	.96	1.11	29
Postwar	1.46	1.14	58
Post-Stalin	1.50	1.17	15
Complete secondary			
Prewar	.81	1.06	52
Wartime	1.00	.99	45
Postwar	1.18	1.12	107
Post-Stalin	1.22	1.13	187
Complete higher			
Prewar	.82	.83	22
Wartime	.83	1.13	18
Postwar	1.24	1.08	133
Post-Stalin	1.31	1.84	147

Note: The two items used to construct the measure were (1) "Some people we've talked to say that most people can be trusted. Others say you can't be too careful in your dealings with people. How did you feel about that during your last normal period [of life in the Soviet Union]?" Answers coded: *Most people can be trusted, You can't be too careful, and It depends* (if volunteered). (2) "Speaking generally, would you have said during your last normal period that most people are more inclined to help others, or more inclined to look out for themselves?" Answers coded: *more inclined to help others, more inclined to look out for themselves, and it depends* (if volunteered).

Scores were derived by assigning a 3 to the answers *Most people can be trusted and more inclined to help others*, 1.5 to *It Depends*, and 0 to *You can't be too careful and more inclined to look out for themselves*, then averaging the values of each item. If only one item was answered with a valid value, only that item is used to compute the score.

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appears to have been more or less indelibly impressed on the older generations but not firmly impressed on the younger ones.

This interpretation is supported by other evidence in the SIP general survey. The respondents were asked whether during their last normal period of life in the USSR they could have criticized an official (*ofitsial'noe litso*): to *nobody, members of their family, friends, other relatives, coworkers or fellow students, or their boss or teacher*.²⁵ While few respondents of any generation said they could have criticized an official to their boss or teacher, among respondents in the prewar and wartime generations, 18% said they could not have talked to anyone about this issue, while only 9% of those in the postwar generation and 4% of those in the post-Stalin generation said they could not have talked to anyone about it.

Of those who felt that they could criticize an official to someone, 44% of those in the prewar and wartime generations would extend the circle of communication only as far as their immediate family; while 31% of those in the postwar generation and only 22% of those in the post-Stalin generation would limit the criticism to their immediate family. This generational pattern holds up even after adjusting for differences due to education.

The differences in the level of faith in people may explain in part why the younger generations are more inclined to become involved in unorthodox political activity than the older ones. Being able to count on the support of others or, at least, relative freedom from worry about their doing harm to you, is likely to facilitate greater political involvement.

The Intimidation Factor and Political Activism

Our aim in the last part of this study is to determine whether intimidation served as a general deterrent on nonconformist

behavior. We also explore the impact of intimidation on *compliant* political activism, to determine whether social atomization or fear of the authorities causes people to be more active in conventional participation and whether some of the sources of nonconformist activity also give impetus to compliant acts.

Measuring Activism. To measure the two dimensions of political activism, we constructed scales that capture both the level and intensity of each person's involvement.²⁶ For compliant activism we devised a summated scale based on the number of types of communal groups to which the respondent belonged in the LNP: housing, repair, or sanitary commissions; commissions at work or in local soviet or Party organizations; membership in the citizens' militia (*druzhina*) or comrades' courts; parents' commissions; the Komsomol; and other communal organizations.²⁷

This measure is weighted by the intensity of involvement in each type of group—whether the respondent was a leader, attended regularly, attended occasionally, or belonged but never attended.²⁸ About half the respondents engaged in some such activity; the average score for the respondents as a whole is 2.1 out of a possible 28, with individual scores ranging from 0 to 28 and a standard deviation of 2.96.

Our measure of nonconformist activism follows the same logic. It is constructed by totaling the number of different types of nonconformist activity in which the respondent engaged during the LNP, from unsanctioned study groups to distributing *samizdat*, engaging in protests, and strikes. It also includes a count of other unconventional acts as defined by the respondent; and it counts similar behaviors reported in response to questions about contacts with the KGB during the respondent's LNP. Ten percent of all respondents participated in some form of

nonconformist activity as defined here; and 2% more were leaders. Although actual individual scores range from 0 to 16, the average score for the respondents as a whole is very low—.41, with a standard deviation of 1.44.²⁹

Hypotheses. We conceptualize the impact of intimidation—fear of the KGB and of other people—on unconventional political behavior by regarding nonconformity as a result of three types of variables: (1) those that *motivate* the expression of political disaffection, (2) those that *facilitate* its public manifestation, and (3) those that *inhibit* its manifestation. This approach is similar to that proposed by Gurr (1968) for explaining civil strife. Gurr's model posits that there are sources of dissatisfaction (relative deprivation) that motivate people to engage in civil strife, but whether or not this engagement occurs is mediated by the factors of state coercion, legitimacy, institutionalization, and facilitation.

As indicators of the motivation for expressing disaffection, we include the respondents' satisfaction with their material quality of life during their LNP, and the respondents' support for established regime norms. Unconventional activists should be less satisfied with their material quality of life, established regime norms, or both.

Certain political resources should facilitate nonconformity. Education, for example, should provide the conceptual skills necessary to visualize alternatives and to manipulate the system (Moore 1954). Increasing education has been shown already to be related to decreasing support among SIP respondents for established institutional practices (Silver 1987). Interest in politics can also be viewed as a resource, one that prompts the individual to seek out more information and thus to weigh the regime's message against other (foreign and domestic) sources. Interest can thus help to reduce the ambiguity sur-

rounding public issues, and the less the ambiguity, the greater the likelihood of questioning the established order (Jahoda and Cook 1954).

Earlier research has shown that the generations that came of age after Stalin were both the ones most likely to engage in compliant activity and the ones most likely to engage in nonconformist activity (Bahry 1987). The young were also more mobilized in the conventional sense (Zimmerman 1987). These higher levels of both types of activism are a product of the renewal of mass organizations after Stalin and of changes in political atmosphere that prompted the growth of dissident movements after Khrushchev. We should expect to find similar generational differences in the level of political involvement in the analysis here.

A person's status may be important as well: those with more material privileges and a job on the Party's *nomenklatura* are likely to have connections and thus a degree of insulation from reprisals, at least for the more common brands of unconventional behavior. On the other hand, persons whose jobs are controlled by the Party's *nomenklatura* may also be more immediately subject to discipline for transgressions against official standards of behavior.

The intimidation factor should tend to inhibit overt unconventional activity. Individuals with a high regard for the capabilities of the secret police and those with low faith in other people should be less likely than others of the same age, education, and degree of disaffection to overstep the bounds of acceptable political behavior.³⁰

Nonconformist Activism. Column 1 in Table 7 shows the relation between nonconformist activism and the hypothesized motivational factors.³¹ As expected, people less supportive of regime norms (particularly state control of the economy) were more likely to be nonconformists.

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Table 7. Regression of Nonconformist and Compliant Activism on Subjective Satisfaction, Support for Regime Norms, Atomization, and Perception of Coercion

Independent Variable	Nonconformist Activism			Compliant Activism
	b (1)	b (2)	b (3)	b (4)
Subjective material satisfaction				
Average satisfaction	-.087**	-.095**	-.066**	-.026
Support of regime norms ^a				
State control	-.225**	-.233**	-.163**	.061
Collective control	-.008	-.009	.039	.154**
Atomization-coercion				
Faith in people ^b		.062**	.039**	.060*
KGB highly competent ^d		-.039	-.067*	-.011
Interest in politics				
Very interested			.214**	.221*
Somewhat Interested			-.005	.112
Slightly interested			-.016	.063
Status measures				
Number of material privileges			.140**	.165**
On party <i>nomenklatura</i>			-.178**	.510**
Highest education				
Complete secondary			-.057	.043
Complete higher			.053	.204*
Generations				
Wartime			.044	.200
Postwar			.110*	.306**
Post-Stalin			.196**	.651**
Punitive experience before LNP				
Punished by KGB			.301**	-.436*
Family member arrested			.065	-.064
Constant	.479**	.439**	.157	-.119
Adjusted R ²	.075	.090	.179	.146
Number of cases	612	612	612	612

Note: Logarithms of the dependent variables are used. A single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

^aThe natural log logarithm of the values of state-private and collective-individual control was used in the equations for this table.

^bScores range from 3 (most faith in people) to 0 (least faith in people).

^cDummy variable: 1 if respondent said that *most* or *almost all* of the leaders of the KGB were competent, 0 if not.

Columns 2 and 3 add in the effects of atomization, coercion, and the other variables hypothesized to facilitate or inhibit the manifestation of nonconformity. As the atomization thesis would predict, the greater people's interpersonal trust (faith in people), the more likely they were to be nonconformist. Similarly, persons who perceived the KGB to be highly competent were less likely to engage in unorthodox activity. Thus, both components of the intimidation factor appear to operate in the expected direction: social atomization and perceptions of the KGB's competence serve to deter would-be nonconformists. At the same time, consistent with our earlier findings concerning *individual* deterrence, prior punishment by the KGB is associated with an increased likelihood that the respondents were nonconformist in their LNP.

Persons who were more interested in political affairs were more likely to be unconventional activists, even after adjusting for the effects of education, material satisfaction, and support for the regime. At the same time, interest in politics is also positively associated with *compliant* activism (see column 4). Thus, those who are interested in politics are likely to be motivated to some sort of action whatever their political orientation.

Having more material privileges is also positively related to nonconformity. One explanation for this may be that persons with access to privileges benefit from greater *protektsiya* (connections) with people in high places, and hence risk less by their involvement in nonconformist activities.³² Another explanation may be that the privileged among the SIP respondents more often were in the arts and culture and not only had more temptations and opportunities for engaging in unconventional activities but also were more attracted to them. A third explanation may be that having access to privileges is linked to greater incentives to become involved in political affairs, both

nonconformist and conformist. That there is a positive relation between access to privileges and participation in compliant activism (column 4) is consistent with this interpretation.

Persons who held important official positions that require Party approval were less likely to be involved in nonconformist activity and more likely to be compliant activists than those who did not hold such positions (compare columns 3 and 4). Thus, they may have been inhibited by Party control from engaging in unconventional behavior, whatever their personal sense of material satisfaction or level of support for regime norms.

As expected, persons in the postwar and post-Stalin generations were more involved in political nonconformity, even after we adjust for their more critical views of established institutional practices, their lower levels of satisfaction (higher aspirations) concerning material conditions, their greater sense of the capabilities of the leaders of the KGB, and their higher levels of interpersonal trust. In other words, the relation between generation and nonconformist activism holds up even after adjusting for the effects of motivational factors, sense of intimidation, and degree of faith in people.

One could, of course, maintain that young people are inclined toward nonconformity, and that progression through the life cycle fosters greater compliance as people develop a greater stake in society in connection with forming their own family and developing their careers. Evidence in the survey challenges the life-cycle interpretation, however. Nonconformity did not diminish evenly with age among the postwar and post-Stalin generations (see Bahry 1987). It is also important to keep in mind that the "younger generations" in the survey do not consist of "very young people." Respondents in the postwar generation had a median age of 41 at the midpoint of their LNP; and respondents in the post-Stalin generation

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had a median age of 28 at the midpoint of their LNP.

The younger generations were also more likely than the older generations to be involved in *compliant* political organizations. Hence, the activism of the younger cohorts appears to reflect special formative experiences and commitments of those generations that cannot be readily reduced to a set of explicitly measured motivational and ideological factors.

Compliant Activism. The results in column 4 indicate that the motivation for compliant activism derives from something other than dissatisfaction with the material quality of life. Nor is it an expression of satisfaction with the established political order. That those with higher education, as well as those who hold Party *nomenklatura* positions, were more likely to be compliant activists implies that they were responding to the expectation for persons of their status to become involved in public organizations and *obshchestvennaya rabota* (work for society). Hence, such activism may be less a reflection of individual conviction and more a calculated conformity, a formal adherence to official norms of behavior intended to assure physical survival or to secure career advancement (for a similar interpretation, see Shtromas 1984).

Comparison of the determinants of nonconformist and compliant activism reveals that some factors both motivate and give a direction to political activism —toward conformity or nonconformity. These include degree of satisfaction with the material quality of life, level of support for regime norms, and whether a person holds a job that is under the control of the Party's *nomenklatura*. Other factors stimulate involvement in both types of activism: faith in people, interest in politics, access to privilege, and coming of political age in the postwar period.

Intimidation, however, works differently with respect to the two types of

political involvement. A perception of the KGB as competent inhibits nonconformity but does not breed greater compliant activism, while atomization appears to deter both types of political activism, nonconformist and compliant.

Conclusions and Implications

Our analysis confirms that general deterrence works, at least to a degree. What is striking is that individual deterrence does not. Perceptions of the regime's coercive capability tend to inhibit dissent *ex ante*; but once people do transgress the borders of acceptable political behavior, punishment (short of incapacitation) is not likely to keep them from further nonconformity. We think this indicates that the experience of being punished reduces an individual's possible gains from future compliance and induces a more permanent hostility to the regime.

Thus, there appears to be a trade-off between individual and general deterrence. By cutting off the return route to normalcy for those whom it punishes, the state assures a certain amount of recidivism and may create a class of people who are permanently alienated. At the same time, the unidirectional nature of this route heightens the effectiveness of general deterrence: the permanency of the stigma of being punished may be a key to deterring the otherwise disaffected from nonconformity in the first place.

Therefore, aside from the question of cost-effectiveness, singling out leaders for reprisals is logical from the regime's standpoint. By not trying to punish every transgressor of official norms, the state minimizes the number of people permanently alienated by being penalized. But punishing leaders provides a demonstration effect that enhances general deterrence.

Our data also reveal that the modernization of coercion has engendered a sense

of its predictability. Soviet citizens, especially the young and the highly educated, seem to believe they know the odds of reprisal if they violate official political norms. However, the differences in such perceptions suggest that the reduction in the use of mass terror since Stalin's day has had different impacts on different publics.

For witnesses of the Great Terror in the late 1930s, the message is still clear: *the KGB can still get you, and you shouldn't trust other people.* Those who survived the Stalin era see the secret police as less competent, perhaps because of the arbitrariness and sheer scope of police activity during that time. But among these generations, the more competent the KGB is perceived to be, the less nonconformity.

For the young, the Great Terror is an appropriated memory. The postwar and post-Stalin generations view the KGB as more professionally competent, but they are also more inclined to see the KGB's actions as predictable and avoidable. Without the experience of the Stalin era, they seem to feel better able to calculate the odds of reprisal. Equally important, these generations have grown up in an atmosphere of more stable social relations. Mistrust of others has diminished; these generations are less atomized.

Not all members of the postwar and post-Stalin generations see the world through the same eyes, however. The college-educated are much more sanguine about their ability to avoid political trouble, in part because they possess the conceptual skills and information needed to manipulate the system, and in part because the kinds of unconventional activities in which they are most likely to engage bring fewer reprisals. Few unsanctioned "intellectual" activities (e.g., unsanctioned study groups, reading and distributing *samizdat-tamizdat*) evoked any official penalty. But the types of nonconformity in which the less educated are likely to engage (e.g., strikes) are much

more likely to evoke a coercive response.

The regime's selectivity in dealing with unorthodox behavior thus carries class overtones and may help to explain why there are fewer blue-collar dissenters. In addition, the less educated are more satisfied with material conditions, more supportive of state controls over the economy and over the citizenry (Silver 1987), and more positive in their view of the leaders of Soviet institutions in general.

For Soviet leaders, there appears to be a trade-off between building up mass support for the established political order and building up the image of the KGB. The greater the sense of material satisfaction, then the more positive the evaluation of Soviet leaders as a whole, and the stronger the backing for the established norms of state control. But the greater the perceived capability of the KGB—that is, the more coercive the regime is believed to be—the weaker the popular support for the regime.

Our results thus suggest that if Soviet leaders were to downgrade the KGB, they could increase support for the regime. But a KGB with a less potent image would also be less able to deter nonconformity. Hence, a leadership that considers the maintenance of its own control over society to be a paramount goal is likely to continue to promote the KGB's image. As long as the KGB does not threaten the leadership itself, the leadership can accept the resulting reduction in popular support as a necessary cost.

In sum, the regime's ability to maintain social control relies on a correlation of forces: the manipulation of material and normative rewards and a mix of mistrust and intimidation. One critical variable is the perceived level of material welfare of the population. The greater the sense of material well-being, the greater the popular support for Soviet political orthodoxy and the more positive the evaluations of Soviet leaders. But even among those who have a low sense of material

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satisfaction and low support for the system, social atomization and perceptions of the regime's coercive potential still inhibit nonconformity.

Our data show that the balance is changing, however. The success of Soviet leaders in maintaining political peace depends on their striking a balance between individual and general deterrence, on a residuum of social atomization, and on a reserve of popular support that is stronger among the older genera-

tions and the less educated. But with the succession of generations, the social atomization bred by the Great Terror may be withering away, along with its inhibiting effect on unconventional behavior. Generational replacement and increasing educational attainments also appear to be weakening support for the old orthodoxies. Therefore, a regime bent on suppressing dissent and open opposition must rely on other means to achieve the same degree of control.

Appendix

**Table A-1. Mean Rating of Leaders of Institutions for Competence and Honesty,
by Level of Education within Generations**

Level of Education Within Generations	Local Party	Local Soviet	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB	All
COMPETENCE									
All respondents									
Less than secondary	2.29	2.40	2.30	2.65	2.91	2.94	2.36	2.45	2.54
Complete secondary	2.13	2.20	2.20	2.58	2.86	2.90	2.33	2.62	2.48
Complete higher	1.94	2.01	2.10	2.46	2.77	2.72	2.07	2.64	2.34
Prewar									
Less than secondary	2.17	2.22	2.09	2.55	2.81	2.74	2.18	2.43	2.40
Complete secondary	1.97	2.16	2.03	2.45	2.72	2.72	2.04	2.03	2.26
Complete higher	1.67	2.00	1.92	2.43	2.51	2.59	1.73	1.85	2.16
Wartime									
Less than secondary	2.12	2.40	2.44	2.63	2.77	2.98	2.17	2.39	2.53
Complete secondary	1.95	2.17	2.08	2.48	2.70	2.68	2.11	2.13	2.32
Complete higher	1.93	1.98	1.91	2.56	2.70	2.74	1.97	2.30	2.30
Postwar									
Less than secondary	2.48	2.50	2.42	2.73	3.07	3.09	2.60	2.45	2.68
Complete secondary	2.17	2.24	2.20	2.53	2.76	2.92	2.37	2.61	2.47
Complete higher	1.97	2.00	2.13	2.45	2.76	2.66	2.02	2.61	2.34
Post-Stalin									
Less than secondary	2.33	2.77	2.46	2.78	3.00	2.96	2.57	2.67	2.65
Complete secondary	2.18	2.20	2.25	2.65	2.96	2.96	2.39	2.81	2.56
Complete higher	1.94	2.02	2.11	2.46	2.82	2.78	2.15	2.78	2.37
HONESTY									
All respondents									
Less than secondary	1.76	1.88	1.69	2.02	2.34	2.53	1.76	1.49	1.99
Complete secondary	1.48	1.61	1.40	1.86	2.20	2.20	1.36	1.24	1.70
Complete higher	1.10	1.37	1.21	1.84	2.08	1.94	.93	.94	1.46
Prewar generation									
Less than secondary	1.59	1.85	1.63	1.99	2.21	2.34	1.58	1.34	1.89
Complete secondary	1.59	1.79	1.59	1.90	2.35	2.29	1.41	1.10	1.80
Complete higher	1.31	1.63	1.29	1.93	2.09	2.08	1.00	.85	1.57
Wartime									
Less than secondary	1.55	1.69	1.45	1.78	2.04	2.44	1.47	1.31	1.84
Complete secondary	1.51	1.70	1.48	1.92	2.11	2.20	1.52	1.30	1.73
Complete higher	1.21	1.35	1.22	1.80	2.17	2.15	1.20	1.02	1.54
Postwar									
Less than secondary	2.06	2.04	1.88	2.18	2.58	2.75	2.07	1.65	2.18
Complete secondary	1.43	1.55	1.30	1.84	2.13	2.19	1.26	1.13	1.63
Complete higher	1.06	1.30	1.17	1.78	2.00	1.90	.88	.91	1.43
Post-Stalin									
Less than secondary	1.76	1.93	1.80	2.07	2.65	2.62	2.05	1.96	2.12
Complete secondary	1.48	1.59	1.40	1.86	2.22	2.19	1.38	1.32	1.71
Complete higher	1.09	1.39	1.24	1.88	2.14	1.94	.93	.95	1.46

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**Table A-2. Mean Rating of Leaders of Institutions for Competence and Honesty,
by Generation within Levels of Education**

Generation within Levels of Education	Local Party	Local Soviet	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB	All
COMPETENCE									
All respondents									
Prewar	1.99	2.15	2.04	2.49	2.71	2.71	2.04	2.17	2.30
Wartime	2.00	2.19	2.14	2.54	2.72	2.78	2.10	2.24	2.38
Postwar	2.12	2.16	2.20	2.52	2.80	2.82	2.23	2.59	2.44
Post-Stalin	2.07	2.13	2.19	2.57	2.90	2.88	2.29	2.79	2.48
Less than secondary									
Prewar	2.17	2.22	2.09	2.55	2.81	2.74	2.18	2.43	2.40
Wartime	2.12	2.10	2.44	2.63	2.77	2.98	2.17	2.39	2.53
Postwar	2.48	2.50	2.42	2.73	3.07	3.09	2.60	2.45	2.68
Post-Stalin	2.33	2.77	2.46	2.78	3.00	2.96	2.57	2.67	2.65
Complete secondary									
Prewar	1.97	2.16	2.03	2.45	2.72	2.72	2.04	2.03	2.26
Wartime	1.95	2.17	2.08	2.48	2.70	2.68	2.11	2.13	2.32
Postwar	2.18	2.24	2.20	2.53	2.76	2.92	2.37	2.61	2.47
Post-Stalin	2.18	2.20	2.25	2.65	2.96	2.96	2.39	2.81	2.56
Complete higher									
Prewar	1.67	2.00	1.92	2.43	2.51	2.59	1.73	1.85	2.16
Wartime	1.93	1.98	1.91	2.56	2.70	2.74	1.97	2.30	2.30
Postwar	1.97	2.00	2.13	2.45	2.76	2.66	2.02	2.61	2.34
Post-Stalin	1.94	2.02	2.11	2.46	2.82	2.78	2.15	2.78	2.37
HONESTY									
All respondents									
Prewar	1.53	1.78	1.55	1.95	2.24	2.27	1.41	1.16	1.80
Wartime	1.44	1.61	1.41	1.85	2.11	2.26	1.43	1.24	1.72
Postwar	1.34	1.50	1.32	1.86	2.12	2.14	1.18	1.10	1.61
Post-Stalin	1.31	1.51	1.33	1.87	2.20	2.09	1.19	1.17	1.61
Less than secondary									
Prewar	1.59	1.85	1.63	1.99	2.21	2.34	1.58	1.34	1.89
Wartime	1.55	1.69	1.45	1.78	2.04	2.44	1.47	1.31	1.84
Postwar	2.06	2.04	1.88	2.18	2.58	2.75	2.07	1.65	2.18
Post-Stalin	1.76	1.93	1.80	2.07	2.65	2.62	2.05	1.96	2.12
Complete secondary									
Prewar	1.59	1.79	1.59	1.90	2.35	2.29	1.41	1.10	1.80
Wartime	1.51	1.70	1.48	1.93	2.11	2.20	1.52	1.30	1.73
Postwar	1.43	1.55	1.30	1.84	2.13	2.19	1.26	1.13	1.63
Post-Stalin	1.48	1.59	1.40	1.86	2.22	2.19	1.38	1.32	1.71
Complete higher									
Prewar	1.31	1.63	1.29	1.93	2.09	2.08	1.00	.85	1.57
Wartime	1.21	1.35	1.22	1.80	2.17	2.15	1.20	1.02	1.54
Postwar	1.06	1.30	1.17	1.78	2.00	1.90	.88	.91	1.43
Post-Stalin	1.09	1.39	1.24	1.88	2.14	1.94	.93	.95	1.46

Table A-3. Pearson Correlations between Evaluations of Competence and Honesty of Leaders of Eight Soviet Institutions

Institution	Local Soviet	Local Party	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB
COMPETENCE								
Local Soviet	1.0	.68	.59	.65	.53	.57	.60	.42
Local Party		1.0	.66	.52	.47	.53	.74	.58
Local Police			1.0	.51	.48	.55	.65	.64
Managers of Industry				1.0	.60	.59	.50	.46
Academy of Sciences					1.0	.65	.50	.49
Military						1.0	.54	.57
Politburo							1.0	.66
KGB								1.0
Average R								.55
HONESTY								
Local Soviet	1.0	.79	.73	.65	.64	.66	.70	.66
Local Party		1.0	.76	.61	.60	.62	.80	.77
Local Police			1.0	.62	.58	.61	.71	.73
Managers of Industry				1.0	.64	.65	.54	.52
Academy of Sciences					1.0	.76	.56	.53
Military						1.0	.60	.56
Politburo							1.0	.84
KGB								1.0
Average R								.66

Notes

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1. We refer to *nonconformity* and *unconventional activity* rather than *dissent* for two reasons: First, the term *dissent* implies active, programmatic opposition, while we are concerned with the disposition to overstep the bounds of politically correct behavior, programmatic or not. Second, most respondents who did overstep would not necessarily refer to themselves as *dissidents*.

2. In two cases, part of the respondent's LNP

occurred during Nikita Khrushchev's regime. In all other cases, the respondent's LNP both began and ended during Brezhnev's regime. In 92% of the cases, the respondent's LNP ended between 1978 and 1981, inclusive.

3. Since our concern is with adult political activity, we restrict our analysis to the 2,667 respondents who had reached age 18 at the start of their LNP. Field work for the survey was conducted by the National Opinion Research Center. The average length of the interviews was three hours. All but 10 of the interviews were conducted in Russian. The sample was a stratified random sample based on a list of adult emigrants from the USSR to the United States between the dates mentioned. Participation in the survey was voluntary. The response rate was 79%. For further information about the sample, see Anderson and Silver 1987a.

4. The respondent's ethnic identity comes into play in two ways. One is on questions asking specifically about ethnic relations and nationality policies in the USSR. Second, Soviet emigrants in our study came from two basic groups: persons in the third wave of the Jewish emigration (which includes many non-Jewish spouses and family members), and individuals who were political dissidents or other special cases involving family

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reunification (this group consists primarily of non-Jews, but it also includes many Jews). For this reason, non-Jewish respondents were more likely to be politically nonconformist than Jewish respondents. For further discussion of potential sources of bias in the survey, see Bahry 1985, 1987; Silver 1987; Anderson and Silver 1987a, 1987b; Millar 1986; and Swafford et al. 1987.

5. These concepts are widely used in the literature on criminology. For a brief statement, see von Hirsch 1976.

6. *Samizdat* (literally "self-publication") is underground material by Soviet authors. *Tamizdat* (literally "publication over there") is material by Soviet authors that has not been approved for publication or distribution in the Soviet Union but has been published abroad.

7. Thomas Schelling has observed that threats can often be made more credible if the actor who makes the threat appears to be irrational and unpredictable: 'The essence of randomization in a two-person zero-sum game is to preclude the adversary's gaining intelligence about one's own mode of play—to prevent his deductive anticipation of how one may make up one's own mind, and to protect oneself from tell-tale regularities of behavior that an adversary might discern or from inadvertent bias in one's choice that an adversary might anticipate' (1963, 175).

8. Many more engaged in "private" nonconformity such as reading of *samizdat-tamizdat*. And many engaged in behaviors that are officially frowned upon but tacitly allowed, such as listening to Western radio broadcasts. In addition, almost 20% of the respondents indicated that they had attended unofficial art shows or other cultural events; but information from interviewers suggests that many people did not hear the word *unofficial* in the question and hence answered in terms of the regime-sanctioned cultural activities that they attended. See Bahry 1987.

9. Punishment includes any action taken against the respondent by any agency or official (including the KGB, the Party, supervisors at work, and other organizations or individuals sanctioned by the state). The penalties include reprimands at work or school; interrogations or reprimands by Party officials or the KGB; being threatened, harassed, followed, or arrested. Most official actions taken against the individual were reprimands, warnings, or interrogations.

10. It is important to note that the boundaries of the permissible fluctuate over time (Reddaway 1983). But there appear to be clear patterns of reprisal for the period under study.

11. This information is obtained by asking questions about any official actions that were taken in connection with the respondents' participation in other unconventional activities, their failure to participate in mobilized participation (such as voting),

or their making individual protests to officials, as well as the question, Other than what you may have already told me, did you have any (other) contact with the KGB?

12. The low frequency of contact with the KGB overall among SIP respondents could reflect a reluctance of the respondents to speak frankly about these contacts. However, the frequency of contact is consistent with Reddaway's (1983) estimates of some two hundred political arrests per year in the late 1970s in the entire USSR.

13. We also tested whether the effect of individual deterrence might depend on whether a person was arrested or interned rather than harassed or interrogated; but the severity of punishment made no difference in later political nonconformity. To be complete, the analysis should also include data on the incidence of recidivism among people who were nonconformists before their LNP but who were not penalized for it, but the survey does not provide this information.

14. This contrasts with the general Soviet philosophy of punishment, which focuses on rehabilitation rather than retribution (Hazard 1983, 99ff).

15. Two series of questions were asked. The first concerned the honesty of officials: 'Here is a list of some institutions that exist in the Soviet Union. I would like you to think about how many of the people in charge of each of them you felt were *honest* during your last normal period. For each one, circle the number under the category that best describes how many of the people in charge were *honest*' (*almost all, most, some, hardly any, or none*).

The second series concerned the competency of officials: 'Now turn the page over. On this side, think about how many of the people in charge at each of these institutions were competent, that is, did their jobs well. Remember, try to think about how you felt during your last normal period.'

"People in charge" translates as *rukovoditelei*; "competent" translates as *kompetentnyi*; and "honest" translates as *chestnyi*. *Chestnyi* may be interpreted not simply as "honest" but also as "honorable" or "having integrity."

16. A factor analysis of the responses to the 16 items confirms that the responses to the competence items are distinct from the responses to the honesty items. The analytic results are not presented here.

17. *None* is assigned a value of 0; *hardly any*, 1; *some*, 2; *most*, 3; and *almost all*, 4. Averaging the evaluations across all eight institutions, we get a mean value on competence of 2.43, with a standard deviation of .782.

18. The "state-control" dimension is reflected in answers to questions about the respondent's preference for state ownership of heavy industry, state control of agriculture, and state provision of medical care. The "collective-control" dimension is reflected in answers to questions about limitations on the right to strike, control of population movement

through residence permits, and protecting the rights of society rather than the rights of individuals accused of crimes. See Silver 1987.

19. A Party *nomenklatura* post is a job or position in the state or cooperative sector for which a Party official controls personnel selection. The official can either directly determine or exercise a veto over who fills the post. Five percent ($N = 134$) of the respondents reported that their job during their LNP was a Party *nomenklatura* position.

20. Full specification of the relationships between experience with the KGB, subjective material satisfaction, and evaluation of the authorities requires a multiequation model. Our main concern here is whether the relationship between the respondents' evaluations of the authorities and their social backgrounds, subjective material satisfaction, and support for regime norms remains even after their punitive experiences are taken into account. The answer is yes, they do.

21. This is the r-square obtained by regressing the evaluation of the competency of the leaders of the KGB on the evaluations of the competency of the other institutional leaders.

22. This involves the use of an instrumental variable derived by regressing the evaluation of the KGB on the evaluations of the other seven agencies. The resulting regression coefficients were then used to estimate an expected evaluation of the KGB for each respondent based on the respondent's evaluation of the other agencies. The expected KGB evaluation was then subtracted from the respondent's actual (reported) KGB evaluation to derive a *KGB differential*—the difference between the actual and expected rating of the KGB's competence given the respondent's ratings of the other institutions. The mean value for the KGB differential is 0, with a standard deviation of .781, and with values ranging between -3.3 and 3.5.

23. This can be shown by reversing the assumed direction of the causal relationship between support for regime norms and the evaluation of competence of the authorities from the direction specified in Tables 3 and 4. However, to study individual differences in the evaluation of the authorities, it seemed most reasonable to assume that these evaluations are contingent on the individual's subjective material welfare and on his or her affect toward the regime in general. The relation could in fact be reciprocal.

24. The mean on the faith-in-people measure is 1.172, the standard deviation is 1.117, and scores range from 0 to 3. The wording of the items and the method of calculating the combined measure is given in the notes to Table 6. Three additional items from Rosenberg's misanthropy scale were included in the survey. These were agree-disagree items rather than balanced items with arguments and counterarguments; they appear to have been subject to a substantial amount of acquiescence response bias. We

exclude them from the analysis for this reason.

25. This was one of a series of questions: "We are interested in the kinds of things people in the Soviet Union discuss with one another. Please look at this card, and tell me with which of these people you could have [vy mogli by] talked to during your last normal period about . . . the latest fashions or sports event? where to buy clothing on the black market? criticizing a government official?" The card listed the five groups mentioned in the text, and multiple answers were recorded for each question.

26. We performed a similar analysis of the impact of intimidation on individual contacting of government and party organs and on the use of *blat* or *protektstva* (connections) in such bureaucratic encounters. The results suggest that the variables we examine here (in particular, perceptions of the competence of the KGB and of interpersonal trust) have little effect on such behaviors. Contacting involves a distinct type of activity with different costs and benefits that mobilize the public in a different way.

27. We counted *leadership* in a trade union as a conventional activity; but simple membership was excluded since it is all but mandatory. Party membership could not be included: in pretests of the questionnaire, a direct question about whether respondents belonged to the Party proved to be so sensitive that it was omitted from the survey. However, our measure of whether the respondents held Party *nomenklatura* posts in the LNP is a partial substitute for such a variable.

28. For each type of activity during the LNP, respondents were given one point if they belonged but never attended meetings, two points if they attended sometimes, three points if they attended regularly, and four points if they were leaders. For Komsomol members, the questionnaire asked only whether the respondents had ever belonged and whether they had been leaders. We counted only those leaders and members who had been in the Komsomol during their LNP.

29. The summary measure assigns two points for each type of nonconformist activity in which the respondent participated (but did not lead) during the LNP and four points for each type of activity in which the respondent took a leading role.

30. Our measure of intimidation is a binary variable derived from the question on competence of the KGB. Respondents who reported that *most* or *almost all* of the leaders of the KGB were competent are assigned a value of one; respondents who stated that *some*, *hardly any*, or *none* of the leaders of the KGB were competent are assigned a value of zero. Responses coded as *don't know*, *not ascertained*, and *refused to answer* are coded as missing.

31. A majority of the cases are scored as zero on each dependent variable. This suggests that it would be appropriate to use a method other than ordinary-least-squares (OLS) regression to test for the sources of compliant and nonconformist activism. In

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analyses not shown here, we applied probit analysis to test the same hypotheses. We tested first whether people engaged in *any* kind of nonconformist political behavior and then whether those who engaged in any such behavior were likely to become leaders. The results suggest that the independent variables play different roles in accounting for transitions across different thresholds of nonconformity. Although there are some differences in the relative strengths of the independent variables when this

alternative technique is used instead of OLS, the basic conclusions suggested by the probit analysis are similar to those produced by OLS.

32. However, we tested whether political nonconformists were less likely to be punished if they held a Party *nomenklatura* post or were compliant activists. The answer is no: the incidence of punishment depended more on the type of nonconformity than on job status or participation in approved activities.

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HOW ARE FOREIGN POLICY ATTITUDES STRUCTURED? A HIERARCHICAL MODEL

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It has long been assumed that foreign-policy attitudes of the mass public are random, disorganized, and unconstrained if they exist at all. Further, foreign-policy thinking has not been found to be structured along standard ideological (liberal-conservative) lines, partisan lines, or class lines. We attempt to move the discussion from a question of whether foreign-policy attitudes are structured to a question of how they are structured. We propose and estimate (using a LISREL model) a hierarchically organized foreign-policy belief structure in which specific policy preferences are derived from postures (broad, abstract beliefs regarding appropriate general governmental strategies). These postures, in turn, are assumed to be constrained by a set of core values about the international community.

It is by now accepted as a truism that structure and consistency are highly desirable properties of a political belief system—carrying important implications for the competence of the average citizen as well as the functioning of the democratic process. At the individual level, citizens with more consistent and highly related attitudes are assumed to be more thoughtful and efficient in their political reasoning. And at the system level, communication between political elites and the mass public is assumed to be greatly facilitated when the electorate imposes a common and meaningful structure on its attitudes.

Because of the importance of the topic, the past two decades have produced numerous studies investigating the degree to which citizens demonstrate any meaningful structure in their political attitudes (see Asher 1984; Kinder 1983; and Niemi and Weisberg 1984 for chronicals). The

seminal research was conducted by Converse (1964), who found very little consistency either among domestic attitudes, among foreign-policy attitudes, or between the two policy domains. As one recent reviewer of the debate has concluded, if we take ideological constraint to mean that specific policy positions are deduced from widely shared, sweeping ideological principles, then the consensus of research clearly indicates that most U.S. citizens are "innocent of ideology" (Kinder 1983, 401). For example, it is evident that specific issue positions in such diverse domains as social-moral policy, economic policy, and foreign policy do not derive from the single liberal-conservative dimension for which Converse searched in vain.

Yet, we would argue that it is inappropriate to conclude that mass attitudes lack any sort of meaningful structure; instead, analysts must move beyond the narrow

operationalization of constraint adopted by Converse to discover ways that citizens actually do organize their political attitudes.¹ Both the theoretical literature on schemata and the empirical findings of constraint research suggest that substantially more evidence of structure among political attitudes may be uncovered by adopting an approach that first, is domain specific and second, focuses on the connections between general and specific idea elements in a given domain. The recent schemata literature, for example, indicates that "domain-specific information is structured in memory in cohesive 'kernels' of thought" (Hamill, Lodge, and Blake 1985, 851). This work suggests that individuals bring different rules, criteria and processes to bear in different policy domains. Hamill, Lodge, and Blake, for instance, find that while individuals seem to structure their attitudes on economic issues according to a "rich-poor" schema, they are more likely to employ an ideological or partisan schema to structure attitudes on noneconomic issues. It is unlikely, consequently, that individuals would employ a generic criteria, or ideology, to structure attitudes in all policy domains.

The schema literature also suggests that attitude structure within a domain is more usefully studied by focusing on the relationship between general and specific attitudes (*vertical constraint*) than by focusing on the relationship between idea elements at the same level of abstraction (*horizontal constraint*). Briefly, schema theory assumes that people are "cognitive misers" who have limited ability for dealing with information and thus must use their old, generic knowledge to interpret new, specific information. From this perspective, attitude structure centers primarily on the linkages between abstract and concrete idea elements, where the former are assumed to "constrain" the latter. This emphasis is reminiscent of Converse's (1964) original conceptualization

of (psychological) constraint, which assumes that specific beliefs and attitudes derive from more "superordinate values or postures" (p. 211). This perspective provides a good fit to the available evidence generated from constraint research: studies which measure vertical constraint find considerable consistency between policy positions and more general idea elements (e.g., Conover and Feldman 1984; Lane 1962; Marcus, Tabb, and Sullivan 1974; Peffley and Hurwitz 1985), while research that measures horizontal constraint, or the relationship between attitudes at the same level of abstraction (e.g., Converse 1964), typically finds little consistency.

For these reasons, we argue that it is more productive to examine attitude structure using an intensive, domain-specific approach than an extensive, inter-domain approach. We have selected the foreign-policy domain for the focus of our examination for two related reasons. In the first place, foreign-policy attitudes have traditionally been assumed to be underdeveloped and disorganized, as will be discussed. Yet, more recent studies indicate that foreign-policy attitudes do exhibit structure, even if the structure does not necessarily conform to the neat liberal-conservative pattern envisioned by Converse and others.

Second, we examine the foreign-policy domain because, at present, we lack an adequate understanding of its antecedents and correlates. Standard approaches to the study of domestic attitudes are often found to be lacking when applied to foreign affairs. Partisan and liberal-conservative identifications, for example, are typically poor predictors of foreign-policy attitudes. And while social cleavages and group identifications are useful in predicting many domestic issues, social groups have not been found to differ greatly on foreign-policy issues, and the differences that do arise are often difficult to explain (Gamson and Modigliani 1969;

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Hamilton 1972). One's foreign-policy attitudes do not seem to be a matter of simple self-interest, either (e.g., see Lau, Brown, and Sears 1978). Our lack of knowledge in this important area is especially puzzling in light of much recent evidence that foreign-policy attitudes play a crucial role in influencing various forms of political evaluation, such as presidential voting (Aldrich, Sullivan, and Borgida 1986; Kessel 1984) and presidential support (Hurwitz and Peffley 1987).

It is our purpose to explore the properties of foreign-policy belief systems. Specifically, using a multiple indicator (LISREL) model, we estimate levels of foreign-policy issue consistency to determine whether the electorate is really as undisciplined and thoughtless in its approach to foreign-policy reasoning as suggested and to determine what general beliefs, if any, the mass public uses to structure more specific attitudes in this very important domain.

Background

The Earliest Work

The finding that U.S. citizens are not particularly sophisticated about foreign-policy matters is longstanding, going back to Almond's (1950) presentation of his "mood theory," where he argued, "foreign policy attitudes among most Americans lack intellectual structure and factual content. Under normal circumstances the American public has tended to be indifferent to questions of foreign policy because of their remoteness from every day interests and activities. . . . Foreign policy, save in moments of grave crisis, has to labor under a handicap; it has to shout loudly to be heard even a little" (pp. 69-71). A mass of survey evidence supports Almond's thesis that U.S. citizens are often unconcerned and uninformed

about foreign affairs (Erskine 1963; Free and Cantril 1968; Simon 1974). Moreover, the question of whether mass foreign-policy attitudes lack intellectual structure was answered in the affirmative with the publication of Converse's 1964 study which, as noted above, found little consistency among foreign-policy attitudes and even less consistency between foreign and domestic attitudes. Just as bothersome were Converse's (1964) findings that foreign-policy attitudes were not only unstable over the waves of the 1956-58-60 National Election Studies (NES) but were even less stable than domestic attitudes. Instability was also found to be the hallmark of foreign-policy attitudes in a more recent study by Converse and Markus (1979). Taken together, then, this evidence indicates little in the way of structure, integrity, or solidity in citizens' foreign-policy cognition.

More Recent Evidence

Scholars began to reexamine the structure of foreign-policy attitudes during the Vietnam War period. One of the first of these more recent studies, conducted by Verba et al. (1967) found Vietnam-related public opinion to be "relatively orderly. . . . The correlational analysis among scales shows patterns of consistency among the population" (p. 330). A dove-hawk continuum proved to be important for organizing the attitudes of many of the respondents. Verba et al.'s findings encouraged a large literature examining the structure of foreign-policy attitudes (e.g., Bardes and Oldendick 1978; Bennett 1974; Maggiotto and Wittkopf 1981; Modigliani 1972; Wittkopf 1981), which, almost without exception, supported the conclusion that foreign-policy attitudes are substantially more organized than Converse had determined them to be.

Skeptics of this recent literature might argue that findings of attitude structure

we now have several types of evidence that Converse may have seriously underestimated attitudinal stability, in general; more specifically, we also have evidence that foreign-policy opinions are every bit as stable as their domestic-policy counterparts. Achen (1975), Erikson (1978, 1979), and others argue that response inconsistencies over time are due primarily to random measurement error in the survey items used to assess these attitudes. When over-time correlations of foreign-policy attitudes in the 1956-58-60 NES panel (foreign aid, military involvement, and isolationism in Achen's study) are corrected for measurement error, they are quite high, approaching 1.0.²

In short, there is mounting evidence that foreign-policy attitudes are considerably more stable and organized than early research had found. This recent literature, in addition to its disagreements with the conclusions of Converse, shares several other characteristics. In the first place, the studies do not assume that foreign-policy attitudes are structured unidimensionally along the same liberal-conservative continuum on which attitudes in other domains fall. Rather, with few exceptions, people were found to use multiple dimensions to organize their foreign-policy beliefs—dimensions specific to the domain of foreign affairs, such as militarism, isolationism, and liberal internationalism, to name but a few. Second, many of the studies focus on the relationship between general and specific attitudes (vertical, rather than horizontal, constraint). Much of this work relies on exploratory factor analysis to uncover superordinate constructs (or factors) capable of explaining the covariation between specific foreign-policy opinions. And, as argued above, it is far more realistic to expect attitude structure, if it exists, to be vertical than to be horizontal. are specious, given Converse's (1964) discovery that foreign-policy attitudes are so remarkably unstable over time. In fact,

Some Problems with the Literature

There is, then, impressive evidence that the foreign-policy thinking of individuals is structured; unfortunately, though, the recent literature is plagued by a number of methodological and conceptual problems that must be addressed. In the first place, as noted, much of the work employs factor analysis to uncover evidence of attitude structure, a technique with several notable shortcomings. For one, the final solution of any exploratory factor model is heavily influenced by essentially arbitrary mathematical constraints rather than substantive or theoretical ones (Long 1984). Another drawback of the technique is that interpreting the substantive content of a factor involves a certain amount of arbitrariness on the part of the analyst (Long 1984); merely supplying a label of, say, *militarism* to a factor provides no new insight into the exact meaning of the construct.

A more general problem with the research, both factor analytic and correlational, is that it is largely descriptive, with few examples of theory-guided discussion. While there is a general consensus that foreign-policy attitudes are somehow interrelated, few authors have paid much attention to the questions of why or how they are related. And while there seems to be general agreement that belief structures are largely vertical (i.e., that specific foreign-policy attitudes are bound by more abstract, general beliefs), there is very little discussion of the utility of such an organization for the individual. Thus, while the recent work in the area has been encouraging and has demonstrated the inappropriateness of Converse's methods and conclusions, a great deal more needs to be done to determine precisely how, and why, foreign-policy attitudes are linked together.

In order to answer these questions and others, we begin with a realistic appraisal of the process by which the ordinary

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citizen attempts to make sense out of the complex international environment. The difficulties inherent in gaining an understanding of such a complicated universe argue for a hierarchical model of political reasoning, wherein citizens use general and abstract beliefs to constrain more specific foreign-policy attitudes and preferences.

An Information-Processing Perspective

We base our investigation of how people make sense out of international relations on an information-processing approach. Grounded in cognitive-psychology and social-cognition literatures, the information-processing perspective assumes that because humans have very severe cognitive limitations, they often behave as *cognitive misers*, coping with their shortcomings by taking shortcuts whenever possible (see Fiske and Taylor 1984 for a review of this perspective). Importantly, an individual's reliance on simplification is assumed to increase with the complexity of the environment. Exceedingly simple stimuli can be processed in a relatively veridical, faithful way, even by a cognitively limited perceiver. But a complicated and confusing world requires extensive use of heuristics to render the environment interpretable and manageable.

Among the various policy domains that comprise the political environment of the average citizen, the international sphere is exceptionally complex and ambiguous. International events that occur around the globe are remote, fluid, and extraordinarily complicated. They are also ambiguous in that information regarding the international scene is often distorted or deliberately withheld from the public. Consequently, citizens are forced to rely on the assessments of U.S. political elites and media commentators, who interpret

world events but who also, quite often, disagree with one another. In short, the complexity of international affairs makes the policy domain a very ambiguous and difficult one for the public to follow, at least without having to pay substantial costs.

Owing to the complexity of the domain, *cognitive heuristics* (information short-cutting) is of great importance in explaining how a "typical citizen" processes foreign-policy information, makes decisions on foreign-policy issues, or structures foreign-policy beliefs. Yet the standard guidelines used to make sense of politics (and political issues) have not been found useful in the foreign-policy domain. It has frequently been argued, for example, that many individuals support policies that enhance their well-being or self-interest. However, the connections between one's self-interest—narrowly defined—and foreign affairs are, doubtless, even more difficult to determine than in the domestic sphere. Thus, it is not surprising that studies have found only very weak relationships between self-interest and foreign-policy attitudes (Mueller 1973; Lau, Brown, and Sears 1978). Other common criteria by which policies are often evaluated include partisanship, liberal-conservative ideology, and social class. But, as noted previously, Converse found that foreign-policy attitudes are not organized along the liberal-conservative dimension; as well, he found virtually no systematic or predictable difference between Democrats and Republicans on foreign-policy issues, a finding replicated in more recent years (e.g., Pomper 1975; Verba et al. 1967). And with the exception of isolationist sentiments, foreign-policy opinions have not been found to be connected to social status (e.g., Gamson and Modigliani 1969).

In short, political judgments in the international sphere must be made under considerable uncertainty and without many of the interpretive aids commonly

used in the domestic arena. It would nevertheless be a mistake to assume that in the face of this uncertainty, most citizens will refrain from holding foreign-policy attitudes or will simply embrace "official" governmental policies as their own. Following the literature on social cognition in social psychology, we assume that people cope with uncertainty in international affairs much the same way they do in other content domains—by relying on their store of general knowledge to guide the processing of more specific information.

Among researchers who study schemata, such general-to-specific reasoning has been called "top-down" or "theory-driven" processing and is usually represented by a hierarchical structure in which abstract beliefs influence the response of the individual to more specific stimuli. It is theory driven in the sense that under conditions of uncertainty, people are assumed to behave as cognitive misers by using old, generic knowledge to interpret new, specific information. General beliefs permit economical judgments to be made under uncertainty because they provide interpretations for ambiguous stimuli, select information for storage or retrieval from memory, and "fill in" missing or ambiguous information with best guesses or default values (Taylor and Crocker 1981; Tversky and Kahneman 1981).

There are several examples of such hierarchical models of political reasoning in foreign affairs. Conover and Feldman (1984) found that their sample of university students used more general schemata to organize their more specific policy preferences in foreign affairs and other domains. And in his computer simulation of foreign-policy reasoning, Carbonell (1978) investigated a hierarchical belief structure in which higher-level goals (e.g., Communist containment) partially determine more specific policy goals (e.g., increasing military strength). Finally, in

his study of elite foreign-policy decision making, Jervis (1976) argued that the general beliefs held by diplomats and other experts exerted a powerful effect on their interpretations of foreign-policy events. These studies provide preliminary evidence that encourages us to investigate such hierarchical models as are common to the mass public with a conventional survey approach.

A Hierarchical Model of Foreign-Policy Attitudes

We expect citizens' foreign-policy attitudes to conform generally to the hierarchical model of constraint presented in Figure 1, where more abstract idea elements constrain more specific ones. At the most concrete level (the bottom or third tier of the figure), are preferences for specific foreign-policy issues—defense spending, nuclear-arms policy, international trade, and policies toward the USSR. Constraining these attitudes at the second (middle) tier of the attitude hierarchy are more abstract beliefs concerning the appropriate role of government in its handling of foreign affairs. These normative beliefs are collectively referred to as *postures*, for they convey the general stance or orientation the individual would like to see the government adopt in conducting foreign policy, without indicating what specific policies should be employed to attain the desired goal. Should the government adopt an aggressive or an accommodating stance in dealing with other countries? Should the government chart an internationalist or an isolationist course in international affairs? These are the basic themes defined in the factor-analytic studies. By relying on such general prescriptions to render more specific preferences, citizens are able to categorize and to evaluate economically a wide variety of concrete policies on the basis of whether those policies are consistent with their more general postures.

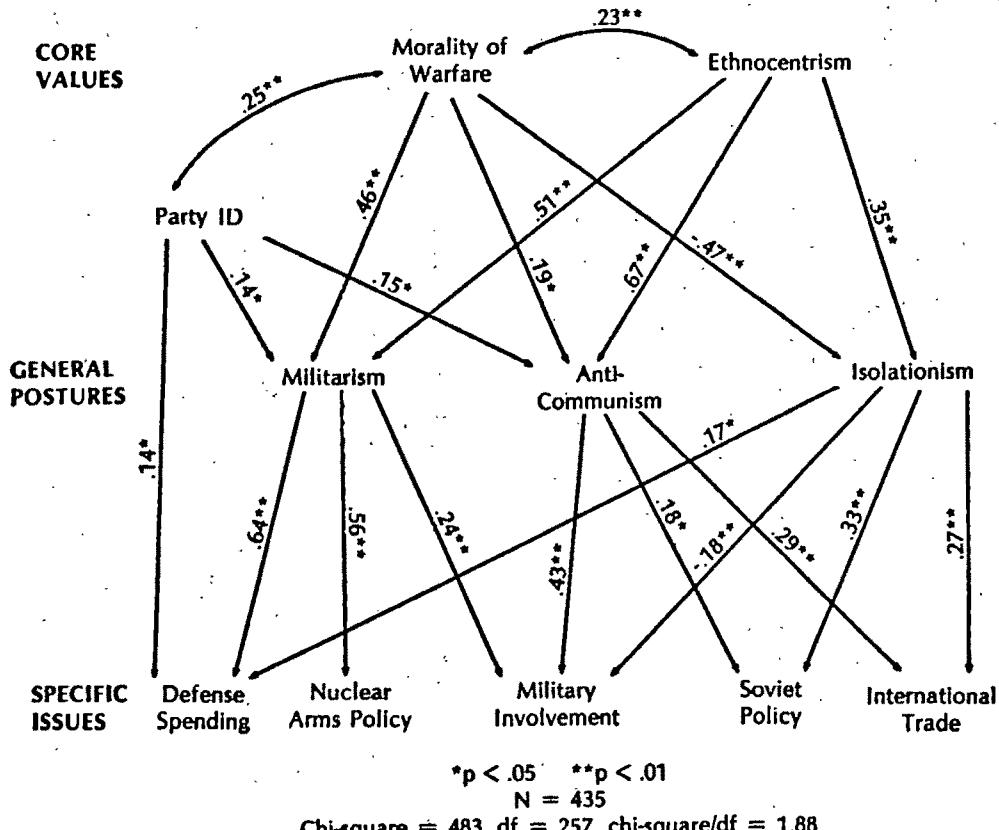
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Thus, to use a simple example, whether an individual favors sending U.S. troops to Central America will depend in large part on whether the individual favors an isolationist or an interventionist posture in world affairs.

What more fundamental idea elements might constrain foreign-policy postures? At the first (and uppermost) tier of the

hierarchy in Figure 1 are what we call *core values*, such as ethnocentrism and moral beliefs about killing in warfare. Core values are easily distinguishable from postures in that they do not refer directly to governmental actions or policies. While postures are normative beliefs regarding a government's behavior, values are more personal statements

Figure 1. Structural Relationships between Foreign-Policy Attitudes in Hierarchical Model of Constraint



Note: Coefficients are standardized betas estimated by full-information maximum likelihood. Estimates were computed with LISREL 6. All nonsignificant paths are omitted from the figure. Respondents scoring on the lower end of the variables are more accepting of warfare, ethnocentric, Republican, militaristic, anti-Communistic, isolationist, supportive of defense spending, opposed to arms control, supportive of military involvement in Central America, opposed to relations with Russia, and in favor of trade restrictions.

regarding the individual's priorities and concerns. The belief that one's country is superior to all others (i.e., ethnocentrism), for example, carries with it no explicit reference to governmental policy, in either broad or specific terms. Conceptually, values (and the fundamental beliefs that link values to more specific attitudes) have been assumed to occupy a central position in belief systems (Kinder 1983). Rokeach (1973) has argued, for example, that values "lead us to take particular positions on social issues" (p. 13), and Allport (1961) notes that "attitudes themselves depend on preexisting social values" (pp. 802-3). Several recent studies have found empirical support for this view. Values of economic individualism, racial equality, and gender equality have been found to be an important source of attitudes toward welfare policies, civil rights, and women's issues, respectively (Feldman 1983; Kinder and Sears 1981; Sears, Huddy, and Schaffer 1986; Sniderman, Brody, and Kuklinski 1984).

The particular postures and fundamental values that occupy the upper reaches of the hierarchical model will be described more fully below. At this point we wish to emphasize a characteristic shared by these two idea elements that makes them especially attractive as interpretive aids: they are "easy,"⁴ largely because a deep familiarity with foreign-policy events and information is not a prerequisite to possessing a belief at either the value or the postural level. Postures should make intuitive sense to even a rank amateur in the international domain, while fundamental values, by their very nature, are based on personal beliefs that are central to one's world view and are not dependent on substantial expertise in foreign affairs.

Moreover, at least two of the dimensions, ethnocentrism and anticomunism, may rest primarily on an individual's affective orientation to the United States (i.e., ethnocentrism) and its prin-

ciple adversary, the Soviet Union (i.e., anticomunism). While this affect may be based on more philosophical principles for some individuals, it need not be. By simply knowing one's likes and dislikes, it should be a relatively easy task to place oneself on dimensions like ethnocentrism and anticomunism.⁵ Finally, values and postures should be relatively easy dimensions for the public to use in view of the likelihood that most public rhetoric in the area of foreign policy will be pitched at a general, symbolic level rather than at the concrete, policy level.

By way of summary, we note that investigating this hierarchical model of foreign-policy constraint allows us to overcome many of the limitations of previous research. Most importantly, in contrast to many factor-analytic studies, we directly measure the relationship between general and specific elements in a foreign-policy belief system. Our focus on foreign-policy attitudes also allows us to investigate a variety of vertical linkages between abstract and concrete elements that might be missed in a more superficial analysis. Further, we explore a common framework of ideological structure that is capable of being assessed using a conventional survey approach.

Methods

Sample and Measurement

The data for investigating the structure of foreign-policy attitudes come from a telephone survey conducted by the Survey Research Center (SRC) at the University of Kentucky, Lexington during February 1986. A variant of random-digit dialing was used to select the 501 Lexington residents who comprised the sample.⁶ The professional interviewers at the SRC asked respondents a battery of questions concerning their attitudes on foreign affairs during the course of each inter-

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view, which lasted an average of 10 to 15 minutes.

To estimate our hierarchical model of foreign-policy constraint, it was necessary to measure foreign-policy attitudes at several levels of abstraction in order to tap not only specific policy preferences but also foreign-policy postures and core values. The survey questions are presented in the Appendix; the vast majority of items were pretested in a pilot study of Lexington residents ($N = 128$), most of whom were students at the University of Kentucky.

Postures

We expect an individual's concrete issue preferences to be based on three global postures that describe the general direction the government should take in international affairs: *militarism*, *isolationism*, and *anticommunism*. Our selection of these postures has been motivated by the work of Kegley and Wittkopf (1982), Maggiotto and Wittkopf (1981), Schneider (1984), and others who have written about foreign-policy attitudes from a historical perspective. It is the contention of these authors that the contemporary U.S. public makes two basic foreign-policy choices: (1) whether or not to become involved with other nations and (2) if involvement is desired, whether the involvement should be militaristic or non-militaristic (e.g., diplomatic). Consequently, we include the isolationism and militarism postures to represent these two basic choices the mass public must face.

If *isolationism* provides guidance regarding the appropriate extent of involvement and *militarism* provides guidance regarding the type of involvement, *anticommunism* provides guidance regarding the targets of involvement. A central question in U.S. foreign policy in the postwar period has been, What is the appropriate posture of the United States toward Communist-bloc nations? Since

World War II, the Soviet Union and Communist countries have been viewed by U.S. presidents as the major international threat to the nation's interests and security (Kegley and Wittkopf 1982); and most major foreign-policy developments over this period (e.g., containment, détente, and others) have been justified as a means of coping with this threat.

All of these dimensions have consistently shown up in factor-analytic studies as superordinate constructs structuring more specific foreign-policy attitudes. Moreover, we believe these broad dimensions underlie much of the public discourse in the international sphere.⁷ Our definition and operationalization of each construct deserves some elaboration.

Militarism. The dimension of *militarism* is anchored, on the one end, by a desire that the government assume an assertive, militant foreign-policy posture through military strength and on the other by a desire for a more flexible and accommodating stance through negotiations. The specific items designed to tap this dimension emphasize "toughness" as opposed to "flexibility" in dealing with other countries (A.1 in the Appendix), the view that the best way to achieve peace is through military strength rather than negotiations (A.2), and the belief that the United States should be willing to "go to the brink of war" to preserve its military dominance (A.3). We expect this dimension to be an important predictor of the contemporary divisions that exist in public support for either a defense buildup at home or the use of U.S. military force overseas.

Anticommunism. Beliefs about the appropriate posture of the United States toward Communist-bloc countries should also play a central role in shaping concrete policy attitudes (see Gamson and Modigliani 1969 for corroborating evidence on this point). Four items were used to assess *anticommunism*: respondents were asked

whether the United States should do everything it can to "prevent the spread of communism" (B.1) or "check the spread of Soviet influence" (B.2) in the world, whether communism "is an acceptable form of government for some countries" (B.3), and whether or not the United States should try hard to "get along with Russia" (B.4). This posture should predict not only specific policy preferences in U.S.-Soviet relations, but also specific defense spending, military involvement, and nuclear-arms attitudes.

Isolationism. The *isolationism* posture is defined by a general desire that the government avoid any ties or entanglements with other countries, whatever the nature of the relationship (i.e., whether political, diplomatic, or militaristic). Since isolationism has longstanding roots in U.S. history (see McCormick 1985), it is not surprising that it has been used with some frequency to describe the foreign-policy orientations of the U.S. public (see, for example, McClosky 1967 and Sniderman and Citrin 1971).

While a number of isolationism scales exist, commonly used indexes either rely on questions too specific for use at the postural level (e.g., McClosky 1967) or rest on the questionable assumption that isolationism and internationalism lie at opposite poles of a single dimension (e.g., Watts and Free 1978). To avoid such problems, isolationism was measured by two Likert-scale items that expressed only isolationist (not internationalist) sentiments pitched at a fairly general level of abstraction. Thus, isolationists in our study are more likely to agree that "we shouldn't risk our happiness by getting involved with other countries" (C.1) and that the United States should turn inward and focus on problems at home rather than world affairs (C.2). This posture should constrain any policy that pertains to international involvement.

Specific Policy Preferences

To establish the range of applicability of our postural variables, we asked respondents to indicate their more specific foreign-policy preferences in several areas, including defense spending, nuclear-arms policy, military involvement (in Latin America), policy toward the Soviet Union, and international trade. The measurement of these more specific attitudes was an easier task, since there are a wealth of possible survey items to be culled from various national surveys, such as those used by the National Election Studies and the Chicago Council on Foreign Relations. As quick perusal of the appropriate section of the Appendix demonstrates, we followed the practice of measuring each policy attitude (except Soviet relations) with several indicators.

Core Values

One's placement on the postural dimensions is assumed to be guided largely by two core values: ethnocentrism and the morality of killing in warfare.

Ethnocentrism. Ethnocentrism is defined here as the belief that one's country is superior to all others; we expect such beliefs to underlie all three of the postures we have included in this study.⁸ If an individual truly believes that the United States is vastly superior to other countries in the world, this belief would certainly bolster the idea that the appropriate posture of the government should be the aggressive pursuit of our national interests (i.e., militarism) as well as the protection of our system from the threat posed by Communist countries, whose interests and values are seen to be antithetical to ours. Moreover, to the degree that ethnocentrism fosters a self-centered or parochial view of the world, the tendency may be to draw inward into an isolationist shell rather than to push outward in the world.

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The questions measuring ethnocentrism appear in the Appendix; they ask respondents whether other countries should try to make their governments as much like ours as possible (I.1), whether they agree that our government is the best form of government yet devised (I.2), whether other countries have governments that are just as good as ours (I.3), and how the United States compares with other countries in the world (I.4).

Morality of Warfare. Beliefs about the morality of killing in warfare are also presumed to constrain the postures described above. Clearly, one's willingness to endorse a militaristic posture should be related to one's values regarding the "rightness" of military solutions. Moreover, a strong sense that warfare is wrong should lead individuals to reject internationalism, given the tendency for many types of involvements to become military in nature. And finally, we would expect those most wary of warfare to advocate more restraint in dealing with the Soviets given the possibility that an aggressive containment policy with the Soviet Union could result in exacerbating, rather than alleviating, international tensions.

Briefly, an individual's values concerning the morality of warfare—whether taking the life of one's enemy is viewed as unconscionable or whether it is seen as a matter of necessity, perhaps even virtue—were determined by asking respondents whether it is "acceptable to kill one's enemies when fighting for one's country" (J.1) and whether a "person who loves his fellow man should refuse to fight in any war" (J.2).

As was the case with our selection of postures, we incorporated these two values into our model on theoretical grounds. It can be seen that we selected values of large range, which should be logically linked to all three of the postural positions. This is not to suggest that our list of values is exhaustive, as other values

(liberty, for instance) can and should be explored.

Methodology

We used Joreskog's (1973) method for the analysis of covariance structures (LISREL) to estimate the hierarchical model of foreign-policy attitudes.⁹ The covariance-structures model holds several advantages over other techniques used to estimate attitude structure and constraint. For one, the arbitrary mathematical constraints imposed on the exploratory factor model are replaced in LISREL with substantive constraints that can be "tested" with the data at hand. In addition, when one has multiple indicators of many of the idea elements in the constraint model, as we do, problems of measurement error that have plagued earlier studies of constraint (see, e.g., Achen 1975; Erikson 1978, 1979) are not so severe, since the structural relationships between latent idea elements estimated by LISREL are, in a sense, corrected for attenuation due to random measurement error.

Model Specification

Our model assumes that more concrete idea elements are constrained, or determined by more abstract attitudes at the next higher level of abstraction. Thus, each of the postures is assumed to be constrained by both of the core values, and each of the specific policy attitudes is assumed to be constrained by all three of the postures. The exogenous variables in the model, the two core values, are linked by unexplained covariances.

Partisanship and liberalism-conservatism were also included in the model as exogenous variables influencing postures and policy preferences alike. These variables, measured in the standard seven-point format developed by the NES-SRC, are included in the model for two reasons: We have noted, first, that past research

suggests that foreign-policy attitudes do not seem to be organized around these more domestic orientations. We should like to see whether this independence holds for the foreign-policy attitudes measured in this study. Second, these factors serve as control variables in the model to ferret out spurious associations between foreign-policy attitudes related only through their mutual dependence on partisanship and liberalism-conservatism.¹⁰

Findings

The estimated epistemic correlations between the indicators and the latent idea elements are presented in Table 1. Generally, the indicators appear to be fairly reliable measures of the theoretical constructs.

Figure 1 presents the LISREL estimates of the structural relationships between idea elements at different levels of

Table 1. Epistemic Correlations between Indicators and Factors

Theoretical Construct	Indicator	Epistemic Correlations	Construct R ²
General Postures			
A. Militarism	1. be tough or be flexible 2. negotiate or maintain military strength 3. maintain dominance	.55 .50 .55	.69
B. Anticommunism	1. contain communism 2. contain soviets 3. communism acceptable 4. get along with Russia	.75 .65 .41 .25	.61
C. Isolationism	1. avoid involvements 2. not worry about world affairs	.60 .75	.30
Specific Issues			
D. Defense spending	1. level of spending 2. defense or nondefense cuts	.61 .69	.52
E. Military involvement	1. Central America 2. El Salvador	.78 .73	.37
F. Nuclear arms	1. nuclear freeze 2. nuclear-weapon increase	.56 .77	.43
G. Soviet relations	1. cultural ties	1.0	.18
H. International trade	1. import restriction 2. job protection	.78 .62	.18
Core Values			
I. Ethnocentrism	1. others should imitate U.S. 2. U.S. government best 3. other governments as good 4. how U.S. compares	.61 .46 .46 .36	
J. Morality of war	1. killing acceptable 2. should refuse to fight	.58 .55	

Note: Coefficients are standardized *betas* estimated by full-information maximum likelihood. Estimates are computed from LISREL 6.

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abstraction. Overall, the hierarchical model fits the data quite well. The chi-square-to-degrees-of-fit statistic is 257 and, with 482.87 degrees of freedom, the chi-square-to-degrees-of-freedom ratio is 1.88.¹¹ The most general feature to note about the results in Figure 1 is the high level of interconnectedness between foreign-policy idea elements. In almost every case, attitudes at the postural and specific policy levels are significantly tied to at least two of the idea elements measured at a higher level of abstraction. And in several instances, the linkages are quite strong. Finally, as indicated by the r-squares in Table 1, a substantial proportion of the variation in most of the endogenous attitudinal elements (i.e., postures and specific policy preferences) is "explained" by more abstract beliefs (i.e., core values and postures, respectively). This is no small achievement in light of the expectations created by prior studies. Aside from the factor-analytic studies, a generation of research has cast severe doubt on the prospect of finding any evidence of consistency among foreign-policy attitudes. The general pattern of our results, however, clearly indicates that when constraint is measured as a series of vertical relationships between attitudes at different levels of abstraction, the degree of structure among foreign-policy attitudes is generally impressive.

The size and significance of the relationships between foreign-policy attitudes stand in striking contrast to the estimated linkages between these attitudes and partisanship and liberalism-conservatism. Our results thus echo those of previous studies, which failed to detect much evidence of a connection between these identifications and foreign-policy preferences; not only are most of the linkages statistically insignificant (and thus omitted from the figure) but those that are significant are uniformly low. Independent of their core values, for example, Republicans are only slightly more likely than

Democrats to opt for militaristic and anti-Communist postures in the international arena; and independent of their postures, they are only marginally more likely to favor increases in defense spending. Significant liberal-conservative differences (not shown) emerge only on the *militarism* dimension.¹²

Core Values and Postures

A closer inspection of the connections among foreign-policy attitudes turns up a number of interesting findings. Focusing first on the many arrows connecting idea elements at the top of Figure 2, it is evident that individuals' core values are an important foundation of their foreign-policy postures. Collectively, the two values (along with party identification and liberalism-conservatism) account for between 30% and 70% of the variance in the postures.

The relationships uncovered here provide valuable insights into the various ways that core values anchor the general orientations people develop to guide their foreign-policy thinking. And for the most part, the linkages that emerge are consistent with our conceptualization of the particular postures and core values included in the study. For example, as one might predict, respondents desiring a more militant stance in world affairs are likely to reject the idea that killing in warfare is immoral ($\beta = .46$); they also tend to endorse the ethnocentric view that their country is without equal in the world ($\beta = .19$).

Both of the core values affect the degree to which an individual prefers an isolationist posture in world affairs. One fairly strong basis for isolationism is an expressed concern about killing in warfare ($\beta = -.47$). The implication is that the desire to retreat from international affairs may stem, in part, from apprehension about the possibility of international involvements leading to military engage-

ments. We also find that a self-centered belief in the superiority of the United States ($\beta = .35$) underlies isolationist currents in public attitudes.

Ethnocentric values also contribute heavily to individuals' anti-Communist orientations ($\beta = .67$). On reflection, an anti-Communist posture seems a natural counterpart to an ardent faith in the "American way." As Levinson (1957) has argued, "Ethnocentric thinking in the sphere of international relations, like other forms of ethnocentrism, is based on a rigid and pervasive distinction between in-groups and out-groups . . . the focal out-group at any time being those nations who are seen, rightly or wrongly, as different from ours. . . . Other nations are seen as inferior, envious and threatening" (pp. 38-39). For many U.S. citizens, the longstanding "out-group" in foreign affairs has been the Soviet Union and other Communist countries, which many minds represent as an anti-ideal group whose interests and values run contrary to their own.

Postures and Specific Issues

A casual inspection of the lower portion of Figure 2 reveals that the three postures are crucial considerations in guiding an individual's preferences on a variety of concrete issues in foreign affairs. The effect of isolationism is as one might expect: the tendency is to reject consistently those foreign-policy options that call for more active involvement between the United States and other countries. Thus, isolationists, more so than non-isolationists, are opposed to committing U.S. troops overseas ($\beta = -.18$) and to renewed cultural relations with the Soviet Union ($\beta = .33$) but favor the imposition of trade barriers to protect U.S. jobs ($\beta = .27$). On the other hand, isolationists are in favor of increased defense spending. Though this relationship is not particularly large ($\beta = .17$), it is apparent that

the isolationist's preferred method of responding to external threat is to increase the U.S. arsenal on the home front (probably for *defensive* purposes) rather than to increase military (*offensive*) commitments overseas. In sum, while the *isolationism* dimension is a more powerful determinant of nonmilitary positions than of military positions, it still appears to provide an important cue in guiding people's preferences across a wide range of different types of issues.

An individual's affinity for an assertive, versus an accommodating, posture by the government is primarily an important predictor of military-type issues. Very clear and strong differences arise on the issues of defense spending and nuclear policy between militarists and accommodationists, with the former being much more likely to favor general increases in defense spending and to oppose arms policies that require the U.S. to cut back its nuclear arsenal. Individuals who favor a more militant U.S. posture in the abstract also tend to favor sending U.S. troops to various trouble spots under more specific circumstances.

The third posture, anticomunism, shows up as a significant predictor of both military and nonmilitary issues. Individuals favoring a tougher posture toward Communist countries are more likely than not to favor sending U.S. troops to Latin America ($\beta = .43$) and placing restrictions on foreign imports ($\beta = .29$) but oppose resuming cultural ties with the Soviet Union ($\beta = .18$). The rather modest size of the coefficient for Soviet policy may be explained by the tendency for our postural variable to reflect more militant sentiments toward the containment of communism, sentiments that may not be relevant for the rather non-controversial and diplomatic policy expressed in our measure of Soviet policy.

The absence of significant linkages between the anti-Communist posture on the one hand and defense spending and

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nuclear policy on the other is surprising. Nuclear-arms policies presumably involve the Soviet Union in some way and proposals to increase defense spending are often justified on the grounds that they provide a necessary deterrent to the Soviet Union and its allies. Our findings suggest, however, that one's position on these issues is, first and foremost, a reflection of the degree to which one favors an assertive, versus an accommodating, approach to foreign affairs. It may be that defense-spending and nuclear-policy issues are accounted for quite well by the *militarism* dimension, leaving little room for influence from *anticommunism*, which, after all, overlaps considerably with the *militarism* dimension ($r = .60$).¹³

The Degree of Specific-Issue Constraint

Our data also shed some light on the question of what kinds of foreign-policy issues are more apt to be grounded in general considerations and principles. Judging from the r -squares of the issue variables in Table 1, it is clear that individuals' positions on military issues, such as defense spending, nuclear policy, and military involvement, are better accounted for by the three postures than are the nonmilitary issues of international trade and Soviet policy. The differences here may be due to either or both of two reasons. First, it may be that the majority of postures included in this study (i.e., *militarism* and *anticommunism*) are more relevant guides for evaluating military, than for evaluating nonmilitary, issues. Yet it is also difficult to imagine general principles other than the postures measured here that might be used by the average citizen to anchor opinions on issues like international trade and diplomatic relations with the Soviet Union solidly. Liberal-conservative and partisan identifications, for example, provide no significant connective glue on these issues.

Moreover, it should be noted that *isolationism* and *anticommunism* did prove to be important predictors of these issues; the problem is simply that these dimensions failed to account for a substantial proportion of the variance on the non-military issues.

It seems more plausible to make a second argument, that military issues are more highly constrained because they are inherently more threatening to the public, are more often the object of media coverage, and are generally more salient in the mass public's mind. Because the public is more likely to think seriously about issues like military involvement and defense spending, these issues are more likely to be linked to general postures. Under normal circumstances, nonmilitary issues are, as a group, less likely to penetrate the mass public's awareness (Bennett 1974). It is therefore these nonsalient issues that come close to fitting the stereotype of foreign-policy attitudes found in much of the literature, where public thinking is portrayed as being relatively undeveloped and disorganized; though, even here, it is important to keep in mind that non-military issues in this study were not found to be completely random or disorganized, only more so than military issues.

Conclusions and Implications

How do individuals make sense out of a bewildering international environment, given the irrelevance of standard political heuristics? To answer this question, we resisted the assumption of unidimensional ideology by earlier studies, as well as the exploratory procedures of factor analysts. Instead, using an intensive, domain-specific approach, we found the foreign-policy views of respondents in our study to be constrained to an impressive degree. Thus, we find core values like ethnocentrism and the morality of war to structure foreign-policy postures like militar-

ism, anticomunism, and isolationism; which in turn are important determinants of individuals' preferences across a wide range of specific policies, such as defense spending, Soviet relations, involvement of U.S. troops overseas, international trade, and nuclear arms. This means that earlier findings that international attitudes are unrelated to a liberal-conservative dimension should not be interpreted to mean that foreign-policy attitudes lack structure. On the contrary, the general attitudes that constrain specific preferences are distinctive to the foreign-policy domain.

Critics might well argue that it is contradictory to view the electorate as structured and organized in its thinking when, at the same time, it is largely ignorant of the substance of such attitudes (we do not challenge the robust finding that citizens know little about foreign affairs). Sniderman and Tetlock (1986, 79) ask the question, "How is it possible for ordinary citizens to put together a consistent outlook on politics, given that they know so little about it?" They (and we) answer this question by emphasizing the functional nature of heuristics; that is, individuals organize information because such organization helps to simplify the world. Thus, a paucity of information does not *impede* structure and consistency; on the contrary, it *motivates* the development and employment of structure. Thus, we see individuals as attempting to cope with an extraordinarily confusing world (with limited resources to pay information costs) by structuring views about specific foreign policies according to their more general and abstract beliefs.

Our findings should be a source of comfort to those who appreciate the benefits of public involvement in governmental affairs. When Converse initiated the debate over constraint more than two decades ago, he justified the importance of the research by referring to the inherent benefits of mass-elite communication.

When the attitudes citizens possess are not consistent nor tied together by an underlying ideology, the public will not be able to follow the themes and rhetoric of candidates and incumbents, and, moreover, the elected officials will not readily understand what general public impulses helped to carry them to power. The results of this study suggest that the public does possess general orientations that help to inform and anchor its opinions on specific foreign-policy issues.

The results of this study should also provide some relief to analysts who have been frustrated by the seeming "random" nature of political behavior in the international realm, where traditional perspectives for explaining behavior in domestic politics have not been very useful. The postures that respondents used to anchor their specific policy preferences should prove to be important analytical yardsticks for future efforts to interpret and predict mass reactions to foreign-policy events. It is both more powerful and more parsimonious for students of political behavior to examine these general orientations than to focus on the opinions to specific issues, which change over time.

Our study also provides some new insights into the question of when, and under what circumstances, the public will become interested (or disinterested) in world affairs and which issue preferences are likely to be more well developed than others. Some of the specific policies, such as defense spending, military involvement, and nuclear policy, are clearly better explained by the higher-level beliefs (postures) than are others (i.e., the non-military issues). We speculate that one of the reasons why foreign policy appears to be salient during the current administration is that President Reagan has consistently spoken to precisely those military issues most likely to be constrained by the commonly shared postures. In comparison, President Carter focused primarily on less accessible and less salient foreign-

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policy issues—on human rights and non-military international relations—which do not appear to be constrained by the postures. Thus, political elites who focus on military concerns or who frame their appeals in terms of popular themes, like militarism, anticommunism, and isolationism, are at least assured of a wider public audience. Taking some exception with Almond's (1950) earlier conclusions, we would argue that foreign policy does not necessarily have to "shout loudly to be heard only a little"; it simply has to speak clearly and in familiar terms, perhaps rattling a few sabers for emphasis.

Finally, there are several limitations on the present study that deserve special mention. In the first place, our sample overrepresents better-educated individuals, who may be expected to exhibit a greater degree of interconnectedness on political attitudes. Further, we must acknowledge the possibility that constraint is higher during an administration noted for its controversial, bold, and consistent style of international relations; there is little mistaking the president's feelings toward the Soviets, toward military buildup, or toward support for regimes friendly to the U.S. We mention these caveats not because we believe our findings are inherently time bound or lacking in external validity but rather because we recognize the need to replicate these results across different regimes and different populations.

Appendix: Lexington Survey Items

For *Some people . . . Others . . .* statements, respondents were asked whether they *agreed* or *disagreed* with the statement and whether they agreed or disagreed *strongly* or *not so strongly*, producing a five-point scale with *uncertain* and *don't know* responses at the midpoint of the scale. For straightforward state-

ments, respondents were asked which viewpoint came closer to their own opinion and whether it was *somewhat close* or *very close* to their own view, producing a five-point scale with *depends* and *don't know* responses at the midpoint of the scale. *Don't know* responses were assigned a value equal to the midpoint of the scale for all of the five-point scales. On average, the percentage of *don't know* responses was fairly small—just above 3%, or about 16 cases—ranging from a low of 1% to an atypical high of 12%.

General Postures

A. Militarism

1. Some people feel that in dealing with other nations our government should be strong and tough. Others feel that our government should be understanding and flexible.
2. Some people feel that the best way to ensure peace is through military strength. Others feel that the best way to peace is to sit down with other nations and work out our disagreements.
3. The U.S. should maintain its dominant position as the world's most powerful nation at all costs, even going to the brink of war if necessary.

B. Anticommunism

1. The United States should do everything it can to prevent the spread of communism to any other part of the world.
2. The United States should do everything it can to check the spread of Soviet influence in the world.
3. Communism may have its problems, but it is an acceptable form of government for some countries.
4. Some people say that our government should try very hard to get along with Russia. Others say that

it would be a mistake to try very hard to get along with Russia.

C. Isolationism

1. We shouldn't risk our happiness and well-being by getting involved with other nations.
2. The United States shouldn't worry about world affairs but just concentrate on taking care of problems here at home.

Specific Issues

D. Defense Spending

1. Considering the situation today at home and abroad, do you think the total amount the United States is spending for defense and military purposes should be increased, kept at the present level, reduced, or ended altogether?
2. In your opinion, what should be the government's main approach in reducing the federal deficit? If you had to choose, would you rather see the government cut spending for defense or cut non-defense spending?

E. Military Involvement

1. Would you *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* sending U.S. troops to Central America to stop the spread of communism?
2. Please tell me whether you would *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* the use of U.S. troops if the Communists took over in El Salvador, Central America?

E. Nuclear Arms

1. Do you think the United States should freeze the production of nuclear weapons on its own, do so only if the Soviet Union agrees to a nuclear freeze as well, or do you think the United States should not

freeze the production of nuclear weapons at all?

2. Would you *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* building more nuclear weapons to increase the U.S. arsenal?

G. Soviet Relations

1. Would you *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* resuming cultural and educational exchanges with the Soviet Union?

H. International Trade

1. Some people feel we should restrict the sale of foreign products like Japanese cars in the U.S. to protect American jobs. Others feel restrictions are wrong because they lead to higher prices for American products at home.
2. Would you *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* restricting foreign imports to protect American jobs?

Core Values

I. Ethnocentrism

1. Other countries should try to make their governments as much like ours as possible.
2. While the American form of government may not be perfect, it is the best form of government yet devised.
3. Many other countries have governments that are just as good as ours.
4. How do you think the United States compares with other countries in the world? Do you think that it is really no better than many other countries, that it is better than many countries but not necessarily the best, or that it is absolutely the best country in the world?

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J. Morality of War

1. It is certainly acceptable to kill one's enemy when fighting for one's country.
2. A person who loves his fellow man should refuse to fight in any war.

Notes

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1. It is not our intention to engage in Converse bashing. Because of his pioneering work in the field, all participants in the constraint debate are intellectually indebted to him.

2. Using data from a five-wave panel, Feldman (1985) also finds responses to domestic and foreign-policy issues to be almost perfectly stable. In addition, Feldman makes the point that the practice of measuring response stability with correlation coefficients (as practiced by Converse [1964]; and Converse and Markus [1979]) can seriously underestimate the true stability of foreign-policy attitudes because such coefficients are sensitive to the variance of the underlying variables. Because responses to domestic-affairs questions tend to be more polarized and more variable than responses to foreign-affairs items, the over-time correlations for domestic attitudes are much higher, even though they are actually no more stable than foreign-policy responses.

3. As an example, Bardes and Oldendick (1978) factor-analyze an initial pool of several hundred items to generate five factors—*militarism, non-military involvement, world problems, détente, and international organizations*—which, they argue, organize the specific foreign-policy attitudes of most individuals.

4. We do not use this term in the same sense as Carmines and Stimson (1980).

5. See Brady and Sniderman's (1985) recent article on the tendency for people to use their affect toward various groups as simple guides for placing themselves and these groups on various issue dimensions, a method the authors term the *likability heuristic*.

6. The demographic characteristics of the sample come reasonably close to approximating those found in the Lexington population. According to the 1980 census, blacks comprise 13% of the Lexington population; our sample includes the same percentage of blacks. The percentage of women in Lexington is 52%; in our sample the percentage is 50%. More educated and affluent individuals are slightly over-

represented in our sample. The median income in Lexington is \$15,915, while the median-income category in the sample is \$20,000-\$30,000 (the median is located at the lower end of this category, however). Respective population and sample percentages for education are *less than high school*, 29% and 14%; *completed high school*, 28% and 26%; *some college*, 18% and 26%; and *graduated college*, 25% and 34%.

7. We examined two additional postures that proved to be rather poor predictors of specific policy preferences. Both postures—nationalism and human rights—are attempts to tap the trade-off between national interests and moral considerations. The dimension of nationalism is anchored by the desire to promote the United States' interests overseas versus the desire to balance U.S. interests with the legitimate claims of other nations. Human rights was measured by agreement with the statement that the "U.S. should not support governments that abuse the human rights of its citizens, even if that country could help us militarily or economically." Neither posture proved useful as an explanation of individuals' preferences on specific foreign-policy issues.

8. Observers of the U.S. people as early as Tocqueville (1955, 235) have commented on our ethnocentric mentality: "The inhabitants of the United States . . . have an immediately high opinion of themselves and are not far from believing that they form a species apart from the rest of the human race."

9. LISREL combines elements of factor analysis found in psychometrics with structural-equation modeling found in path analysis and econometrics. In the factor-analysis portion of the LISREL procedure, the analyst specifies a measurement model where error-free latent variables are constructed from several imperfectly measured indicators. In the structural-equation portion of the LISREL model, the causal relations between the latent variables are specified. By analyzing the covariances between all the observed variables, LISREL uses maximum-likelihood-estimation techniques to obtain, simultaneously, estimates of the epistemic correlations between the latent variables and their indicators and estimates of the relationships between the latent variables.

10. The model assumes that attitudes at the same level of abstraction are related to one another only by their common dependence on more abstract attitudes. Since this assumption did not appear to be justified in the case of defense spending and nuclear policy (i.e., making cuts in the nuclear arsenal may imply making cuts in defense spending for many respondents), we allowed the disturbances of the equations for these two variables to be correlated by freeing an off-diagonal element in the *psi*-matrix of the LISREL model. To anticipate the estimation of the model below, the standardized correlated error term between these two attitudes is .25, indicating

that there is a significant source of (residual) covariation between defense-spending attitudes and nuclear-policy attitudes that is not accounted for by the three postures. We found little evidence of similar correlated disturbances between other attitudes in the model.

11. Wheaton, et al. (1977) consider a chi-square-to-degrees-of-freedom ratio of about 5.0 or less as "beginning to be reasonable." Two other goodness-of-fit statistics (not reported in Table 1)—the adjusted goodness-of-fit index (.893) and the root-mean-square residual (.074)—also show little evidence of model weakness.

12. The impact of ideological self-placement on militarism is .13, significant at the .05 level. Significant covariances are also found between liberalism-conservatism and ethnocentrism (.20), morality of warfare (.15), and party identification (.22).

13. Though anticomunism and militarism are related, we found little evidence of multicollinearity as a problem in these data, judging from the magnitude of the correlations between estimates of the parameters of the model (provided as part of the technical output available in LISREL 6).

14. A closer look at the foreign-policy attitudes typically used to illustrate the stereotypical view shows a preponderance of nonmilitary types of issues, such as diplomatic relations with China (e.g., Erikson, Luttbeg, and Tedin 1980) and foreign-aid policy (Converse and Markus 1979). Bennett (1974) has also noted that more salient foreign-policy-issue attitudes are more likely to be organized in the public's mind.

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POLITICAL CLEAVAGES AND CHANGING EXPOSURE TO TRADE

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Combining the classical theorem of Stolper and Samuelson with a model of politics derived from Becker leads to the conclusion that exogenous changes in the risks or costs of countries' external trade will stimulate domestic conflict between owners of locally scarce and locally abundant factors. A traditional three-factor model then predicts quite specific coalitions and cleavages among owners of land, labor, and capital, depending only on the given country's level of economic development and its land-labor ratio. A preliminary survey of historical periods of expanding and contracting trade, and of such specific cases as the German "marriage of iron and rye," U.S. and Latin American populism, and Asian socialism, suggests the accuracy of this hypothesis. While the importance of such other factors as cultural divisions and political inheritance cannot be denied, the role of exogenous changes in the risks and costs of trade deserves further investigation.

Why countries have the political cleavages they do and why those cleavages change are among the enduring mysteries of comparative politics. Among the many factors that have been adduced as partial explanations are preexisting cultural and religious divisions, the rapidity and timing of industrialization or of the grant of mass suffrage, the sequence of "crises" of modernization, the electoral system, and—most recently—the product cycle (see, inter alia, Binder et al. 1971; Duverger 1959; Kurth 1979a, 1979b; Lipset and Rokkan 1967; Rokkan 1970, 1981).

Without denying the importance of any of these variables, I want to suggest the relevance of a factor that has, until now, been widely neglected: externally induced changes—in countries with different factor endowments—in exposure to international trade.

To be sure, some studies of individual countries, and even a few comparative inquiries, have argued the significance of changing international trade in particular circumstances: one thinks, in particular, of Abraham 1981, Gerschenkron 1943, Gourevitch 1977 and 1986, Rosenberg 1943, Sunkel and Paz 1973. One author, Cameron (1978), has even suggested a relation, at least in recent decades, between exposure to trade and the rate of growth in state expenditure.

Arguing much more generally, I shall try to show that basic results of the theory of international trade—including, in particular, the well-known Stolper-Samuelson Theorem (Stolper and Samuelson 1941)—imply that increases or decreases in the costs and difficulty of international trade should powerfully affect domestic political cleavages and should do so differently, but predictably,

in countries with different factor endowments. Moreover, I shall suggest that these implications conform surprisingly well with what has been observed about patterns of cleavage and about changes in those patterns in a great variety of countries during four periods of global change in exposure to trade, namely the "long" sixteenth century, the nineteenth century, the Depression of the 1930s, and the years since World War II.

Nonetheless, what I present here remains conjectural and preliminary. The evidence I shall be able to advance is suggestive rather than conclusive. It is principally the clarity of the logical case that seems to me to justify further refinement and testing.

The Stolper-Samuelson Theorem

In 1941 Wolfgang Stolper and Paul Samuelson solved conclusively the old riddle of gains and losses from protection (or, for that matter, from free trade). They showed that in any society protection benefits—and liberalization of trade harms—owners of factors in which that society is poorly endowed, relative to the rest of the world, as well as producers who use the scarce factors intensively.¹ Conversely, protection harms—and liberalization benefits—owners of factors the given society holds abundantly relative to the rest of the world, and producers who use the abundant factors intensively.² Thus, in a society rich in labor but poor in capital, protection would benefit capital and harm labor; and liberalization of trade would benefit labor and harm capital.

So far, the theorem is what it is usually perceived to be: merely a statement, if an important and sweeping one, about the effects of tariff policy. The picture is altered, however, when one realizes that exogenous changes can have exactly the same effects as increases or decreases in protection. A cheapening of transport

costs, for example, is indistinguishable in its impact from an across-the-board decrease in every affected state's tariffs (Mundell 1957, 330); so is any change in the international regime that decreases the risks or the transaction costs of trade. The converse is of course equally true: when a nation's external transport becomes dearer, or its trade less secure, it is affected exactly as if it had imposed a higher tariff.

The point is of more than academic interest because we know, historically, that major changes in the risks and costs of international trade have occurred: notoriously, the railroads and steamships of the nineteenth century brought drastically cheaper transportation (Landes 1969, 153-54, 196, 201-2; Hobsbawm 1979, Chap. 3); so, in our own generation, did supertankers, cheap oil, and containerization (Rosecrance 1986, 142). According to the familiar argument of Kindleberger (1973) and others, international hegemony decreases both the risks and the transaction costs of international trade; and the decline of hegemonic power makes trade more expensive, perhaps—as, according to this interpretation, in the 1930s—prohibitively so. Analyzing a much earlier period, the Belgian historian Henri Pirenne (1939) attributed much of the final decline of the Roman Empire to the growing insecurity of interregional, and especially of Mediterranean, trade after 600 A.D.³

Global changes of these kinds, it follows, should have had global consequences. The "transportation revolutions" of the sixteenth, the nineteenth, and scarcely less of the mid-twentieth century must have benefited, in each affected country, owners and intensive employers of locally abundant factors and must have harmed owners and intensive employers of locally scarce factors. The events of the 1930s should have had exactly the opposite effect. What, however, will have been the political consequences of those shifts

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of wealth and income? To answer that question we require a rudimentary model of the political process and a somewhat more definite one of the economy.

Simple Models of the Polity and the Economy

I shall assume of domestic political processes only two things: (1) that the beneficiaries of a change will try to continue and accelerate it, while the victims of the same change will endeavor to retard or to halt it; and (2) that those who enjoy a sudden increase in (actual or potential)⁴ wealth and income will thereby be enabled to expand their political influence as well (cf. Becker 1983). As regards international trade, (1) implies that the gainers from any exogenous change will seek to continue and to expand free trade, while the losers will seek protection (and, if that fails, imperialism);⁵ (2) implies that those who gain, or are positioned to gain, economically from exogenous changes in international trade will increase their political power as well.

Economically, I propose to adopt with minor refinements the traditional three-factor model—land, labor, and capital—and to assume, for now, that the land-labor ratio informs us fully about any country's endowment of those two factors. No country, in other words, can be rich both in land and in labor: a high land-labor ratio implies abundance of land and scarcity of labor; a low ratio signifies the opposite. (I shall later relax this assumption.) Finally, I shall simply define an *advanced* economy as one in which capital is abundant.

This model of factor endowments inevitably oversimplifies reality and will require amendment. Its present simplicity, however, permits us in theory to place any country's economy into one of four cells (see Figure 1), according to (1)

whether it is advanced or backward and (2) whether its land-labor ratio is high or low. We recognize, in other words, only economies that are (1) capital rich, land rich, and labor poor; (2) capital rich, land poor, and labor rich; (3) capital poor, land rich, and labor poor; or (4) capital poor, land poor, and labor rich.

Political Effects of Increasing Exposure to Trade

I shall now try to demonstrate that the Stolper-Samuelson Theorem, applied to our simple model, implies that increasing exposure to trade must result in *urban-rural conflict* in two kinds of economies and in *class conflict* in the two others.

Consider first the upper right-hand cell of Figure 1: the advanced (therefore capital-rich) economy endowed abundantly in labor but poorly in land. Expanding trade must benefit both capitalists and workers; it harms only landowners and the pastoral and agricultural enterprises that use land intensively. Both capitalists and workers—that is to say, almost the entire urban sector—should favor free trade; agriculture should on the whole be protectionist. Moreover, we expect the capitalists and the workers to try, very likely in concert, to expand their political influence. Depending on pre-existing circumstances, they may seek concretely an extension of the franchise, a reapportionment of seats, a diminution in the powers of an upper house or of a gentry-based political elite, or a violent "bourgeois" revolution.

Urban-rural conflict should also arise in backward, labor-poor economies (the lower left-hand cell of Figure 1) when trade expands, albeit with a complete reversal of fronts. In such "frontier" societies, both capital and labor are scarce: hence both are harmed by expanding trade and will seek protection. Only land is abundant, and therefore only agri-

Figure 1. Four Main Types of Factor Endowments

		Land-Labor Ratio	
		High	Low
Advanced Economy	Abundant:	Capital Land	Abundant: Capital Labor
	Scarce:	Labor	Scarce: Land
Backward Economy	Abundant:	Land	Abundant: Labor
	Scarce:	Capital Labor	Scarce: Capital Land

culture will gain from free trade. Farmers and pastoralists will try to expand their influence in some movement of a "Populist" and antiurban stripe.

Conversely, in backward economies with low land-labor ratios (the lower right-hand cell of Figure 1), land and capital are scarce and labor is abundant. The model therefore predicts *class conflict*: labor will pursue free trade and expanded political power (including, in some circumstances, a workers' revolution); landowners, capitalists, and capital-intensive manufacturers will unite to support protection, imperialism, and a politics of continued exclusion. (Lest the picture of a rising in support of freer markets seem too improbable a priori, I observe at once its general conformity with Popkin's 1979 astute interpretation of the Vietnamese revolution.)

The reverse form of class conflict is expected to arise in the final case, that of an advanced but land-rich economy (the upper left-hand cell of Figure 1) under increasing exposure to trade. Because both capital and land are abundant, capitalists, capital-intensive industries, and agriculture will all benefit from, and will endorse, free trade; labor being

scarce, workers and labor-intensive industries will embrace protection and (if need be) imperialism. The benefited sectors will seek to expand their political power, if not by disfranchisement then by curtailment of workers' economic prerogatives and suppression of their organizations.

These implications of the theory of international trade (summarized in Figure 2) seem clear, but do they in any way describe reality? I shall address that question more fully below, but for now it is worth observing how closely the experience of three major countries—Germany, Britain, and the United States—conforms to this analysis in the period of rapidly expanding trade in the last third of the nineteenth century; and how far it can go to explain otherwise puzzling disparities in those states' patterns of political evolution.

Germany and the United States were both still relatively backward, that is, capital-poor, societies: both, in fact, imported considerable amounts of capital in this period (Feis 1965, 24–25 and Chap. 3). Germany, however, was rich in labor and poor in land; the United States, of course, was in exactly the opposite posi-

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Figure 2. Predicted Effects of Expanding Exposure to Trade

		Land-Labor Ratio	
		High	Low
Advanced Economy	Class cleavage: Land and capital free-trading, assertive Labor defensive, protectionist	Urban-rural cleavage: Capital and labor free-trading, assertive Land defensive, protectionist (Radicalism)	
	Urban-rural cleavage: Land free-trading, assertive Labor and capital defensive, protectionist (U.S. Populism)	Class cleavage: Labor free-trading, assertive Land and capital defensive, protectionist (Socialism)	
Backward Economy			

tion. Again, the demonstration is easy: the United States imported—and Germany exported (not least to the United States)—workers.⁶ The theory, of course, predicts class conflict in Germany, with labor the “revolutionary” and free-trading element and with land and capital united in support of protection and imperialism. Surely this description will not ring false to any student of German socialism or of Germany’s infamous “marriage of iron and rye.”⁷ For the United States, conversely, the theory predicts—quite accurately, I submit—urban-rural conflict, with the agrarians now assuming the “revolutionary” and free-trading role and with capital and labor uniting in a protectionist and imperialist coalition. E. E. Schattschneider (1960) or Walter Dean Burnham (1970) could hardly have described more succinctly the history of populism and of the election of 1896.⁸

Britain, on the other hand, was already an advanced economy in the later nineteenth century, one in which capital was so abundant that it was exported in vast quantities (Feis 1965, Chap. 1). That it was also rich in labor is demonstrated by

its extensive exports of that factor to the United States, Canada, Australia, New Zealand, and Africa.⁹ Britain therefore falls into the upper right-hand quadrant of Figure 1 and is predicted to exhibit a rural-urban cleavage, with fronts opposite to those found in the United States: capitalists and labor unite in support of free trade and in demands for expanded political power, while landowners and agriculture support protection and imperialism.

While this picture surely obscures important nuances, it illuminates a crucial difference between Britain and, for example, Germany in this period: in Britain, capitalists and labor *did* unite effectively in the Liberal party and forced an expanded suffrage and curtailment of (still principally landowning) aristocratic power; in Germany, with liberalism shattered (Sheehan 1978), the suffrage for the powerful state parliaments was actually contracted, and—far from eroding aristocratic power—the bourgeoisie grew more and more *verjunkert* in style and aspirations.

Political Effects of Declining Exposure to Trade

When declining hegemony or rising costs of transportation substantially constrict external trade, the gainers and losers are simply the reverse of those under increasing exposure to trade: owners of locally scarce factors prosper, owners of locally abundant ones suffer. The latter, however, can invoke no such simple remedy as protection or imperialism; aside from tentative "internationalist" efforts to restore orderly markets (Gourvitch 1986, Chap. 4), they must largely accept their fate. Power and policy, we expect, will shift in each case toward the owners and intensive users of scarce factors.

Let us first consider the situation of the highly *developed* (and therefore, by our earlier definition, capital-rich) economies. In an economy of this kind with a high land-labor ratio (the upper left-hand cell of Figure 1), we should expect intense *class* conflict precipitated by a newly aggressive working class. Land and capital are both abundant in such an economy; hence, under declining trade, owners of both factors (and producers who use either factor intensively) lose. Labor being the only scarce resource, workers are well positioned to reap a significant windfall from the protection that dearer or riskier trade affords; and, according to our earlier assumption, like any other benefited class, they will soon try to parlay their greater economic into greater political power. Capitalists and landowners, even if they were previously at odds, will unite to oppose labor's demands.

Quite to the contrary, declining trade in an advanced economy that is labor rich and land poor (the upper right-hand cell of Figure 1) will entail *urban-rural* conflict. Capital and labor, being both abundant, are both harmed by the contraction of external trade. Agriculture, as the

intense exploiter of the only scarce factor, gains significantly and quickly tries to translate its gain into greater political control.

Urban-rural conflict is also predicted for backward, land-rich countries under declining trade; but here agriculture is on the defensive. Labor and capital being both scarce, both benefit from the contraction of trade; land, as the only locally abundant factor, retreats. The urban sectors unite, in a parallel to the "radical" coalition of labor-rich developed countries under expanding trade, to demand an increased voice in the state.

Finally, in backward economies rich in labor rather than land, class conflict resumes, with labor this time on the defensive. Capital and land, as the locally scarce factors, gain from declining trade; labor, locally abundant, loses economically and is soon threatened politically.

Observe again, as a first test of the plausibility of these results—summarized in Figure 3—how they appear to account for some prominent disparities of political response to the last precipitous decline of international trade, the Depression of the 1930s. The U.S. New Deal represented a sharp turn to the left and occasioned a significant increase in organized labor's political power. In Germany, a depression of similar depth (gauged by unemployment rates and declines in industrial production [Landes 1969, 391]) brought to power first Hindenburg's and then Hitler's dictatorship. In both, landowners exercised markedly greater influence than they had under Weimar (Abraham 1981, 85–115 and Chap. 4; Gessner 1977); and indeed a credible case can be made out that the rural sector was the principal early beneficiary of the Nazi regime (see, *inter alia*, Gerschenkron 1943, 154–63; Gies 1968; Holt 1936, 173–74, 194ff.; Schoenbaum 1966, 156–63).¹⁰ Yet this is exactly the broad difference that the model would lead us to anticipate if we accept that by 1930 both countries were

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Figure 3. Predicted Effects of Declining Exposure to Trade

		Land-Labor Ratio	
		High	Low
Advanced Economy		Class cleavage: Labor gains power. Land and capital lose. (U.S. New Deal)	Urban-rural cleavage: Land gains power. Labor and capital lose. (Western European Fascism)
	Backward Economy	Urban-rural cleavage: Labor and capital gain power. Land loses. (South American Populism)	Class cleavage: Land and capital gain power. Labor loses. (Asian & Eastern European Fascism)

economically advanced—although Germany, after reparations and cessions of industrial territory, was surely less abundant in capital than the United States—but the United States remained rich in land, which in Germany was scarce. Only an obtuse observer would claim that such factors as cultural inheritance and recent defeat in war played no role; but surely it is also important to recognize the sectoral impact of declining trade in the two societies.¹¹

As regards the less-developed economies of the time, it may be profitable to contrast the Depression's impact on such South American cases as Argentina and Brazil with its effects in the leading Asian country, Japan. In Argentina and Brazil, it is usually asserted (Cardoso and Faletto 1979, 124–26 and Chap. 5; Skidmore and Smith 1984, 59–60; Sunkel and Paz 1973, 352–54), the Depression gave rise to, or at the least strengthened, "Populist" coalitions that united labor and the urban middle classes in opposition to traditional, landowning elites. In Japan, growing military influence suppressed representative institutions and nascent workers' organizations, ruling in the interest—albeit

probably not under the domination—of landowners and capitalists (Kato 1974; Reischauer 1974, 186–87, 195–99). (Similar suppressions of labor occurred in China and Vietnam [Clubb 1972, 135–40; Popkin 1979, xix, 215].)

In considering these contrasting responses, should we not take into account that Argentina and Brazil were rich in land and poor in labor (recall the extent of immigration, especially into Argentina), while in Japan (and, with local exceptions, in Asia generally) labor was abundant and land was scarce (respectively, the lower left- and right-hand cells of Figure 3)?

A Preliminary Survey of the Evidence

I want now to undertake a more systematic, if still sketchy, examination of the historical evidence that bears on the hypotheses developed here. This effort will serve principally to suggest directions for further research; it can in no way be described as conclusive.

The "Long" Sixteenth Century

It has long been recognized that improvements in navigation and shipbuilding permitted, from about 1450 on, a previously unimagined expansion of trade, which eventuated in the European "discovery" and colonization of the Americas (Cipolla 1965). Among social scientists, Immanuel Wallerstein (1974) has studied this period most intensively; and it is worth emphasizing that the present analysis conforms with essential aspects, and, indeed, permits some clarification, of his.

Within the context of the age, what Wallerstein calls the *core* economies of the new world system—those, essentially, of northwestern Europe—were defined by their abundance in capital and labor, and by their relative scarcity of land. The *periphery* can be described as the exact inverse: rich in land, poor in both capital and—often leading to the adoption of slavery or serfdom—labor. Under expanding trade, the regimes of the core come to be dominated by a "bourgeois" coalition of capital and skilled labor (the Dutch Republic, the Tudors), and of the manufactures that use both intensively; the older, landed elites lose ground. Conversely, in the periphery, land—in the persons of plantation owners and *Gutsherren*—suppresses both capital and labor and, indeed, almost all urban life.

So far the equation seems apt. Can we, however, not go on to define that Wallersteinian chimera, the *semiperiphery* (Wallerstein 1974, 102–7), as comprising economies that fall into the lower right-hand cell of Figure 1, economies poor in capital and land, rich in labor? That would, I suspect, accurately describe most of the southern European economies in this period; and it would correctly predict (see again Figure 2) the intense class conflict (including the German Peasants War [Moore 1967, 463–67]) and the wholly retrograde and protectionist policies adopted by a peculiarly united class of

landowners and capitalists in many of these regions.¹²

The Nineteenth Century

We can again proceed regionally, generalizing on the sketch of Britain, Germany, and the United States developed earlier for this period. For the period just before the great cheapening of transportation—roughly at the middle of the nineteenth century¹³—Britain can stand as the surrogate for the advanced and labor-rich economies of northwest Europe generally, including Belgium, the Netherlands, and northern France (Hobsbawm 1962, Chap. 9; Landes 1969, Chap. 3). For this whole region, as for Britain, the model predicts that expanding trade would engender rural-urban conflict: capitalists and workers, united in support of free trade and greater urban influence, oppose a more traditional and protectionist landed sector. It does not seem to me farfetched to see the powerful liberalism and radicalism of this whole region in the later nineteenth century (Carstairs 1980, 50, 62; Cobban 1965, 21–28, 58–67; Daalder 1966, 196–98; Lorwin 1966, 152–55)—or, for that matter, much of the conflict between secularism and clericalism—in this light.

Almost all of the rest of Europe at the dawn of this period can be compared with Germany: poor in capital and in land, rich in labor.¹⁴ (The land-labor ratio seems as a rule to have declined as one moved from north to south within the economically backward regions of Europe [see figures for 1846 in Bowden, Karpovich, and Usher 1937, 3].) As it does for Germany, the model predicts for these other countries, particularly in southern Europe, class conflict as a consequence of increasing exposure to trade: workers (including agricultural wage laborers) press for more open markets and greater influence; capitalists and landowners unite in support of protection and more

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traditional rule. In its main aspect, this seems to me only a restatement of a central tendency that has long been remarked, namely that class conflict in the nineteenth century came at an earlier phase of industrialization, and more bitterly, to southern and central than to northwestern Europe (e.g., Lipset 1970, 28–30; Macridis 1978, 485–87; cf. Thomson 1962, 375–78); and it seems to me a more credible account of these regions' extremism than Duverger's (1959, 238) famous invocation of an allegedly more mercurial "Latin" temperament.

The United States, finally, represents the land-rich, but labor- and capital-poor "frontier societies" of this period generally: most of both Americas, Australia, New Zealand, even those parts of central and southern Africa that would soon be opened to commercial agriculture. Here, expanding trade benefits and strengthens landowners and farmers against protectionist capitalists and workers (although, as in the United States, the protectionist forces may still prevail); rural-urban conflict ensues, precipitated by demands from the rural sector.

Again, this does not at first glance appear wide of the mark. In many of the Latin American societies, this period cemented landed rule (Skidmore and Smith 1984, 50; Sunkel and Paz 1973, 306–21); in the United States and Canada, it was characterized by conflicts between the industrial East and the agricultural West (Easterbrook and Aitken 1958, 503–4); in almost wholly agricultural Australia, trade precipitated a cleavage between free-trading landowners and increasingly protectionist rural and urban wage labor (Gollan 1955, esp. 162–69; Greenwood 1955, 216–20).

In all of these cases, as I have emphasized before, other factors were surely at work and important aspects are neglected by the present analysis; but it is essential also not to ignore the benefits and costs of expanding trade to the various sectors.

The Depression of the 1930s

Here the fit between theory and reality seems quite strong. Not only the United States but Canada, Australia, and New Zealand were by this time advanced, land-rich economies. Labor, their only scarce factor, gained from the collapse of international trade: workers became more militant, policy shifted to the left. Most Latin American societies remained land-rich but backward; and for them this was quite generally the period of "Populist" coalitions of the two scarce factors, labor and capital. In developed northern Europe, owners and exploiters of the locally scarce factor of land grew more assertive, and generally more powerful, wherever previous developments had not caused them to disappear; capitalists and workers lost ground. Finally, throughout the backward regions of the world economy, where labor was abundant and land was scarce—not only in Asia but in southern and eastern Europe—labor lost to a renascent coalition of the locally scarce factors of land and capital: in Spain, Italy, Rumania, Hungary, and Poland, to name only the most prominent cases (Carsten 1967, Chaps. 2, 5 and pp. 194–204).

After World War II

Under U.S. hegemony, and with new economies in transportation and communication, the West since World War II has experienced one of history's more dramatic expansions of international trade (Organization for Economic Cooperation and Development [OECD] 1982, 62–63). Again, the theory would lead us to expect different regional consequences.

In the developed, labor-rich and land-poor economies—including now not only most of Europe but Japan—the model would predict an "end of ideology," at least as regards issues of class: labor and capital, both beneficiaries of expanding

trade, unite to advance it and to oppose any remaining pretensions to rule by the landowning groups.¹⁵ Conversely, in the land-rich and still underdeveloped economies of Latin America, expanding trade displaces the Depression-era "Populist" coalitions of labor and capital and brings renewed influence to the landed sectors. The areas of Asia and of southern Europe that are economically backward and abundant only in labor experience labor militancy and, in not a few cases, revolutionary workers' movements. Finally, and perhaps more as a statement about the future, the few economies rich in both capital and land—principally those of North America, Australia, and New Zealand—should, as they become seriously exposed to international trade, experience class conflict and a considerable suppression of labor. Capital and agriculture will for the most part unite in support of the free trade that benefits them; labor, as the locally scarce factor, will favor protection and imperialism.

Further Implications

To the extent that the model has gained any credibility from the foregoing brief survey, it may be useful to observe some of its other implications for disciplinary riddles and conjectures. Take first Gerschenkron's (1962) observation, and Hirschman's (1968) subsequent challenge and amendment of it, that "latecomers" to economic development tend to assign a stronger rôle to the state. From the present perspective, what should matter more, at least among labor-rich economies, is whether development *precedes* or *follows* significant exposure to trade. In an economy that has accumulated abundant capital before it is opened to trade, capital and labor will operate in relative harmony, and little state intervention will be required. Where trade precedes development, assertive labor faces—as it did in Imperial Germany—the united opposition

of capitalists and landowners. To the extent that labor wins this struggle, it will require a strong state to administer the economy; to the extent that capital and land prevail, a state powerful enough to suppress labor is needed. Either route leads to a stronger state.

Even this generalization, however, applies only to economies where labor is abundant, and land scarce. Hence Hirschman's observation that "latecomers" in Latin America do not behave as Gerschenkron predicts should not surprise us. Where land is abundant, and labor scarce—as has generally been true of the Americas—"late" economic modernization (i.e., one that follows significant exposure to trade) radicalizes owners of land rather than owners of labor. In such "frontier" economies, labor and capital again find themselves in the same political camp, this time in support of protection. In the absence of class conflict, no powerful state is required.

This last point, of course, sheds some light on Sombart's old question, Why is there no socialism in the United States? If this model is right, the question is appropriately broadened to, Why is there no socialism in land-rich economies? Simply put, socialism develops most readily where labor is favored by rising exposure to trade and capital is not; labor is then progressive and capital is reactionary. But labor is never favored by rising trade where it is scarce. Powerful socialist movements, the present model suggests, are confined to backward and labor-rich economies under conditions of expanding trade (the less-developed European societies in the later nineteenth century; Asia after World War II).

A third riddle this approach may help resolve is that of the coalitional basis and aims of the North in the U.S. Civil War.¹⁶ As Barrington Moore, Jr. posed the question in a memorable chapter of *Social Origins of Dictatorship and Democracy* (1967, Chap. 3), What was the connection

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between *protection* and Free Soil in the platform of the Republican party or of the North more generally, and Why did so broad a coalition support both aims?

If, as seems apparent, labor was scarce in the United States, then the nineteenth century's increasing exposure to trade should have depressed, or at least retarded the advance of, wages. By definition, slaves already received a lower wage than they would voluntarily accept (Else, why coerce them?); and increased trade could reasonably be seen as intensifying, or at least as retarding the demise of, slavery. Conversely, protection in a labor-scarce economy might so raise the general wage level (while, paradoxically, also increasing returns to scarce capital) as to make manumission feasible. Hence to link protection and abolition might seem a wholly sensible strategy. Moreover, because protection in that period would benefit workers and capitalists generally, it could attract the support of a very wide coalition. At least some of the mystery seems dissolved.

Relaxing the Reliance on Land-Labor Ratios

For the sake of logical completeness, and to fill a nagging empirical gap, let us now relax the assumption that the land-labor ratio informs us completely about the relative abundance of these two factors. We admit, in other words, that a country may be rich or poor in *both* land and labor. Four new cases arise in theory if (as I suspect) rarely in practice (see Figure 4): economies may be, as before, advanced or backward (i.e., capital rich or capital poor); but they may now be rich in both land and labor or poorly endowed in both factors.

Two cases—that of the advanced economy rich in both factors and of the backward one poor in both—are theoretically improbable¹⁷ and politically uninterest-

ing: if all factors were abundant relative to the rest of the world, the society would unanimously embrace free trade; if all were scarce, it would agree on protection. Let us consider, then, the remaining two possibilities.

In an advanced economy where both land and labor are scarce, expanding trade will benefit only capital. Agriculture and labor—*green* and *red*—will unite in support of protection and, if need be, imperialism; only capitalists will embrace free trade. When trade contracts in such an economy, the scarce factors of land and labor gain, and capital loses, influence; farmers and peasants are likely to seek expanded mass participation in politics and a radical curtailment of capitalist power.

In a backward economy with abundant land and labor (a possibility considered explicitly by Myint [1958, 323]), change in exposure to trade again mobilizes a coalition of red and green, but with diametrically opposed positions. Expanding trade now *benefits* farmers and workers but harms capitalists; and the labor-landowner coalition pursues a wider franchise, free trade, and disempowerment of capital. Contracting trade, however, benefits only the owners of capital and injures both workers and farmers; again intense conflict between capital and the other two factors is predicted, ending in either a capitalist dictatorship or an anti-capitalist revolution.

It is tempting, if speculative in the extreme, to see in the red-green coalitions of Scandinavia in the 1930s (Gourevitch 1986, 131–35; Hancock 1972, 30–31; Rokkan 1966, 84) the natural response to trade contraction of (by then) capital-rich but land- and labor-poor economies; and, conversely, to view modern Russian history, at least until well after World War II, as that of a backward but land- *and* labor-rich economy,¹⁸ which, in a time of expanding trade, indeed forged an anti-capitalist coalition of peasants and work-

Figure 4. Predicted Effects on Economies That Are Rich or Poor
in both Land and Labor

	Land and Labor both Abundant	Land and Labor both Scarce
Advanced Economy		Expanding trade: Capital assertive, free-trading Land and labor protectionist, defensive Declining trade: Land and labor gain power. Capital loses.
Backward Economy	Expanding trade: Land and labor free-trading, assertive Capital defensive, protectionist Declining trade: Capital gains power. Land and labor lose.	

ers and, when trade contracted, experienced (as Stalin's enemies alleged at the time) a dictatorship of state capital over both workers and farmers.

Certainly so long as we cling to the view that land can only be abundant where labor is not, and vice-versa, we can offer no trade-based account of red-green coalitions; indeed, changing exposure to trade must drive the two factors apart, for it always helps the one and hurts the other. On the one hand, this reflects reality—coalitions of labor and agriculture have been rare, and have failed even where much seemed to speak for them (e.g., in U.S. populism and on the German left); on the other, it leaves the few actual red-green coalitions, particularly those that arose in circumstances of changing exposure to trade, as standing refutations of the model.

Possible Objections

At least three objections can plausibly be raised to the whole line of analysis that

I have advanced here:

First and most fundamentally, it may be argued that the effects sketched out here will not obtain in countries that depend only slightly on trade. A Belgium, where external trade (taken as the sum of exports and imports) roughly equals GDP, can indeed be affected profoundly by changes in the risks or costs of international commerce; but a state like the United States in the 1960s, where trade amounted to scarcely a tenth of GDP, will have remained largely immune (OECD 1982, 62–63).

This view, while superficially plausible, is incorrect. The Stolper-Samuelson result obtains at any margin; and, in fact, holders of scarce factors have been quite as devastated by expanding trade in almost autarkic economies—one need think only of the weavers of capital-poor India or Silesia, exposed in the nineteenth century to the competition of Lancashire mills—as in ones previously more dependent on trade. (Cf. Thomson 1962, 163–64, on the vast dislocations that even slight exposure

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to trade occasioned in previously isolated areas of nineteenth-century Europe.)

Second, one can ask why the cleavages indicated here should persist. In a world of perfectly mobile factors and rational behavior, people would quickly disinvest from losing factors and enterprises (e.g., farming in Britain after 1880) and move to sectors whose auspices were more favorable. Markets should swiftly clear, and a new, if different, political equilibrium should be achieved.

To this, two answers may be given. First, in some cases trade expands or contracts so rapidly as to frustrate rational expectations. Especially in countries that experience a steady series of such exogenous shocks—Europe, for example, since 1840—divisions based on factor endowments (which ordinarily change only gradually)¹⁹ will be repeatedly revived. Second, often enough some factors' privileged access to political influence makes the extraction of rents and subsidies seem cheaper than adaptation: Prussian *Junker*, familiarly, sought (and, rather easily, won) protection rather than adjustment. In such circumstances, adaptation may be long delayed, sometimes with ultimately disastrous consequences.

Finally, it may be objected that I have said nothing about the outcome of these conflicts. I have not done so for the simple reason that I cannot: history makes it all too plain—as in the cases of nineteenth-century Germany and the United States—that the economic losers from trade may win politically over more than the short run. What I have advanced here is a speculation about *cleavages*, not about outcomes. I have asserted only that those who gain from fluctuations in trade will be strengthened and emboldened politically; nothing guarantees that they will win. Victory or defeat depends, so far as I can see, on precisely those institutional and cultural factors that this perspective so resolutely ignores.

Conclusion

I have not claimed that changes in countries' exposure to trade explain all, or even most, of their varying patterns of political cleavage. It would be foolish to ignore the importance of ancient cultural and religious loyalties, of wars and migrations, or of such historical memories as the French Revolution and the *Kulturkampf*. Neither have I offered anything like a convincing empirical demonstration of the modest hypotheses I have advanced; at most, the empirical regularities that I have noted or have taken over from such authorities as Gerschenkron and Lipset can serve to suggest the plausibility of the model and the value of further refinement and testing of it.

I have presented a theoretical puzzle, a kind of social-scientific "thought-experiment" in Hempel's (1965) original sense: a teasing out of unexpected and sometimes counterintuitive implications of theories already widely accepted (Chap. 7). For the Stolper-Samuelson Theorem is generally, indeed almost universally, embraced; yet, coupled with a stark and unexceptionable model of the political realm, it plainly implies that changes in exposure to trade must profoundly affect nations' internal political cleavages. Do they do so? If they do not, what is wrong—either with our theories of international trade or with our understanding of politics?

Notes

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1. In fact, the effect flows backward from products and is an extension of the Heckscher-Ohlin theorem: under free trade, countries export products whose manufacture uses locally abundant, and import products whose manufacture uses locally scarce, factors intensively (cf. Leamer 1984, esp. 8-10).

2. Admittedly, this result depends on simplifying assumptions that are never achieved in the real world, among them perfect mobility of factors within national boundaries, a world of only two factors and two goods, and incomplete specialization. Still, as an approximation to reality, it remains highly serviceable (cf. Ethier 1984, esp. 163-64, 181).

3. Later historians have, of course, largely rejected Pirenne's attribution of this insecurity to the rise of Islam and its alleged blockade of Mediterranean commerce (Havighurst 1958). It can hardly be doubted, however, that the decline of Roman power by itself rendered interregional trade far less secure.

4. As transportation costs fall, states may offset the effect by adopting protection. Owners of abundant factors then still have substantial potential gains from trade, which they may mortgage to pressure policy toward lower levels of protection.

5. Countries that lack essential resources can only beggar themselves by protection. Ultimately, those in such a society who seek protection from trade must advocate conquest of the missing resources—as indeed occurred in Japan and Germany in the 1930s.

6. Between 1871 and 1890, just under two million Germans emigrated to points outside Europe; in the same years, some seven million immigrants entered the United States (Mitchell 1978, Tbl. A-5; Williams, Current, and Freidel 1969, 158).

7. The Stolper-Samuelson analysis also helps to clear up what had seemed even to the perspicacious Gerschenkron (1943, 26-27) an insoluble riddle: why the *smallholding* German peasants had quickly become as protectionist as the *Junker*. Not only landowners, we now see, but all enterprises that used land intensively, will have been harmed by free trade. On the other hand—and later the distinction will become crucial—agricultural *wage labor* should have been free trading.

8. That the farmers of the Great Plains were hardly prospering in these years is no refutation of the analysis advanced here. Their potential gains were great (see n. 4), and their suffering could plausibly be attributed not to expanded trade but to the obstacles or exploitation laid upon that trade by

other sectors. As in Marxist analysis, the older relations of production and of politics could be seen as "fetters."

9. Emigrants from the United Kingdom to areas outside Europe totalled 5.1 million between 1871 and 1890 (see Mitchell 1978, Tbl. A-5).

10. Certainly they had been among its earliest and strongest supporters: virtually every study of late Weimar voting patterns (e.g., Brown 1982; Childers 1983; Lipset 1960, 138-48) has found a large rural-urban difference (controlling for such other variables as religion and class) in support for National Socialism.

11. Historians have, of course, often recognized declining trade's sectoral effects on Weimar's final convulsions; the controversial essay of Abraham (1981) is only the best-known example. They may, however, have exaggerated agriculture's woes (see Holt 1936; Rogowski 1982).

12. Sabean (1969, Chap. 3) and Bickle (1981, 76-78) link the Peasants War convincingly to the density and rapid growth of population in the affected areas, i.e., to an increasing abundance of labor.

13. "The world's trade between 1800 and 1840 had not quite doubled. Between 1850 and 1870 it increased by 260 percent" (Hobsbawm 1979, 33).

14. Finer distinctions would require a more precise definition of factor abundance and scarcity. The one commonly accepted for the case of more than two factors stems from Vanek's extension of the Heckscher-Ohlin Theorem (Leamer 1984, 15); it defines a country as abundant (or scarce) in a factor to the extent that its share of world endowment in that factor exceeds (or falls short of) its share of world consumption of all goods and services. Leamer's (1984, App. D) Factor Abundance Profiles are a tentative effort to apply this definition to present-day economies. To do so with any precision for earlier periods hardly seems possible.

15. Zysman seems to me to have captured the essence of European and Japanese agricultural policy in this period: 'The peasantry could be held in place [by subsidies and price supports] even as its economic and social positions were destroyed' (1983, 24).

16. I am grateful to David D'Lugo and Pradeep Chhibber for having raised this issue in seminar discussion.

17. More precisely, they are inconsistent with balanced trade (cf. Leamer 1984, 8-10; Leamer 1987, 14-15).

18. There can be no doubt of Russia's abundance of land: as late as 1960, its population per square kilometer of agricultural land (35.7) was comparable to that of the United States (40.9) or Canada (28.4) and strikingly lower than those of even the more thinly populated nations of western and central Europe (e.g., France, 133; Poland, 146) (World Bank 1983). On the other hand, Myint's (1958, 323-31)

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insightful analysis suggests how even sparsely populated regions can have great reserves of under-employed labor under conditions of primitive markets and social structures; and he takes episodes of extremely rapid economic growth, such as the USSR exhibited in the 1930s, as putative evidence of such "surplus" labor (Myint 1958, 323-24, 327).

19. The chief exception to this rule arises from extensions of trade to wholly new areas with quite different factor endowments. In 1860, for example, Prussia was abundant in land relative to its trading partners; as soon as the North American plains and the Argentine *pampas* were opened, it ceased to be so. I am grateful to my colleague Arthur Stein for having pointed this out.

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PUBLIC OPINION AND THE U.S. SUPREME COURT: FDR'S COURT-PACKING PLAN

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I show the intimate connection between the actions of the justices and support for the Supreme Court during one of the most critical periods of U.S. political history, the four months of 1937 during which Franklin D. Roosevelt sought legislation to "pack" the high bench with friendly personnel. Over the period from 3 February through 10 June 1937, the Gallup Poll queried national samples on 18 separate occasions about FDR's plan. These observations constitute the core of my analyses. I demonstrate the crucial influence of judicial behavior and the mass media in shaping public opinion toward the Supreme Court. This research illuminates the dynamics of public support for the justices, contributes to a clearer understanding of an important historical episode, shows the considerable impact of the mass media on public attitudes toward the Court, and adds more evidence on the role of political events in the making of public opinion.

Political institutions require some minimal level of public support to survive and to thrive. It is, of course, exceedingly difficult to document the consequences of popular support or the lack thereof; but most political scientists seem to agree that it matters. For the Supreme Court, which has no budgetary power or military authority, standing with the public takes on an especially large dimension. Students of the Court and public opinion have emphasized the lack of information, instability of attitudes, and shallowness of support for specific decisions among the U.S. citizenry (Adamany and Grossman 1983; Casey 1974, 1976; Daniels 1973; Dennis 1975; Dolbeare 1967; Dolbeare and Hammond 1968; Kessel 1966; Murphy and Tanenhaus 1968a, 1968b; Tanenhaus and Murphy 1981). That characterization of the mass public—as ill informed about and indifferent toward the Court—undoubtedly captures the essence of opinion

at the level of individuals. But in a recent article, I argued that "the responsiveness of public support for the Court in the aggregate to political events and shifts in the behavior of the justices stands in stark contrast to the conventional image of the American citizenry as out of touch with and unmoved by the Supreme Court" (Caldeira 1986, 1210).

Here I intend to show the intimate connection between the actions of the justices and support for the Supreme Court during one of the critical periods of U.S. political history, the four months of 1937 during which Franklin D. Roosevelt sought legislation to permit him to "pack" the high bench with friendly personnel. The argument is disarmingly simple: the justices themselves helped to shape events and build up institutional support with a series of well-timed decisions. More specifically, I demonstrate that in two crucial actions, the Court let the air out of FDR's sails; the president, despite a

magical touch, had little success in persuading the public of his program's urgency; and the exposure of the proposal in the mass media expanded the "scope of conflict" and helped the justices appreciably. I do not, of course, wish to suggest that the Supreme Court routed President Roosevelt in the battle over constitutional politics in the 1930s. For, in the words of one scholar, the Court won the battle, and FDR won the war (Burns 1956). Nevertheless, the Supreme Court did influence public opinion in a significant fashion in the short run. This research is of consequence because it illustrates the dynamics of public support for the justices, contributes to a clearer understanding of an important historical episode, shows the considerable impact of the mass media on public attitudes toward the Court, and adds more evidence on the role of political events in the making of public opinion (see Dalton and Duval 1986; Kernell 1976, 1986; Lehne and Reynolds 1978; Ostrom and Simon 1985). The results suggest, again, that public support in the aggregate for the Supreme Court responds in a rational and explicable manner to the actions and decisions of political elites (see Caldeira 1986).

Quite unlike most issues, FDR's proposal forced the public to choose between the widely approved policies of an extremely popular president and the institutional integrity of a controversial Supreme Court. Seldom in U.S. constitutional history have the president and the Court gone head-to-head in battle so directly for so long (see Murphy 1962; Nagel 1965; Schmidhauser and Berg 1972; Scigliano 1971). For five months, the mass media, Congress, and the president focused on little else. Indeed the political controversy was sufficiently warm to motivate George Gallup to mount an extended series of polls of the public on the question of the proposed change. Over the period from 3 February through 10 June 1937, the Gallup Poll queried

national samples on 18 separate occasions about "Court packing" and related issues. This set of observations constitutes the core of the statistical analyses in the present paper. These data have with certain exceptions lain fallow for nearly 50 years (cf. Cantwell 1946; Handberg 1984). It is, altogether, an exceedingly rich lode of data on perceptions of the Supreme Court and on the dynamics of public support for political institutions.

The Context of "Court Packing"

Scholars have told the tale, on several occasions and in considerable detail, of the fight in 1937 over President Roosevelt's proposal to pack the Court; here I review the basic chain of events before moving on to the description and analysis of the data (see Adamany 1973; Alsop and Catledge 1938; Baker 1967; Irons 1982; Leuchtenberg 1966, 1969, 1985; Mason 1956).¹ From the outset of the Roosevelt administration, the president and the Court were at cross-purposes. He did not make the decision to attack the Court overnight. The justices, or a majority of them, wanted to constrain national power; Roosevelt, with a "New Deal," set out to expand the horizons of government. After a period of peace between the two branches, the Court in May of 1935 struck down a large segment of the New Deal—the Railway Pension Act, the National Industrial Recovery Act, and the Frazier-Lemke Act—and reversed FDR's dismissal of a member of the Federal Trade Commission. Then, in January 1936, the Court invalidated part of the Agricultural Adjustment Act and in May found the Guffey Coal Act and the Municipal Bankruptcy Act contrary to the Constitution. To add insult to injury, the Court in *Morehead v. ex rel. Tipaldo* struck down the State of New York's attempt to set a minimum wage for women. These and other cases signaled a

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stubborn resistance on the part of the Court to the heart of the New Deal and, for that matter, the emerging modern state. In the view of four, and probably five, of the justices, neither the national nor the state governments had the constitutional capacity to take the actions FDR and others saw as necessary for economic recovery.

In May 1935, Roosevelt criticized the Supreme Court for "relegat[ing] us to the horse-and-buggy definition of inter-state commerce." Still, he took no action against the justices. Others, allies of the New Deal in Congress, introduced a variety of bills to strike at the Court or to amend the Constitution to clarify national legislative powers. Proposed methods of curbing the Supreme Court included the creation of new seats, mandatory retirement at a certain age, abolition of judicial review, the agreement of an extraordinary majority on the Court to invalidate a federal law, and a requirement of advisory opinions on the constitutionality of proposed statutes. During the latter part of 1935 and first half of 1936, the antagonism between the Court and the New Deal was a hot topic in the halls of Congress and in the press. The issue was weighty enough for the Gallup Poll to have asked the public several times in this period about restrictions on the Supreme Court's power to invalidate legislation (see Cantril and Strunk 1951; Murphy 1962, 61). Nevertheless, FDR bided his time, discussing the matter with advisors and maintaining a studied silence on the issue during the presidential campaign of 1936.

Finally, after the election, FDR and Attorney General Homer Cummings fastened upon a proposal from Professor Edward S. Corwin of Princeton; the president would request legislation to permit him to nominate one additional justice for every sitting member who had served 10 or more years and had declined to retire at the age of 70. This proposal, if enacted,

would have given Roosevelt six nominations to the Court. Emerging from a veil of secrecy, the president announced the plan on 5 February to members of the cabinet, leaders of Congress, and a press conference. He added several bonuses to the bill as a smokescreen for the real purpose: additional lower federal judges, reassignment of judges by the Chief Justice to the busiest courts, and a proctor to supervise the flow of litigation. All of this FDR portrayed as a plan to make the justices more efficient and to bring them abreast of their work.

In the wake of the announcement, debate about the proposed restructuring of the Supreme Court poured forth and dominated the public agenda. Story after story—discussions of the plan from every conceivable angle—filled the nation's newspapers. Meanwhile, leaders in Congress, FDR's allies, had little enthusiasm for the program. Over the next few months, as we shall see, a number of political events shaped the ebb and flow of public support for curbing the Court.

Political Events and Public Opinion

Many ingredients influence the outcomes of political campaigns; President Roosevelt's failed attempt to "pack" the Court was the result of a complicated train of events and nonevents. And the first half of 1937 was filled with events of far-reaching historical importance (for a list, see Cantwell 1946, 925). Yet, after a careful review of the period, I have identified four crucial events as particularly instrumental in the battle between the Supreme Court and the president. These events appear over and over again in narratives on the critical junctures in the debate on the Court. Furthermore, of the train of events from February through June, this set of four was the most visible and logically related to shifts in public

opinion. First, on 4 March in a speech at the Democratic Victory Dinner and again in a fireside chat on 8 March, President Roosevelt took the case for Court packing to the people. At the dinner with the faithful, he called for partisan loyalty on the issue. Over the air waves, he called attention to the Supreme Court's alleged usurpation of political power. These speeches and the attendant follow-up by lieutenants, I believe, increased public support for the plan; FDR certainly intended them to do so.

Second, on 29 March, the Court announced the decision in *West Coast Hotel v. Parrish*, upholding the State of Washington's minimum-wage law and overruling the odious precedent of *Adkins v. Children's Hospital*. This decision marked an apparent retreat from the nullification of state legislative power in *Morehead v. Tipaldo*, an opinion delivered less than a year before. The hospitable attitude the Court exhibited in *Parrish* provided a stark contrast to FDR's claims that the justices opposed all forms of social progress. Opponents of the plan, naturally, cited the decision in *Parrish* as proof in part that the nation did not need to restructure the Supreme Court.

Third, on 4 April, the Court validated a major element of the New Deal, the Wagner Act, in four separate cases; *NLRB v. Jones and Laughlin Steel Company* has come down in history as the landmark. Because of the Wagner Act's sweeping nature and the justices' previously demonstrated hostility to broad exertions of national power, the Supreme Court's decision came as somewhat of a surprise. Chief Justice Hughes articulated a vision of national power ample enough to accommodate much of the New Deal. With this display of liberality toward the regulation of interstate commerce, the Supreme Court by deed called the urgency of FDR's scheme into serious question.

Fourth, on 18 May, Justice Van Devan-

ter announced his retirement from the Court. As one of the "Four Horsemen," Van Devanter had contributed a consistent vote in a 5-4 coalition against President Roosevelt's measures and variants of the "little New Deals" from the states. Moreover, although not a productive writer, Van Devanter functioned as the intellectual leader of the Supreme Court's conservatives. With Justice Van Devanter out of the picture and a Rooseveltian in his place, the administration could hope for a 5-4 majority in most of the crucial cases. FDR could not, of course, count on ironclad support from the justices; but the future looked much brighter after 18 May.

In sum, then, I anticipate that President Roosevelt's speeches, the Court's decisions in *West Coast Hotel v. Parrish* and *NLRB v. Jones and Laughlin Steel*, and Justice Van Devanter's departure influenced public support for the proposed restructuring. Later, I shall set out more specific expectations.

Effects of Mass Communications

Even a casual perusal of the literature quickly establishes that scholars have not arrived at a consensus on the impact of mass media in politics. There are, in fact, at least two main questions at issue: Do the media influence what people think about? and Do the media persuade the public in one direction or another? Until quite recently, most scholars agreed that "because individuals were firmly anchored in an original set of beliefs and social network of friends, family, and fellow workers, . . . messages from the media worked primarily to reinforce the dispositions first held" (MacKuen 1931, 21; see, for example, Klapper 1960). The press, Cohen said in a conventional assertion, "may not be successful much of the time in telling people what to think, but it is stunningly successful in telling readers what to think about. . . . It [puts] a claim

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on their attention, powerfully determining what they will be thinking about, and talking about, until the next wave laps their shore" (Cohen 1963, 13). Lamentably, the earliest researchers targeted the capacity of the mass media to influence *evaluations* of political candidates and issues to the exclusion of the *agenda-setting* function of the various modes of communication.

It is now clear, after a spate of scholarly activity in the 1970s, that the press and broadcasting play large roles in bringing issues and personalities to the forefront of public consciousness and keeping them there; the mass media, in sum, help to set the national agenda (e.g., Funkhouser 1973). At the crudest level, some analysts model public opinion as a simple, "mirror image" of an issue's or personality's salience in the mass media: the greater the coverage, the greater the awareness among the citizenry. Others have taken a more sophisticated approach. Thus, for example, MacKuen (1981) has demonstrated the significant impact of media attention on the concerns of the public, but he also finds events and objective conditions to be crucial elements of the equation (see also Erbring, Goldenberg, and Miller 1980; Iyengar, Peters, and Kinder 1982).

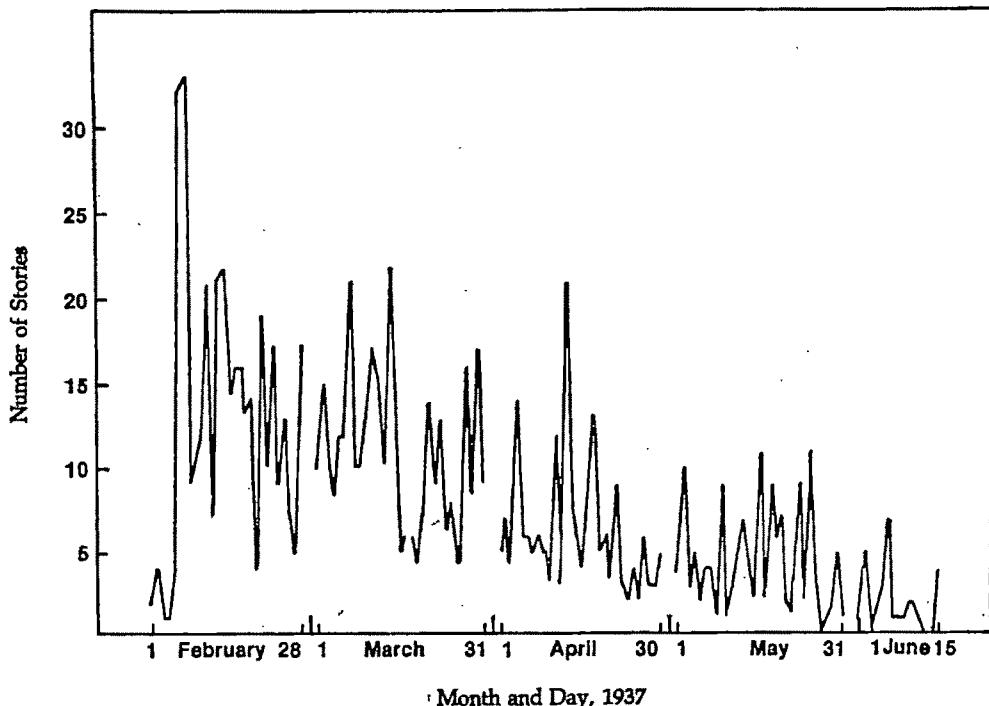
Quite apart from agenda setting, in the last few years, several students of persuasion have mounted a concerted challenge to the conventional wisdom that the mass media exercise "minimal effects" on political decisions and outcomes. In a study of newspapers during the congressional election of 1974, Miller and his associates discovered that readers of highly critical papers displayed more distrust in government than did others: "media style in reporting . . . events . . . had an effect on the degree of popular disaffection found in America" (Miller, Goldberg, and Erbring 1979, 80). Similarly, Coombs (1981) reports that in 1972 and 1974, newspaper endorsements had a

measurable impact on a candidate's chances of victory at the polls.

Here I anticipate a significant relationship between the media's coverage of the Supreme Court and support for Court packing (on the Court and the press, see Grey 1972; Newland 1964). There is, unfortunately, no readily available, comprehensive indicator of the extent of the mass media's attentiveness to the Court and President Roosevelt's plan. Of the major newspapers, only the *New York Times* has an index that reaches back into the 1930s. So far as I can tell, the radio networks have not published or archived indexes of their broadcasts (but see Murrow 1937). For my indicator of the media's coverage, I have adopted the number of stories in the *New York Times*. That measure, taken weekly in the first six months of 1937, correlates strongly with the number of stories on the Court collected in *The Reader's Guide to Periodical Literature*. To be sure, the number of stories in the *New York Times* at best taps indirectly the amount and kind of information the public possesses on a particular subject; after all, its readers constitute a highly educated, elite stratum of U.S. society. Nevertheless, as the correlation between coverage in the *New York Times* and *The Reader's Guide* attests, the coverage in this elite newspaper tracks closely with other sources of information. Furthermore, reports in the *New York Times* undoubtedly diffuse throughout the nation in a "two-step flow" of information. (For a defense of a similar measure, see MacKuen 1981, 63-64.)

Figure 1 displays the number of stories on the Supreme Court in the *New York Times* on a daily basis from 1 February through 15 June 1937. Coverage of the Supreme Court varied a good deal in intensity, from a high in early February to a low in May, and declined across the period. Discussion of the Court did occur daily until late in May, so the public was subjected to a continuous barrage of

Figure 1. Coverage of Supreme Court, 1-15 February



information. The peaks in coverage, naturally, correspond to key events in the campaign over Court packing. Thus, for example, the announcement of the decision in *NLRB v. Jones and Laughlin Steel* set off a raft of stories on it and related matters. What is most remarkable here is the length and intensity of attention to the Supreme Court. Surely the Court has not since then surfaced so long and so prominently on the public agenda, even during the salad days of the Warren Court.

Specifically, I expect a negative relationship between the intensity of coverage and support for Court packing. Public attention to the Court operated, I believe, in classic fashion; Schattschneider wrote of the "socialization of conflict" (1960; see also Truman 1951). That is, as the mass media focused on the issue, more and

more groups, institutions, and individuals entered into the fray. After a reading of the news for that period, one gathers the cumulative impression, especially in the latter half, of a cacaphony of voices and actors for and against the plan. Enemies of FDR and of Court packing did all they could to mobilize potential opponents. And, of course, as more interest attended the issue, more questions inevitably arose. In his study of presidential popularity, Mueller (1973) has characterized a similar phenomenon over the course of a chief executive's term as a "coalition of minorities"; the more exposure a president receives, the more critics he acquires. In any event, the number of stories in the *New York Times* provides a neat index of the expansion of conflict. Extended focus on a subject should, in addition, foster

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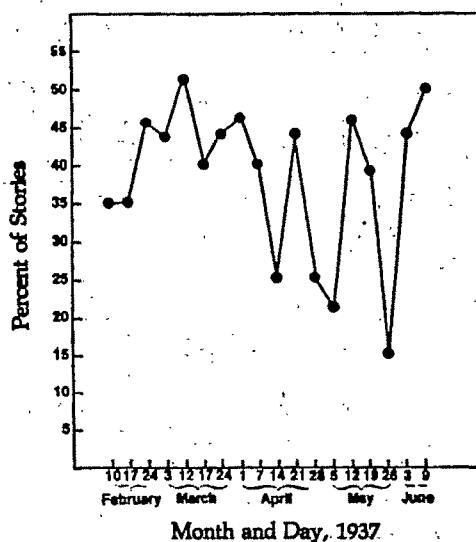
more coherence among the citizenry. Accordingly, media coverage did indeed help to crystallize public opinion toward the president's plan; as the number of stories increased, the percentage of the public with no opinion decreased ($b = -0.06$; $r^2 = .15$).

Figure 2 portrays the directional balance of stories on the Supreme Court in the *New York Times* from February through mid-June 1937. To create this variable, I have divided stories into categories of *positive*, *negative*, and *neutral* toward the Supreme Court. I then excluded the neutral reports. The percentages in Figure 2 represent the portion of the remaining stories I could classify as supportive of Court packing.² Throughout the period under consideration, the coverage of the issue ran against President Roosevelt. In only two weeks did favorable views of Roosevelt's position on the Court hit 50% or higher—immediately after his fireside chat and in the final part of the series as interest in the issue faded. Actually, these last two observations are probably somewhat misleading because the number of stories by that time had decreased to a handful. On average, about 39% of the categorized stories placed FDR's proposal in a favorable light. If these data are at all representative of the mass media's treatment of Court packing—and I believe they are—then FDR clearly received little aid and comfort from the press.³ Naturally, I expect that as the content of coverage became more positive toward the proposal, the segment in the nation in favor of Court packing grew apace. There is, incidentally, no relationship between the level of media coverage and the direction of the stories.

Methods, Models, and Data

To test for the effects of the four events, I have chosen a multiple interrupted time-series design (see McDowell et al. 1980).

Figure 2. Direction of Stories on Court Packing



In its most elementary form, interrupted time-series analysis "attempts to answer a simple question: Does the occurrence of a particular event change a variable's behavior over time?" (Lewis-Beck 1986, 209). Since I specify other independent variables, the model differs from the typical one. Under the best circumstances, I would include for each event a term to pick up a shift in the slope and one to detect a change in the intercept.⁴ Two considerations militate against that strategy here. First of all, the number of observations involved ($N = 18$) is small, and the period of time short (18 weeks). The inclusion of nine parameters for the four events (two for each plus one for the trend), as normal practice indicates, brings on unacceptable levels of multicollinearity and exhausts degrees of freedom. Signs of coefficients go astray. Second, even if full specification were a technical option, I would probably argue for a more limited and parsimonious approach. Because of the brevity of the series in question, a shift in the slope for

an event seems implausible. Instead, we need to monitor sudden, step-level changes in public opinion as a result of the four political events—in other words, to watch for shifts in the intercept.

These ruminations imply the following equation to model the hypothesized consequences of political occurrences:

$$S_t = b_0 + b_1 FDR_{1t} - b_2 PAR_{2t} \\ - b_3 NLRB_{3t} - b_4 VDV_{4t} \\ - b_5 TREND_{5t} + e_t$$

where S_t = 18 time-series observations on the dependent variable, percentage in the sample in favor of Court packing; FDR_{1t} = a dichotomous dummy variable scored 0 for observations before 10 February and 1 after the fireside chat; PAR_{2t} = a dichotomous variable scored 0 before 1 April (the Court announced *West Coast Hotel v. Parrish* on 29 March) and 1 thereafter; $NLRB_{3t}$ = a dummy variable denominated 0 before 12 April and 1 afterward (*NLRB v. Jones and Laughlin Steel* came down on 12 April); VDV_{4t} = a dichotomous variable scored 0 before Justice Van Devanter's resignation on 18 May and 1 thenceforth; and $TREND_{5t}$ = a dummy variable counter for time from 1 to 18. In this model, b_1 , b_2 , b_3 , and b_4 estimate any post intervention changes in the intercept. So, for instance, b_1 represents the immediate impact of President Roosevelt's speech. Based on the considerations outlined in the Political Events and Public Opinion section, I anticipate $b_1 > 0$ and b_2 , b_3 , and $b_4 < 0$.

In the previous section, I discussed the sources and properties of the two indicators of the mass media's treatment of the Supreme Court. For the direction of stories in the *New York Times*, as in Figure 2, I rely on a simple percentage. The indicator of media attention poses one problem. With such a profusion of stories at the outset of the period (see Figure 1), the series contains a formidable trend downward ($NYST_t = 83.9 - 4.24$

$TREND_t$; $r^2 = .85$). The series is, in econometric parlance, "nonstationary." To make the series stationary, I have differenced the number of stories—change in attention is a function of this week's coverage minus last week's coverage. The final model, then, is

$$S_t = b_0 + b_1 FDR_{1t} - b_2 PAR_{2t} \\ - b_3 NLRB_{3t} - b_4 VDV_{4t} \\ - b_5 TREND_{5t} + b_6 NYBAL_{6t} \\ - b_7 CNYST_{7t} + e_t$$

where the notation through $TREND_{5t}$ remains as before; $NYBAL_{6t}$ = the balance of stories in the *New York Times* in favor of Court packing and FDR's approach to the Court, counted weekly, in percentages; $CNYST_{7t}$ = the change in the number of stories from one period to the next; and e_t is a term for errors. On the basis of the discussion in the Effects of Mass Communications section, I expect $b_6 > 0$ and $b_7 < 0$. In the initial runs, the coefficient for $TREND_{5t}$ failed to reach statistical significance; therefore, in the interest of conserving degrees of freedom, I dropped it in subsequent analyses. To estimate the coefficients, I have used a method equivalent to generalized least squares.⁵

Now is the time to cast an eye on and describe the dependent variable, support for Court packing. Figure 3 records support for, opposition to, and lack of, opinion on FDR's plan among 18 national samples.⁶ Support for FDR's proposal varied from a high of 46% immediately after his fireside chat to a low of 31% in the wake of Justice Van Devanter's resignation. Over the entire period, support averaged about 39%. Opposition to Court packing ranged from a low of 41% on 24 March to a high of 49% on 3 March. On average, about 46% of each sample indicated opposition to President Roosevelt's proposed legislation. And it is clear that, after a surge from an early push by FDR, the public support for re-

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structuring the Court rapidly melted. Without extensive analysis of the original surveys, one cannot know with certainty, but I surmise from the patterns that most of the ups and downs in Roosevelt's support probably resulted from movements to and from the realm of no opinion. Opposition remained relatively steady. The proportion with no opinion bobbed around a good deal, from a low of 9% on 3 March to a high of 25% on 19 May.

The Results

Table 1 presents the products of a multiple regression of support for Court packing on the independent variables, corrected for autocorrelation. In general, the results are crisp: five of the six coefficients reach statistical significance, the direction of signs makes sense, and the fit is tight ($R^2 = .88$). Figure 4, a plot of the actual observations of support for Court packing on the predicted values, illustrates the high quality of the statistical explanation; only a few substantial residuals materialize.

The coefficients in Table 1 dramatically demonstrate the considerable latitude the Supreme Court has in shaping the contours of public opinion. President Roosevelt's efforts to persuade the Democratic party and the public of the plan's urgency, on 4 March at the victory dinner and over the air on 8 March, did make a statistically significant—if substantively marginal—impact on support for his bill. In the short run, at least, FDR's speeches boosted support by nearly 3% ($b_1 = 2.7$). Nevertheless, in comparison to the Supreme Court's actions, the president's campaign made only a modest dent in public opinion.

Somewhat surprisingly, the overruling of *Adkins v. Children's Hospital* in *West Coast Hotel v. Parrish* on 29 March made little or no difference in the pattern of public support ($b_2 = -1.2$; $t = -.6$). In the scholarly literature and the textbooks,

Figure 3. Support for Court Packing

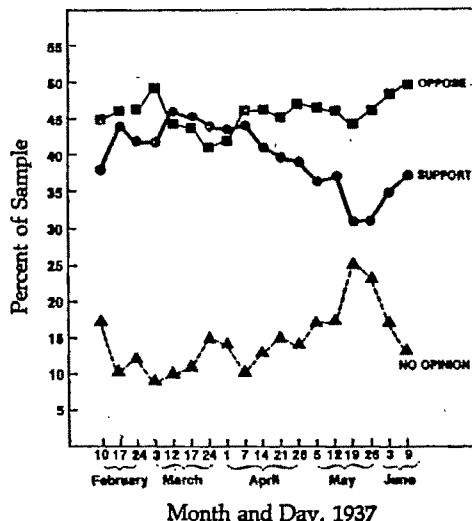


Figure 4. Support for Court Packing:
Actual vs. Predicted

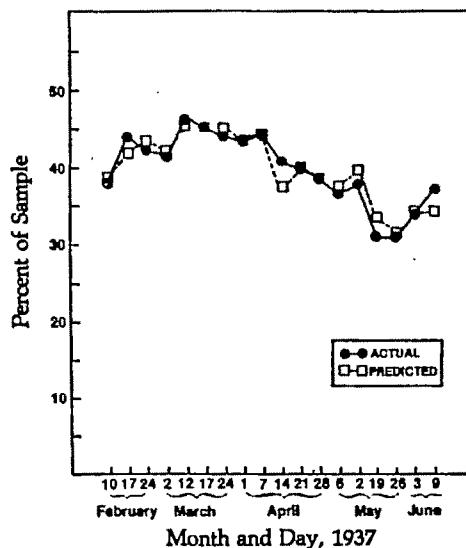


Table 1. Support for Court Packing

Independent Variable	b	t-Ratio	Level of Significance*
Intercept	38.97	16.35	.0001
Political events			
FDR's speeches	2.67	1.73	.0500
Overruling of <i>Adkins</i>	-1.16	-.64	n.s.*
Upholding of Wagner Act	-4.31	-2.48	.0150
Van Devanter's resignation	-5.63	-4.18	.0010
Communications			
Change in media coverage	-.04	-1.68	.0500
Direction of media coverage	.08	1.43	.0750

Note: $R^2 = .88$; $df = 11$; $N = 18$.

*Two-tailed test of significance.

*Statistically significant at .10 or better.

of course, *Parrish* marks a watershed in constitutional development; but after fits and starts in the Court's hospitality toward state regulation of business, it is doubtful whether many observers saw the decision that way. Moreover, because the Supreme Court in *Parrish* dealt with a state law, the connection between the decision and the New Deal was only indirect. This is so even though the overruling of *Adkins* signaled a retreat and commentators rank it as important as *NLRB v. Jones and Laughlin Steel*. It expressed a change in attitude, and that was apparently too subtle for the public to detect; for we know that few members of the relevant political elites actually read the Court's opinions or make themselves familiar with their contents. And, clearly, even a smaller segment of the mass public monitors as much as the general direction of the Court's jurisprudence, let alone specific decisions. It is probably too much to expect the public to react sharply to subtle shifts in public policy.

Upholding the Wagner Act, a contemporary lynchpin of the New Deal and salient because of the recent sit-down

strikes, helped enormously in building opposition to the president's measure. As a result of *NLRB v. Jones and Laughlin Steel*, FDR and his allies lost more than 4% of the public ($b_3 = -4.3$; $t = -2.3$). In the short run, the president's loyalists tried to shore up support and portrayed the decision on the Wagner Act as a temporary, strategic retreat on the part of the justices. That approach obviously did not work. Key political actors, inside and outside the halls of Congress, who understood the decision could readily see the radical departure from the narrow conception of interstate commerce dominant on the Court during the 1930s that Chief Justice Hughes's opinion implied. These results suggest that elite views diffused among the general public in the well-known two-step flow of information and opinions.

Justice Van Devanter's resignation—expected all along—sent the enemies of the Supreme Court into a tailspin. The announcement of Van Devanter's exit alone subtracted more than 5% ($b_4 = -5.6$) from the segment supportive of restructuring the Court. With that resignation, the prospect of a new justice, and

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the streak of 5-4 decisions, it must have seemed clear to many that FDR had a chance to have a sympathetic if small coalition on the Court. Van Devanter's departure, on the heels of *NLRB v. Jones and Laughlin Steel*, made it difficult for even the most loyal New Dealers to demonstrate the need for radical judicial reform.

Two crucial events—*Jones and Laughlin Steel* and Justice Van Devanter's resignation—spelled doom for FDR's bill to enlarge the Court and pack it with justices favorable to the New Deal. Between the two decisions, the Supreme Court decreased support for President Roosevelt's proposal by nearly 10%. Commentators and the justices themselves speak of the Supreme Court as relatively helpless in the arena of public opinion; but these results suggest the contrary. Planned or not, the Court's actions played a dramatic role in defeating the president. Chief Justice Hughes's active participation and superb timing in supplying information and surfacing in the news on occasion during the crisis indicate a great deal of political savvy (Mason 1956). Hughes, of course, had served in many political roles, and he behaved in a manner we might expect from an experienced politician.

The mass media's treatment of the Supreme Court made an appreciable impact on the shape of public sentiment for restructuring. For each 12% increase in favorable coverage of the plan in the *New York Times*, support for Court packing increased 1% ($b_6 = .08$). Since on average 40% of the stories put FDR's plan in a positive light, the *New York Times'* treatment contributed about 3.2% to the coalition in support of Court packing. Change in media coverage, on the other hand, detracted from the plan's support: for each increase of 25 stories, the segment in favor of the proposal declined by about 1% ($b_7 = -.4$). Thus, as I suggested, the bright light of increased scrutiny actually hurt its chances. This finding

dovetails nicely with results I have reported elsewhere: in a study of public support for the Court in 1966-84, increases in media coverage of the Court increased support for the justices. The opponents of FDR managed to expand the scope of conflict to include all manner of people and groups heretofore unorganized. Extended media attention to the issue aided and abetted that mobilization. The intensity and direction of media coverage apparently exercised an equal influence on support for Court packing—after standardization, for media attention, $B_6 = -.20$; for the balance of coverage, $B_7 = .19$. In sum, then, media coverage made a difference in the battle over public opinion.

Conclusions

Public support for Court packing varied considerably over the space of 18 weeks and defies a simple account; but I have tried to trace meaningful shifts and to frame and evaluate explanations of the dynamics of public opinion on this issue. How can we explain the ebb and flow of sentiment for the president's plan (and presumably against the Supreme Court)? The rival hypotheses included political events—FDR's speeches, the Court's decisions in *Parrish* and *Jones and Laughlin Steel*, and Van Devanter's resignation—and the salience and direction of coverage in the mass media. Political events and mass communication both shaped public attitudes on the issue in the aggregate. Even more important, for present purposes, the actions taken by the Court and the justices played a crucial part. Historians will no doubt continue to argue about the political motivations behind and the timing of the Supreme Court's decisions; but regardless of the intentions of the participants, the Supreme Court's behavior made a difference. And to the outside observer, the Supreme Court did

not seem to be a passive party, sitting idly by on the sidelines.

Scholarship on the origins and defeat of FDR's plan to restructure the federal judiciary, like so much else written on the Supreme Court, often subscribes to a mythical view of public attitudes toward the high bench (for an old but classic example, see Lerner 1937). Thus, for example, a recent student of the skirmish states that "for various reasons, Americans generally have regarded the judicial process as somehow outside or above politics" (Nelson 1986, 14). Trying to fathom FDR's failure in the arena of public opinion, a distinguished historian of the period has concluded that the "greater the insecurity of the times, the more people cling to the few institutions that seem unchangeable" (Leuchtenberg 1963, 235-36). Perhaps members of the public hold the Supreme Court in reverence and that is why FDR's proposal went up in flames. Yet the evidence accumulated over the years goes against that notion of the relationship between the public and the Court. I prefer, instead, a much more straightforward account: the Supreme Court outmaneuvered the president. Through a series of shrewd moves, the Court put President Roosevelt in the position of arguing for a radical reform on the slimmest of justifications.

Those moves, of course, spelled an important jurisprudential retreat on the part of the Court. President Roosevelt, in essence offered the Supreme Court a choice between substantive policy and structural integrity. The Court wisely chose to give up on the substantive issues and preserve its structural integrity. And public opinion, as we have seen, supported the Court in this choice. Perhaps one lesson we can extract from these results is that if the justices wish to gain public support in battles with the popularly elected branches, they must first

assess whether the public stands with the president and Congress on the substantive issue; and, if so, the Supreme Court can preserve its institutional integrity by retreating on the issue. The results I have reported here suggest that the public will support existing constitutional arrangements or, at a minimum, existing arrangements as they pertain to the judiciary, so long as these arrangements are the sole issue. It is not at all clear what the public would have done if the Court had insisted on continuing its opposition to the New Deal. Congress and the president cannot successfully diminish the Supreme Court's structural integrity unless they have a major substantive dispute with the Court on issues to which the public has a strong commitment. Even then, we do not know whether the public would choose structural integrity or substantive policy. We do know that even an overwhelmingly popular public official cannot attack the structural integrity of the Court without the benefit of a very important policy issue for which he or she has overwhelming public support.

The research reported here raises many related questions. Did the political events in the spring of 1937 exert a differential impact across strata in the population? It is quite plausible, for example, that Democrats and union members responded differently to the issue than did Republicans and white-collar workers. Did FDR's fireside chat make a bigger mark among his loyalists and in parts of the nation more sympathetic to the New Deal than among others? Observations over time can help us to come to grips with that sort of question. In the cross-sectional case, can we identify the social, political, and economic bases of support for and opposition to the Supreme Court? The answers to these and other questions have important implications for the study of public opinion and the Supreme Court.

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Notes

For some of the data presented here, I appreciate the assistance of the staff of the Roper Opinion Research Center. Michael S. Lewis-Beck advised on some statistical issues, and Peverill Squire made thoughtful comments on an earlier draft.

1. In this section and the next, I have relied heavily on the historical accounts cited here. To smooth the narration, I have refrained from making references to particular books or articles along the way. In his three articles, Leuchtenberg (1966, 1969, 1985) provides an especially good collection of sources on Court packing.

2. If a story, letter to the editor, or editorial praised, endorsed, or sympathized with President Roosevelt's approach to the Court, I coded it as *anti-Supreme Court*. If, on the other hand, coverage placed the Court in a positive light or reported criticism of FDR's proposal, I designated it as *pro-Supreme Court*. Letters to the editor and editorials created few problems. Straight reports proved more difficult. If a story reported on someone who criticized the proposed legislation, I classified it *pro-Court*. Since few people or institutions adopted a neutral stance, I had relatively little trouble in making judgments.

3. Naturally I would like to have data on the balance of coverage from a wide variety of newspapers. That would have entailed far more greater resources than I had at my disposal. Since the data from the *New York Times* mask the variations among newspapers across the nation and attenuate the relationship between the direction of coverage and changes in public opinion, I have undoubtedly underestimated the impact of the mass media on support for Court packing.

4. For simple interrupted time-series analysis, the multiple regression equation takes on the form

$$Y_t = b_0 + b_1 X_{1t} + b_2 X_{2t} + b_3 X_{3t} + e_t$$

where Y_t = N time-series observations on the dependent variable; X_{1t} = a dummy variable counter for time from 1 to N ; X_{2t} = a dichotomous dummy variable scored 0 for observations before the event and 1 for observations after; X_{3t} = a dummy variable counter of time scored 0 for observations before the event and 1, 2, 3, . . . for observations after the event (see Lewis-Beck 1986, 215).

5. More specifically, I used the AUTOREG procedure in SAS. It first estimates the model with ordinary least squares and then obtains estimates of the autoregressive parameters. Accordingly, the program transforms the original data by the appropriate autoregressive model. Finally, after making adjustments, SAS reestimates the ordinary least-squares regression. If all goes well, the procedure should eliminate the problems caused by autocorrelation. (See SAS Institute [1982, 187-202].) For

this equation, I encountered no significant autoregressive parameters [AR(1) = .17; AR(2) = .00]. Thus, the results reported in Table 1 are the product of ordinary least squares.

6. In the first half of the series, the Gallup Poll asked, "Are you in favor of President Roosevelt's proposal regarding the Supreme Court?" Respondents could answer *yes*, *no*, or *no opinion*. Later, for inexplicable reasons, Gallup shifted the wording slightly to "Should Congress pass the president's Supreme Court plan?" Changes in wording are always cause for worry, but my analysis indicates that this particular alteration made little difference. First of all, relationships between support for Court packing and various independent variables such as partisanship, attitude toward FDR, and demographics did not change. Second, in the context of the fully specified model, a dummy variable for question wording does not reach statistical significance. Third, the substance of the question did not change, even though the wording did. Before and after the change, respondents had to make a decision on FDR's proposed law; Court packing constituted the stimulus in both versions.

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THE CORE OF THE CONSTITUTION

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It is often argued that the United States Constitution was designed so as to create a stable political order. Yet in the literature on the formal theory of democracy, there has been very little examination of constitutional provisions for their stability-inducing properties. In this paper we demonstrate that bicameralism and the executive veto tend to create stability, that the legislative override of the executive veto tends to undermine this stability, and that the interaction of bicameralism and the executive veto is likely to produce stable outcomes despite the destabilizing impact of the veto override.

Designing a constitution that would ensure stability in the laws was a major concern of some of the central participants in the Constitutional Convention. In his defense of the resulting document in *The Federalist Papers*, for example, James Madison argued that the proposed Senate, with its slow turnover and insulation from the public, was needed to help avoid instability: "The mutability in the public councils arising from a rapid succession of new members, however qualified they may be, points out, in the strongest manner, the necessity of some stable institution in the government" (*Federalist* no. 62 [p. 380]). In Madison's view this "mutability in the public councils" cost greatly: "To trace the mischievous effects of a mutable government would fill a volume" (p. 380).

The costs of instability were both external and internal in nature. Externally, instability causes the nation to forfeit "the respect and confidence of other nations" (p. 380): "Every nation . . . whose affairs betray a want of wisdom and stability, may calculate on every loss which can be

sustained from the more systematic policy of its wiser neighbors" (p. 381). Internally, the consequences of instability were even worse: "The internal effects of a mutable policy are still more calamitous. It poisons the blessings of liberty itself. It will be of little avail to the people that the laws are made by men of their own choice if the laws be so voluminous that they cannot be read, or so incoherent that they cannot be understood; if they be repealed or revised before they are promulgated, or undergo such incessant changes that no man, who knows what the law is today, can guess what it will be tomorrow" (p. 381). This instability had distributional consequences in the society: "Another effect of public instability is the unreasonable advantage it gives to the sagacious, the enterprising, and the moneyed few over the industrious and uninformed mass of the people. Every new regulation concerning commerce or revenue, or in any manner affecting the value of the different species of property, presents a new harvest to those who watch the change, and can trace its consequences" (p. 381). Com-

merce could also expect to suffer from unstable government: "The want of confidence in the public councils damps every useful undertaking, the success and profit of which may depend on a continuance of existing arrangements. What prudent merchant will hazard his fortunes in any new branch of commerce when he knows not but that his plans may be rendered unlawful before they can be executed?" (p. 381).

The Senate, with its members' six-year terms, their election by state legislatures, and the chamber's slow rate of turnover, could be expected to help avoid this instability. The independently elected president with his veto power was also expected to help. Hamilton argued in *Federalist* number 73 about the consequences of the veto: "It may perhaps be said that the power of preventing bad laws includes that of preventing good ones; and may be used to the one purpose as well as to the other. But this objection will have little weight with those who can properly estimate the mischiefs of that inconstancy and mutability in the laws, which form the greatest blemish in the character of genius of our governments. They will consider every institution calculated to restrain the excess of law-making, and to keep things in the same state in which they happen to be at any given period as much more likely to do good than harm; because it is favorable to greater stability in the system of legislation" (pp. 443-44).

Rapid turnover of legislators was one cause of instability in the laws, and Madison was also concerned about "the propensity of all single and numerous assemblies to yield to the impulse of sudden and violent passions, and to be seduced by factious leaders into intemperate and pernicious resolutions" (*Federalist* no. 62 [p. 379]). But there is reason to think that Madison's views on the instability of republics were also buttressed by a deeper theoretical under-

standing of the instability of majority rule. Theorists of democracy have long been troubled by the fact that a key element of most democratic systems—majority rule—has some undesirable properties. Among these is the fact that for any option that might be chosen by a majority of voters there is usually some other option, preferred by a different majority, which can upset it. Recognition of this problem can be traced back at least two hundred years, to Condorcet's seminal *Essai* (1785) and McGrath (1983, chap. 3) points out that Madison was well acquainted with Condorcet's work; Madison even wrote a review (now unfortunately lost) of the *Essai* and its arguments.

For Madison, then, there were theoretical as well as empirical reasons for designing a government that did not depend on simple majority rule. The Constitutional Convention's chosen design involved, in part, the separation of powers. That the separation of powers has indeed played a major role in creating a stable republic has been argued by many generations of historians and political scientists.

Yet while the formal literature on the theory of democracy has explored the nature of majority-rule instability in great detail—see Cohen 1979; McKelvey 1976, 1979; Miller 1980; Schofield 1978; and Shepsle and Weingast 1984, for example—there has been almost no study of whether a separation-of-powers system has the stability-inducing properties that Madison and Hamilton desired. As far as we can find, only Cox and McKelvey 1984 and Cox 1980 even address the relationship between bicameralism and stability. The former informally describes general conditions for stability in multicameral systems (p. 77). The latter argues that dividing a legislature into two chambers whose members have completely separate Pareto sets will ensure stability in two dimensions (see p. 10). Cox further

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conjectures that adding more policy dimensions can destabilize the situation: it appears that as many veto groups (e.g., chambers or committees) as dimensions are needed to guarantee stability.

Our work is in some ways more developed and in other ways more limited than that of Cox and McKelvey. For the set of constitutional provisions governing the legislative process—bicameralism, the executive veto, and the legislative veto override—we provide tools and formal results for characterizing the existence, shape, and location of any set of stable points. However, we assume that each legislator and the executive has ‘Euclidian preferences,’ that is, a most-preferred point and circular indifference curves in the issue space. We also restrict our analysis to the case of two dimensions. If legislators and the executive are considering an issue involving no more than two dimensions, the constitutional rules we are considering may well create stability. For more complex issues, the constitutional structure may be an insufficient guarantor of stability, and additional institutional features may be required.

Pure Majority Rule and the Bicameral Game

In a social institution, one alternative *dominates* another alternative when there is a set of individuals whose members all prefer the first alternative to the second and who can, given the rules of the game, enforce the first alternative over the second. The *core* of this institution is the set of undominated alternatives. In a pure majority-rule legislature, the core is the alternative that can gain a majority of votes against any other in a two-way vote. When there are three or more legislators and two or more dimensions by which options can be evaluated, a simple majority-rule legislature—playing what we will call a “unicameral game”—gener-

ally lacks a core. That is, for each possible legislative choice there almost always exist other options that are majority preferred (McKelvey 1976, 1979).

For example, consider the illustration in Figure 1. If the six legislators vote by simple majority rule, legislators H_1 , H_2 , H_3 , and S_3 all prefer alternative p to c : p lies inside the (circular) indifference curves through c of legislators H_1 , H_2 , H_3 , and S_3 . A different majority coalition prefers e to p (indifference curves—not drawn—through p of S_1 , S_2 , S_3 , and H_2 all contain e). Yet another coalition (H_1 , H_2 , S_1 , S_2) prefers c to e . No alternative can gain the support of a four-member majority coalition against all other alternatives.

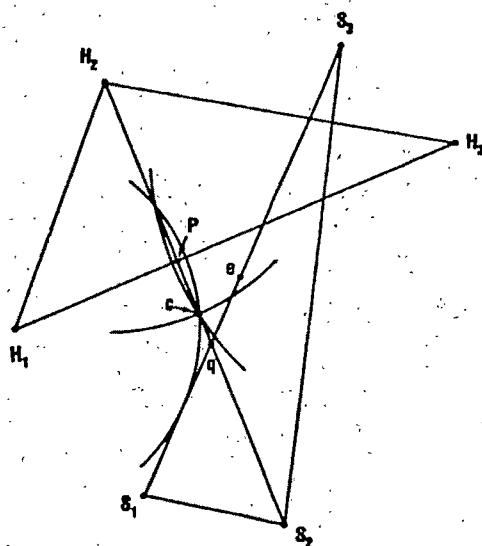
Various constitutional provisions can help overcome this generic instability of simple majority rule. Suppose H_1 , H_2 , and H_3 are all assigned to one chamber (the House), while S_1 , S_2 , and S_3 are all assigned to a different chamber (the Senate). For a bill to pass, it must gain the support of a *particular* four-person coalition—a *joint majority*—consisting of a majority of *each* chamber. These same six legislators no longer generate a majority-rule cycle. There is now a core that includes the point c : no joint majority will upset c in favor of some other point. The institution of bicameralism thus induces stability, given these six legislators’ preferences.

The following section introduces theorems about when bicameralism has this stability-inducing effect. We also show how to find the shape of the bicameral core when it exists.

The Bicameral Game

Madison’s analysis of bicameralism in *Federalist* numbers 62 and 63 lays out, in surprisingly accurate terms, the conditions under which bicameralism can avoid instability. The basic logic, he suggested in discussing the likelihood that the House

Figure 1. A Six-Person Legislature
with a Bicameral Core
but No Unicameral Core



and Senate would agree on undesirable policies, was that "the improbability of sinister combinations will be in proportion to the dissimilarity in the genius of the two bodies" (no. 62 [p. 379]). In our terms, when the ideal points of the members of the two chambers are sufficiently distant from each other, legislative outcomes will be stable: the bicameral game will have a core. Moreover, the outcomes will always lie within the legislative Pareto set: clearly undesirable policies are thereby avoided, as Madison desired.

The required separation between chambers was to be achieved by insulating the Senate from the popular passions to which the House would be exposed. If the Constitution was designed so as to induce in senators preferences substantially different from those induced in representatives, how great must these interchamber differences be for bicameralism to produce a core? To answer this question it is first useful to introduce several definitions and lemmas.

Lemmas about Bisectors

For the members of one chamber, draw the contract curve between the ideal points of each pair of members and extend each contract curve indefinitely in each direction. Some of these contract curves as extended (henceforth we refer to them simply as *contract curves*) will meet both of the following conditions:

1. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a majority of the chamber's members
2. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute a majority of the chamber's members

A chamber-member/chamber-member contract curve meeting both conditions is a *chamber bisector*. It is easily proved (Hammond and Miller 1987, 5, Lemma 1) that every chamber will have at least one chamber bisector.

If a contract curve is a chamber bisector, it is *attractive both ways*. That is, for any point not on the chamber bisector, there is at least one point on the chamber bisector that is majority preferred; see Davis, DeGroot, and Hinich 1972, where chamber bisectors are known as *medians*, or, in n -dimensional space, *median hyperplanes*. A simple geometric version of this assertion will help intuitive understanding:

LEMMA 1. *A chamber bisector is attractive both ways* (Davis, DeGroot, and Hinich 1972, Theorem 2).

Proof. The definition of *chamber bisector* implies that there will always be a majority of members on or to one side of the chamber bisector. It is therefore only necessary to prove that any point w on the *other* side of the chamber bisector from this majority can be beaten by some point on the bisector supported by this

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majority. In particular, consider the point x on the chamber bisector that is closest to w . (It will be the base of a perpendicular dropped to the bisector through w .) The point x will be closer to the ideal points of every legislator on the chamber bisector. It will also be closer to the ideal points of every legislator on the other side of the chamber bisector from w . Thus, the proposed majority will be willing to upset any point on the opposite side of the chamber bisector. The same argument applies to the analogous majority on or to the other side of the chamber bisector. Hence every point off the chamber bisector can be upset by some point on the chamber bisector. QED

There is nothing about a chamber bisector's properties that tells us what is in the core of the bicameral game. But Lemma 1 will be useful in telling us what points are not in the core.

Next, draw each House-member/Senate-member contract curve and extend the curve indefinitely. A joint majority consists of a majority of the House plus a majority of the Senate. When there is an odd number of members in each chamber, one or more of these cross-chamber contract curves will meet both of the following conditions:

1. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a joint majority
2. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute a joint majority

When there is an even number of members in one or both chambers, one or more of these cross-chamber contract curves will meet both of the following conditions:

3. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a joint

majority

4. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute no fewer than one less than a majority in each chamber

A House-member/Senate-member contract curve meeting one of these pairs of conditions is a *bicameral bisector*. In Figure 1, the H_2S_2 contract curve is a bicameral bisector. As with a chamber bisector, it is easily proved that a bicameral bisector will always exist in a bicameral legislature.

If a House-member/Senate-Member contract curve is a bicameral bisector, it is *attractive one way* and may be, but is not necessarily, *attractive both ways*. If it is attractive both ways, for any point not on the bicameral bisector, there is a point on the bisector preferred by a joint majority. For an odd number of members in each chamber the following holds:

LEMMA 2. *If each chamber has an odd number of members, each bicameral bisector has a joint majority on or to each side and is thus attractive both ways.*

Proof of this lemma is almost identical to that of Lemma 1.

If one or both chambers has an even number of members, the bicameral bisectors will commonly have a joint majority only on or to one side—that is, Conditions (3) and (4) are usually met—and each bisector will be attractive only one way.

We can now state the most general conditions for a bicameral game to have a core:

THEOREM 1. *A point x is in the core if and only if no straight line through x leaves a majority of both chambers to one and the same side of that line.*

Proof consists of showing that a core exists when, for each majority in one

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chamber that wants to move in one direction from x , at least half of the other chamber will want to move in the opposite direction from x (see Appendix).

Theorem 1 defines the most general conditions under which a bicameral core will exist. But the theorem is not very useful for determining if a core exists with any particular preference profile: we would have to examine every possible x in the policy space to see if it meets both conditions. And finding one such point tells us little about the existence or location of any other such point.

However, identification of the chamber and bicameral bisectors leads directly to techniques for deducing when a core will exist and establishing what it looks like. Let us first look at the example in Figure 1. The House-Senate bicameral game has a core, which is that portion of the H_2S_2 bicameral bisector lying between House chamber bisector H_1H_3 and Senate chamber bisector S_1S_3 ; that is, the core is line segment pq . The reason? By Lemma 2, H_2S_2 is attractive both ways, so points not on H_2S_2 are not in any core. Furthermore, while a chamber bisector is attractive both ways when there is just a single chamber (Lemma 1), in a bicameral legislature it is attractive in a given direction only if a majority of the other chamber concurs. For example, points above H_1H_3 are attracted to H_1H_3 (from H_2 's general direction) because all three members of the Senate would agree with H_1 and H_3 to move to H_1H_3 . However, points below H_1H_3 are not necessarily attracted to H_1H_3 (from S_2 's general direction) because a majority of the senators would not concur. A similar argument applies to points above and below S_1S_3 . Points on H_2S_2 above H_1H_3 and below S_1S_3 are thus eliminated from any core. What remains is pq .

Note in this example that neither chamber has a core, nor does the unicameral legislature formed by combining the two chambers into one large chamber. Yet the bicameral game does have a core.

If one chamber has an even number of members, this increases the size of the core. Instead of having a single pivotal member at the end of a bicameral bisector, there are generally two bicameral bisectors leading to two pivotal members of the chamber with an even number of members. The *area* between these bicameral bisectors contains candidates for the core (see Hammond and Miller 1987, fig. 5 for an example).

What underlies the Figure 1 example is the fact that there is a set of points on a bicameral bisector that lies between all House chamber bisectors on the one hand and all Senate chamber bisectors on the other. With this observation, we now present a theorem about the existence of a core in bicameral games with one bicameral bisector:

THEOREM 2. *If (1) a bicameral legislature has only one bicameral bisector and (2) there is a point x on the bicameral bisector such that the chamber bisectors from one and only one chamber intersect at the bicameral bisector in each direction from x , then x is a core point.*

(Claim 2 in Cox 1980, p. 10 is implied by this theorem: nonoverlapping of chamber Pareto sets is sufficient for stability but not necessary.) This theorem applies to examples like Figure 1. In effect, for each x on pq , each House majority wants to move in one direction along pq and each Senate majority wants to move in the opposite direction along pq . Stalemate—stability—is the result. (See Appendix for formal proof.)

Bicameral games do not necessarily have a core. In general, the more the chambers resemble each other—the more overlapping their sets of ideal points—the less likely it is that bicameralism will create a core. While the authors of the Constitution desired the House and Senate to be substantially different from each other, it would seem for contem-

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porary Congresses that the ideal points of the two chambers are rather similar, perhaps due to the impact of the 17th Amendment requiring the popular election of senators, perhaps due to the development of the party system.

As a result, there will usually be more than just the one or two bicameral bisectors we have analyzed so far. In these more complex cases, determining whether a core exists is eased by the following theorem:

THEOREM 3. *When there is an odd number of members in each chamber, there are three or more bicameral bisectors, and the bicameral bisectors do not all intersect at the same point, then there is not a core in a bicameral game.*

The proof is simply that since each bicameral bisector is attractive both ways (Lemma 2), each bisector attracts points from off itself, including points on other bicameral bisectors. This means that every point in the space is attracted by some other point. Hence there is no core. Since it seems unlikely that three or more bicameral bisectors will all intersect at precisely the same point, we pose the following:

CONJECTURE 1. *When each chamber has an odd number of members and there are three or more bicameral bisectors, the bicameral game usually has no core.*

Examples of cases in which one or both chambers has an even number of members (see Hammond and Miller 1987, Fig. 11) lead us to venture the following proposition:

CONJECTURE 2. *When there are three or more bicameral bisectors and there is an even number of members in one or both chambers, there is usually a core in a bicameral game.*

The Preservation of Prior Cores in Bicameral Games

Bicameral games can have a core when neither chamber has a core. The significance of this result might be undercut if bicameralism could at times destroy any core that the larger unicameral game (combining all members in one chamber) might have. We can demonstrate, though, that when a unicameral game does have a core, any bicameral game derived from the unicameral game by partitioning its members into two chambers (holding constant each legislator's ideal point) will still have a core. In fact, it is easily proved that the bicameral core will include the unicameral game's core:

THEOREM 4. *If a unicameral game has a core, then the bicameral core exists and includes the unicameral core.*

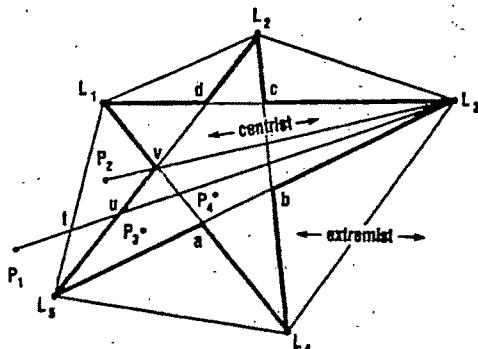
Proof. By contradiction: if the unicameral game has a core but the derived bicameral game has no core, this means that for every x in the issue space, there is a joint majority favoring some y to x . This implies that a majority of all legislators prefer the y to x . But this contradicts the condition that the unicameral game has a core. Hence, if the unicameral game has a core, the bicameral game must have a core. QED

This theorem means that bicameralism cannot be used as an institutional device for destabilizing simple majority rule in those cases in which simple majority rule produces stable outcomes. However, since bicameralism can increase the size of the core when there is a simple-majority-rule core, bicameralism can allow a different outcome than would occur due to simple majority rule.

The Executive-Veto Game

A bicameral game is not guaranteed to have a core. However, bicameralism is not the only constitutional provision that

Figure 2. A Five-Person Unicameral Legislature Facing a Chief Executive at Four Different Locations



can induce stability. We have already cited Hamilton's comment in *Federalist* number 73 that the executive veto would help maintain stability. It might be argued, on the basis of the historical record, that the veto is of little consequence in relations between president and Congress since it is used so infrequently. However, Hamilton suggested in *Federalist* number 74 that the veto does not have to be used to be effective: "A power of this nature in the executive will often have a silent and unperceived, though forcible, operation. When men, engaged in unjustifiable pursuits, are aware that obstructions may come from a quarter which they cannot control, they will often be restrained by the bare apprehension of opposition from doing what they would with eagerness rush into if no such external impediments were to be feared" (p. 446). If Hamilton was right, then we can expect the veto power to induce a core in an executive-veto game even if it is seldom exercised.

Since a simple veto allows the executive to veto any move from the executive's ideal point, that ideal point must be a part of a core. No further proof about this "executive veto game" is required to assert:

THEOREM 5. *The executive-veto game has a core that includes the executive's ideal point.*

We will next demonstrate that the core of the executive-veto game is not necessarily restricted to the executive's ideal point.

The Unicameral Executive-Veto Game

Here we explore executive veto games between an executive and a single legislative chamber. In Figure 2 the executive is located at any of the points labeled P and faces a five-person chamber. There are five chamber bisectors (L_1L_3 , L_3L_5 , L_5L_2 , L_2L_4 , and L_4L_1) which together enclose a five-pointed star defined by the $L_1L_2L_3L_4L_1$ sequence of points. Label an executive ideal point lying outside the star—outside the space completely bounded by chamber bisectors—as *extremist* and one lying on or inside the star—on or inside a space bounded on every side by chamber bisectors—as *centrist*.

If the executive's ideal point is centrist, the following is easily proved:

THEOREM 6. *If the chamber has no core and if the executive has centrist preferences, the core of a unicameral executive-veto game contains only the executive's ideal point.*

For example, in Figure 2 consider the executive ideal point at P_3 . We treat the executive as a separate chamber. There are three "bicameral bisectors" (not drawn)— P_3L_2 , P_3L_5 , and P_3L_1 . Since each bicameral bisector is attractive both ways, the executive can use some legislative coalition to upset every point in the space with an option closer to his or her own ideal point; the only point that cannot be upset is the ideal point. For similar reasons, an executive ideal point at P_4 is the only point in the core.

If the executive has extreme preferences—at P_1 or P_2 outside the star—the bicameral bisectors are P_1L_3 and P_2L_4 .

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respectively. With P_1 , the first chamber bisector that crosses the P_1L_3 bicameral bisector is L_2L_5 ; the point of intersection is u . The L_2L_5 chamber bisector attracts points from its right (the executive concurs) but not from the left (the executive would veto). Hence the core is P_1u . With the executive ideal point at P_2 , the core is P_2v .

The Bicameral Executive-Veto Game

The following theorem helps identify the core of a bicameral executive-veto game:

THEOREM 7. *The bicameral executive-veto core includes (but is not restricted to) the House executive-veto core, the Senate executive-veto core, and the House-Senate bicameral core when it exists.*

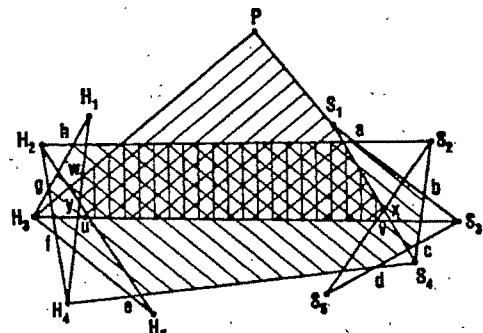
Proof consists of showing that if any two of the three institutions have a "bicameral" core, adding a third institution does not change the first two institutions' joint preference for points in their core over points not in their core. Any "bicameral" core cannot somehow be overruled just because some third institution prefers otherwise.

Figure 3 shows a five-member House and five-member Senate, with executive at P . The core of the bicameral House-Senate game is the uv segment of the H_3S_3 bicameral bisector. The executive-House core is the Pw segment of the PH_3 "bicameral bisector"; the executive-Senate core is the PS_1 segment of the PS_1 "bicameral bisector." The resulting bicameral executive-veto core, encompassed by the $PS_1xvuywP$ lines, is shaded by lines sloping down to the left.

The Veto-Override Game

A unicameral executive-veto game—an executive facing a single chamber—is

Figure 3. A Bicameral Legislature Facing a Chief Executive



guaranteed to be stable. A second legislative chamber is thus unnecessary for stability. The legislature's authority to override the executive's veto does resurrect the specter of instability. However, in *Federalist* number 73 Hamilton defends the override provision with a probabilistic argument about the inability of a bicameral legislature to override the veto. As he remarked, "It is to be hoped that it will not often happen that improper views will govern so large a proportion as two thirds of both branches of the legislature at the same time; and this, too, in defiance of the counterpoising weight of the executive. It is at any rate far less probable that this should be the case than that such views should taint the resolutions and conduct of a bare majority" (p. 446). We will argue that Hamilton's intuitions were largely correct. The stability induced by the veto power is undermined by the veto override, but the requirement of a *dual* override makes it likely that there will be some set of points that cannot be upset.

A result of Greenberg (1979) plays an essential role in our analysis. Define D as the number of policy dimensions under consideration, L as the total number of members in the (unicameral) legislature, m as a bare majority of L , and k as the

number of votes required to upset x in favor of some y ($m < k < L$). A *chamber k-core* is a set of points such that (1) for each y not in the set, there is some x in the set that is preferred by k or more members, and (2) no point in the set is preferred by k or more members to some other point in the set. A chamber *k*-core is, in effect, a generalization of a core for $k \geq m$.

We can now state

LEMMA 3. If $k > [D/(D + 1)]L$, a unicameral game has a chamber *k*-core (Greenberg 1979, Theorem 2).

When there are two dimensions ($D = 2$), for example, a chamber *k*-core is guaranteed to exist when $k > (2/3)L$. Assuming $k > m$, the following are easily shown:

LEMMA 4. If a chamber has a majority rule core, it necessarily has a chamber *k*-core.

(If a chamber has a chamber *k*-core, though, it does not necessarily have a majority rule core.)

LEMMA 5. If a chamber has no *k*-core, it has no majority-rule core.

Now define a *chamber k-sector* as a chamber-member/chamber-member contract curve that has k members on or to one side and the remaining members on the other side. Assuming at least k votes are needed to approve a motion, whenever $k > m$, the following is obvious:

LEMMA 6. A chamber *k*-sector is attractive only one way.

In Figure 2, there are four chamber *k*-sectors, L_1L_3 , L_2L_4 , L_3L_5 , and L_4L_1 . Assuming $k = 4$, together they define the chamber 4-core: it is the pentagon, $abcdva$, internal to the five-pointed star fixed by the five legislators' ideal points.

For a bicameral legislature the analogue of a chamber *k*-core is a *bicameral k-core*.

It is a set of points that no coalition of k_H House members plus k_S Senate members can upset. To find a bicameral *k*-core, define a *bicameral k-sector* as a House-member/Senate-member contract curve with k_H House members and k_S Senate members on or to one side of the line. Assuming k members in each chamber are needed to approve a motion, it is obvious that

LEMMA 7. A bicameral *k*-sector is attractive only one way.

In the bicameral legislature in Figure 3, with $k_H = k_S = 4$, the bicameral 4-core is the $abcdefgha$ region (shaded by lines sloping down to the right) circumscribed by the bicameral 4-sectors (H_2S_2 and H_4S_4) and the chamber 4-sectors (H_1H_3 , H_2H_4 , and H_3H_5 for the House and S_1S_3 , S_2S_4 , and S_3S_5 for the Senate). This bicameral 4-core contains points that no coalition of four or more House members plus four or more Senate members can upset.

Now assume that k is the number of members required to override a veto. Call a chamber majority with at least k members an *override majority* (also called a *k-majority*). Similarly, a *joint override majority* (*joint k-majority*) has at least k_H members in the House and at least k_S members in the Senate. For simplicity in the following analysis we assume chambers of equal (and odd) size and $k_H = k_S = k$.

Given these preliminaries, what conditions are necessary for a core to exist in a veto override game? There are three different cases to examine, whether we are considering a unicameral or bicameral legislature.

For the first case, assume that a *k*-core (unicameral or bicameral) does not exist; if $k \leq (2/3)L$, for example, a *k*-core will not necessarily exist. If no *k*-core exists, there can be no veto-override core. The reason is simple. Since there is no *k*-core, by definition this means that for any x in

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the executive-veto core (which always exists) there is always some override majority (or joint override majority) that can upset x in favor of some y , another override majority that can upset the y in favor of some z , and so forth. In effect, the legislature can eliminate any point in the executive-veto core but, with no k -core, is incapable of replacing the executive-veto core with any points of its own. Hence we can state

THEOREM 8. *If there is no legislative k -core, there exists no veto-override core.*

For the second case, assume that there is a legislative k -core but that the executive-veto core (which always exists) does not intersect it. In this case, too, there will be no veto-override core. The reason? The executive-veto core always attracts points lying outside it. When the core of the executive-veto game does not intersect the k -core, this means that there is an executive-led coalition (containing the executive plus a legislative majority or joint majority) that can upset any point in the k -core. But since the k -core is also attractive (due to the bicameral and/or chamber k -sectors that bound it), any point in the executive-veto core can be overridden by some override majority (or joint override majority) preferring a point in the k -core. Outcomes thus bounce back and forth between the executive-veto core and the legislative k -core. So when the executive-veto core does not intersect the legislative k -core, the veto-override game has no core.

For the third case, assume that the executive veto core *does* intersect the legislative k -core. In this case the veto-override game has a veto-override core, which is the area of intersection of the executive-veto core and the legislative k -core. Consider some point x in this intersection. Since x is in the executive-veto core, no executive-led coalition can upset x . But since x is also in the legis-

lative k -core, there is no legislative override majority that can upset x in favor of something else. Hence x is in the veto-override core. The following theorem thus holds:

THEOREM 9. *For a veto-override game (unicameral or bicameral), there is a veto-override core if and only if (1) the legislature has a k -core, and (2) the core of the executive-veto game intersects this k -core.*

Note an immediate corollary of Theorem 9:

THEOREM 10. *If unanimity is required for the legislature to override, a veto-override game always has a core.*

The reason is that the legislature's entire Pareto set becomes the k -core here, and an executive-veto core always intersects this Pareto set.

We now illustrate these various points.

Unicameral Veto-Override Games

Define an executive's ideal point as *k-centrist* if it lies in a chamber k -core. Otherwise, the executive's ideal point is *k-extremist*.

Now consider a five-person legislature with no core, as in Figure 2. Assume $k = 4$. As previously noted, the legislative 4-core here is the internal pentagon, $abcdva$. With a *k-extremist* ideal point at P_1 , the executive-veto core is the P_{1u} segment of the P_{1L_3} bicameral bisector. Since the executive-veto core does not intersect the legislative 4-core, there is no veto-override core: the executive-veto core attracts points from the legislative 4-core, and vice versa. An executive ideal point at P_3 is also unstable: P_3 is the unique executive-veto core, but it does not intersect the legislative 4-core, hence there is no veto-override core. However, the executive with preferences at P_2 generates an executive-veto core, P_2v , which inter-

sects the legislative 4-core at v . Since v is in the executive-veto core, there is no executive-led coalition that will upset it. Since v lies in the 4-core, there is no four-legislator override coalition that can upset it. Hence v is the unique core to this veto-override game.

In general, if an executive has a k -centrist ideal point, the executive-veto core will always intersect the legislative k -core. Hence the following corollary to Theorem 9:

THEOREM 11. *If there exists a unicameral k -core and the executive has a k -centrist ideal point, the unicameral veto-override game has a core.*

Bicameral Veto-Override Games

Analysis of bicameral veto-override games is similar to that of unicameral veto-override games. Consider Figure 3. The core of the bicameral executive-veto game is the $PS_1xvuywP$ area shaded by diagonal lines sloping down to the left. Assuming $k = 4$, the bicameral 4-core is the $abcdefgha$ region, as previously noted, shaded by lines sloping down to the right. The intersection of the bicameral executive-veto core and the bicameral 4-core is shaded by vertical lines. The joint-override majority eliminates points in the bicameral executive-veto core above the H_2S_2 bicameral 4-sector. The coalitions supporting the executive-veto core eliminate points in the bicameral 4-core below the H_3S_3 bicameral bisector as well as points lying to the left of the H_1H_4 and H_2H_5 chamber bisectors and the PH_3 bicameral bisector and to the right of the S_1S_4 and S_2S_5 chamber bisectors. But some portions of the executive-veto core intersect the bicameral 4-core. This region, shaded by the vertical lines, is the bicameral veto-override core.

The executive ideal point in Figure 3 lies outside the bicameral k -core. The follow-

ing summarizes what happens if it is inside:

THEOREM 12. *If there is a bicameral k -core and the executive's ideal point lies inside it, the bicameral veto-override game has a core.*

The reason of course is that the executive-veto core here necessarily intersects the bicameral k -core. If there is no bicameral k -core, the bicameral veto-override game will have no core. But the following theorem holds:

THEOREM 13. *If either chamber has a chamber k -core, (1) the bicameral legislature has a bicameral k -core, and (2) this bicameral k -core is not restricted to the chamber k -core.*

Finally, the following theorem may seem obvious but will help clarify the discussion in the next section:

THEOREM 14. *If a House-Senate bicameral game has a core, the veto-override game has a core that includes the bicameral core.*

Likelihood of a Bicameral Veto-Override Core

Does the Constitution, which requires a two-thirds vote in each chamber to override a presidential veto, guarantee stability? The answer is that the Constitution does not guarantee stability, but we will argue that stability is nonetheless very likely in the two-dimensional case.

Lemma 3 (Greenberg 1979) guarantees a chamber k -core if $k > (2/3)L$. With a 435-member House, a two-thirds override requires 290 votes. But $290/435 = 2/3$, hence no House k -core is guaranteed to exist. With a 100-member Senate, 67 votes are required for override; this is more than two-thirds. The Senate is thus guaranteed to have a k -core. Hence by Theorem 13 the Congress is guaranteed to

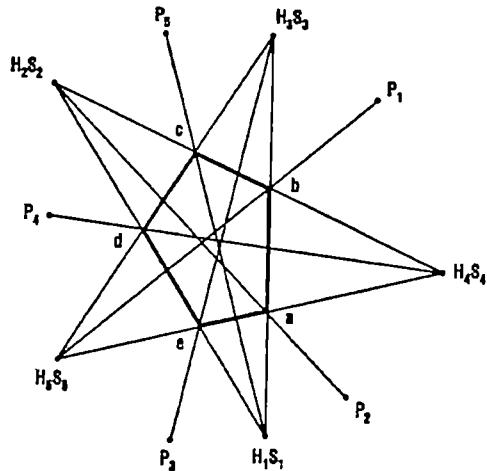
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have a bicameral k -core. But there is no guarantee that the executive-veto core will intersect this bicameral k -core.

Nonetheless, we argue that Hamilton's probabilistic argument is likely to be correct. Let us establish a framework for analyzing his argument. If there is sufficient separation between the ideal points of the House and Senate, as in Figure 3, there will be a core to the bicameral game (Theorem 2) and hence a core to the bicameral veto-override game (Theorem 14). At the other extreme, if there are no differences at all between House and Senate ideal points (in terms of paired House-Senate members), then we have a bicameral version of the unicameral veto-override game. In this case, bicameralism does not add any stability since one chamber is simply the mirror image of the other. The bicameral veto-override game's potential for stability here will be limited to conditions similar to those for unicameral veto-override games (Theorem 9).

Now consider this case in which there is no difference between chambers (see Fig. 4). Each chamber has five members and all five House-Senate pairs perfectly coincide. Assume $k = 4$. An executive inside the bicameral 4-core (labeled $abcdea$ and shown by the heavy outline) will generate an override core (Theorem 12). With an executive ideal point outside the bicameral 4-core, there are five rays on which an executive must lie for there to be a bicameral override core. These rays are the five executive-legislative bisectors— $P_1H_5S_5$, $P_2H_2S_2$, $P_3H_3S_3$, $P_4H_4S_4$, and $P_5H_1S_1$ —that intersect the bicameral 4-core. Only if the executive ideal point lies on one of these five rays—which is very unlikely—will the resulting bicameral executive-veto core intersect the bicameral 4-core. An executive at P_1 , for example, creates an executive veto core, P_1b , which intersects the bicameral 4-core at b . An executive ideal point not on one of these five bisectors will generate no

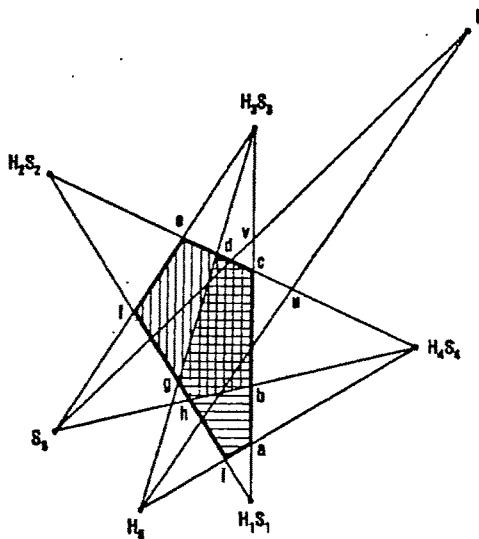
Figure 4. A Bicameral Legislature Facing a Chief Executive at Five Different Locations



override core since the executive-veto core will not intersect the bicameral k -core.

However, if the members of even just one House-Senate pair of legislators do not have identical preferences, then there are numerous prospects for stability that did not previously exist. Consider the two five-person chambers in Figure 5. Members of four of the five pairs of legislators— H_1-S_1 , H_2-S_2 , H_3-S_3 , and H_4-S_4 —have identical preferences; only H_5 and S_5 differ. Assume $k = 4$. There is no core to the overall 10-legislator unicameral game because the required symmetry conditions are not met. Nor does either chamber have a core. The bicameral game between the two chambers has no core because there are more than two bicameral bisectors that do not all intersect at the same point (Theorem 3). Furthermore, there is no veto-override core for the president-House veto-override game since the House's executive-veto core, P_H , does not intersect the House 4-core, $acdgia$ (horizontal shading). Nor is there a veto-override core for the president-Senate

Figure 5. Intersection of Executive-Veto Core with Bicameral 4-core



veto-override game since the Senate's executive-veto core, Pv , does not intersect the Senate 4-core, $bcd\bar{e}fhb$ (vertical shading). So if there is a core to the bicameral veto-override game here, it must be due to the *interaction* between bicameralism and the executive's veto.

And in fact, there is a core to this bicameral veto-override game. By Theorem 9, a bicameral override core exists if the bicameral 4-core intersects the bicameral executive-veto core. The bicameral executive-veto core is the $PucvP$ area. The bicameral 4-core is, in this case, simply the set union of the chamber 4-cores, producing the *acefia* pentagon, which has the heavy outline. The executive-veto core and the bicameral 4-core do intersect but only at c . Hence c is the core of this bicameral veto-override game. Note that P can be moved around substantially without eliminating c as the override core.

The modest divergence between the House and Senate, involving just H_3 and S_3 , is thus sufficient to create possibilities

for an override core that did not exist in Figure 4. If this divergence between H_3 and S_3 increases, the possibilities of a core increase. While space does not permit the presentation of further examples (see Hammond and Miller 1987, Figs. 21, 22), it appears that as additional cross-chamber pairs of legislators diverge, then the existence of a core becomes less and less sensitive to the location of the executive's ideal point. These increasing differences in legislative ideal points increase the size of both the bicameral k -core and the executive-veto core, making it increasingly likely that the two sets will intersect somewhere around the periphery of the bicameral k -core.

Note that this analysis has presumed the executive has an ideal point lying outside the bicameral k -core. If the executive's ideal point lies inside the bicameral k -core, recall that the bicameral veto-override game is guaranteed to have a core (Theorem 12).

Finally, it is important to note that unlike the simple bicameral game, large overlaps of the Pareto optimal sets for the two chambers do not necessarily diminish the likelihood of a bicameral veto-override core. It appears sufficient that the *individual legislators* are not identically matched across chambers. For this reason we feel safe in making the following conjecture:

CONJECTURE 3. *Bicameral veto-override games usually have a core.*

Summary of the Constitutional Order

Six different sets of constitutional provisions have now been examined: unicameral games (pure majority rule), bicameral games, unicameral executive-veto games, bicameral executive-veto games, unicameral veto-override games, and bicameral veto-override games. Considering the many complex relationships

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among games that have been presented, we now need to organize the results.

Define a *preference profile* as the set of preference orderings held by a group of legislators. We will say that constitution A is more stable than constitution B if two conditions hold: (1) if a given profile has a core under B, then that profile must have a core under A; and (2) there is some profile that has a core under A that does not have a core under B. Figure 6 summarizes the is-more-stable-than relationships among the six games. If the recipient of an arrow has a core for some preference profile, the originator of the arrow also has a core for that profile as well as for some other profiles. Since the is-more-stable-than relationship is transitive, we can deduce the relationship between each pair of games. For example, the bicameral executive-veto game is more stable than the bicameral veto-override game, the unicameral veto-override game, the bicameral game, and the unicameral game.

A noncomparability relationship is shown by a dotted line. Games are non-comparable in terms of stability for either of two reasons. In one kind of case, since both the bicameral and the unicameral executive-veto games are guaranteed to have cores, neither is more stable than the other. In the other kind of case, the nature of the relationship simply depends on the particular preference profile under consideration, as with unicameral veto-override games and bicameral games.

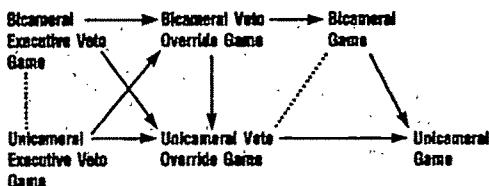
We should emphasize that the results upon which Figure 6 is based depend on our two key assumptions.

First, we have assumed that our two issue dimensions are equally important to every legislator, thereby producing circular indifference curves. The existence of executive-veto cores does not rely on an assumption about the shape of indifference curves. But the extent to which other results can be generalized when legislators and executives have noncircular indif-

Figure 6. Stability Relationships among Six Different Legislative Games

" \rightarrow " means "...is more stable than..."

"..." means "...is not necessarily more stable than..."



ference curves must be ascertained.

Second, we have presumed the existence of only two issue dimensions. Several of our results, to be sure, can be generalized to higher dimensions. But for a bicameral k -core to exist in higher dimensions requires a larger and larger value of k (see Lemma 3). Similarly, with a fixed value of k , as the number of dimensions increases, it seems reasonable to think that any bicameral k -core would get smaller and smaller, and there would be a decreasing likelihood that it would even exist. As a result, the executive-veto core (which will always exist) would be less and less likely to intersect a bicameral k -core (even assuming one does exist). This in turn means a decreasing likelihood that a veto-override core would exist.

Conclusions

We have examined the extent to which the Constitution creates a stable political order. To Madison, however, stability was not the only value the political order should have. In *Federalist* number 37, for example, he wrote:

Among the difficulties encountered by the convention, a very important one must have lain in combining the requisite stability and energy in government with the inviolable attention due to liberty and to the republican form. . . . Energy in government is essential to that security against

external and internal danger and to that prompt and salutary execution of the laws which enter into the very definition of good government. *Stability in government* is essential to national character and to the advantages annexed to it, as well as to that repose and confidence in the minds of the people, which are among the chief blessings of civil society. An irregular and mutable legislation is not more an evil in itself than it is odious to the people; and it may be pronounced with assurance that the people of this country, enlightened as they are with regard to the nature, and interested, as the great body of them are, in the effects of good government, will never be satisfied till some remedy be applied to the vicissitudes and uncertainties which characterize the State administrations (pp. 226-27, emphasis added)

From the Madisonian perspective, then, a properly designed Constitution requires not only stability-inducing features but several others as well.

We have little to say about the relationship of *stability in government* to *energy in government*, to the *republican form of government*, or to *liberty*. It is easily demonstrated, however, that any core (if it exists) to one of our games meets one test of "good" public policy, that of Pareto efficiency. A policy is Pareto efficient if there exists no other policy unanimously preferred to it. If there is a core to a bicameral game, for example, this means that for every alternative in the core there is no other policy preferred by a joint majority. And since there is no other policy preferred by a joint majority, it obviously follows that there is no other policy preferred unanimously, by all members of both chambers. From this we can conclude that points in the bicameral core are all Pareto efficient.

Given our focus on stability, one might plausibly raise the question, as have numerous critics of U.S. democracy, as to whether the constitutional system is, in some sense, too stable. Is it, for example, unresponsive to changes in the locations of the ideal points of legislators and the president?

A well-developed answer to this question requires another paper entirely. But

our results may give some insight into the problem. It appears that the location of any bicameral veto-override core is quite responsive to changes in the location of presidential ideal points. On the other hand, this override core may be relatively resistant to large-scale changes in legislative preferences. In Figure 3, for example, as long as the locations of the president and legislators H_3 and S_1 do not change, many of the other legislators' ideal points can move about in many different ways without much impact on the shape, size, or location of the override core.

This kind of pattern may have been what was intended by those who drafted the Constitution. The (apparent) lack of responsiveness of the core to changes in legislative preferences certainly seems compatible with the concern of Madison and Hamilton about the "mutability in the laws." And it may not be stretching things too much to suggest that the responsiveness of the core to changes in presidential preferences is an essential component of *energy in government*. If these inferences and interpretations prove correct, then our results can be read as a formal demonstration that the Constitution should work, at least in part, as designers like Madison and Hamilton argued it would.

Appendix

Proof of Theorem 1

THEOREM 1. *A point x is in the core if and only if no straight line through x leaves a majority of both chambers to one and the same side of that line.*

Proof. We first show that if x is in the core, then no straight line through x leaves a majority of both chambers to one and the same side of that line. It is a proof by negation, so we assume (see Fig. A-1) that there is some line RS through x that leaves a majority of both chambers to one and the same side of the line.

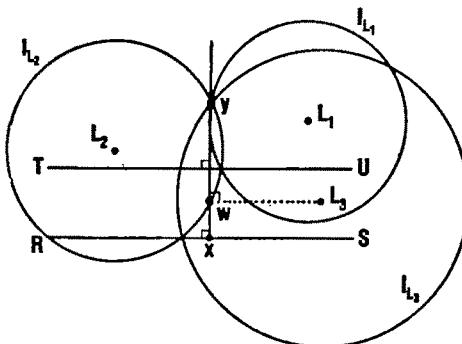
The Core of the Constitution

Consider legislator L_3 , who is in the joint majority to one side of RS and who is closest to the RS line. Now construct a point w the same distance to the same side of the RS line, on a line perpendicular to RS . Now w must be closer to every legislator in that joint majority than is x . As a result, every legislator in the joint majority prefers w to x , and x cannot be in the core, contrary to assumption. Hence, the assumption that there is a joint majority to one and the same side of the line must be wrong.

We next demonstrate that if there is an x such that no line through x has a majority of both chambers to one and the same side of that line, then x is in the core. We will assume there is some y preferred by a joint majority to x and establish a contradiction.

Consider some point y , which, by assumption, is preferred by a joint majority to some x . This means that for a majority of the House and a majority of the Senate, x lies strictly outside their indifference curves through y . Now consider the diagram in Figure A-1. Draw a straight line from x to y . Draw two perpendiculars through the xy line, one through x —producing line RS —and one through a point that bisects xy —producing line TU . Consider any legislator, L_j , in this joint majority. The only way for L_j 's indifference curve through y not to contain x is for L_j to lie above the TU perpendicular; see the indifference curves of L_1 , L_2 , and L_3 through y , for example. But if the ideal points of all legislators in this joint majority who prefer y to x (e.g., legislators like L_1 and L_2 but not L_3) lie above the TU line, then majorities of both the House and Senate necessarily also lie above the RS line through x . Hence there is a majority of both chambers whose ideal points are to one and the same side of a line through x , contrary to the assumption. Thus the assumption that x is not a core point must be wrong. QED.

Figure A-1. Establishing General Conditions for the Existence of a Core in a Bicameral Game



Proof of Theorem 2

In proving Theorem 2, two lemmas are useful:

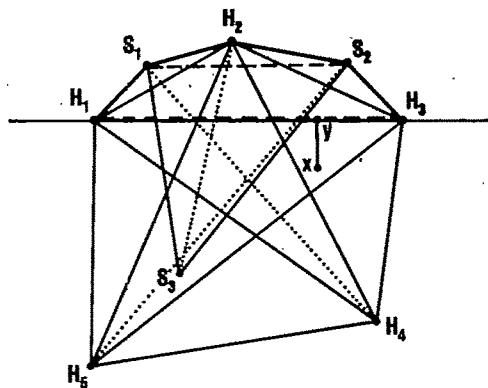
LEMMA A-1. *No point in the Pareto set of a joint majority of minimal size can be upset by some other point in this Pareto set due to the votes of the members of this joint majority.*

This lemma follows directly from the definition of the Pareto set of a group of actors, which is the set of alternatives from which they cannot agree to move.

LEMMA A-2. *Given some joint majority of minimal size, at least one of the contract curves connecting the ideal points of a given chamber's members in the joint majority will be a chamber bisector.*

Proof. Draw the Pareto set of just the House members in the joint majority. Consider some point x outside this House Pareto set but inside the Pareto set of the entire House; see point x in Figure A-2, for example. This point is outside of the Pareto set for coalition H_1 , H_2 , and H_3 . Establish some y on the nearest House

Figure A-2. At Least One Chamber-Member/Chamber-Member Contract Curve Will Be a Chamber Bisector



contract curve in the joint majority's Pareto set by dropping a perpendicular to the nearest House contract curve. Since by assumption the joint majority is of minimal size, only a bare majority of the House will prefer y on or in this House Pareto set to the x lying outside this House Pareto set. If this is not true for at least one House contract curve in this Pareto set, either (1) more than a bare House majority prefers y to x , which violates the assumption that the joint major-

ity is of minimal size, or (2) less than a bare House majority prefers y to x , which violates the assumption that we are considering a joint majority. Since a bare House majority lies on or to one side of the House contract curve, a bare House majority must lie on or to the other side. Hence this House contract curve is a House chamber bisector. The same logic holds for the Senate contract curves in the minimal-size joint majority. QED

We can now prove

THEOREM 2. *If (1) a bicameral legislature has only one bicameral bisector and (2) there is a point x on the bicameral bisector such that the chamber bisectors from one and only one chamber intersect the bicameral bisector in each direction from x , then x is a core point.*

Proof. By negation: we assume Conditions (1) and (2), but assume that some point y beats x .

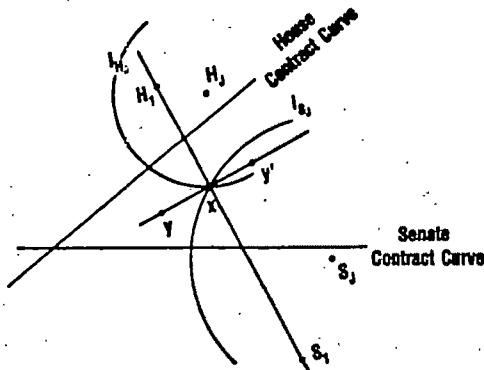
The House and Senate members at either end of (and defining) the bicameral bisector will be called H_1 and S_1 (see Figure A-3). Examining the joint coalition that prefers y to x , there are three mutually exclusive and collectively exhaustive cases to consider, each of which leads to a contradiction.

Case 1. Both H_1 and S_1 are members of the joint coalition preferring y to x .

The first combination is easily ruled out as a minimally sized joint majority that can upset x . Since the interests of H_1 and S_1 are diametrically opposed along H_1S_1 , no minimal joint majority that includes both H_1 and S_1 can prefer some y to x . Some y off the H_1S_1 bicameral bisector would be rejected by both H_1 and S_1 in favor of some point on the bicameral bisector, while movement along the bicameral bisector necessarily leaves one or both of them worse off. So the joint majority preferring y to x will necessarily lack either H_1 or S_1 or both.

Case 2. Neither H_1 nor S_1 are members

Figure A-3. Establishing the Existence and Location of a Core in a Bicameral Game



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of the joint coalition preferring y to x .

We next prove that no minimally sized joint majority lacking both H_1 and S_1 can exist that prefers some y to x . Draw a perpendicular to H_1S_1 through x . We will show that the indifference curves through x of a Senate majority cannot all cross the perpendicular from below and that the indifference curves through x of a House majority cannot all cross the perpendicular from above. This will mean that the $H^{-1}(x)$ win set of a House majority cannot possibly intersect the $S^{-1}(x)$ win set of a Senate majority. Recall that H_1S_1 is the only bicameral bisector. Hence, to the right of H_1S_1 there is, by definition, no more than one less than a majority of the Senate, while to the left of H_1S_1 there is no more than one less than a majority of the Senate. Since we have by assumption ruled S_1 (as well as H_1) out of the joint majority upsetting x in favor of y , the Senate members of the joint majority must come from both the right and the left of H_1S_1 ; no other way of mustering a Senate majority is possible. However, no indifference curve through x of any Senator to the right of H_1S_1 can possibly cross the perpendicular on the left of H_1S_1 (see the indifference curve of S_1 , labeled I_{S_1} , for example). Similarly, no indifference curve of any House member to the right of H_1S_1 can possibly cross the perpendicular on the left of H_1S_1 (see the indifference curve of H_1 — I_{H_1} —for example). Hence the $H^{-1}(x)$ and $S^{-1}(x)$ win sets cannot intersect to the left of H_1S_1 . As a result, there can exist no joint majority that prefers some y to the left of H_1S_1 . An identical argument demonstrates that there can exist no joint majority that upsets x in favor of some y' to the right of H_1S_1 . Therefore, no joint majority including neither H_1 nor S_1 will prefer some y to the x .

Case 3. Either H_1 or S_1 are members of the joint coalition preferring y to x , but not both.

Assume without loss of generality that

it includes only H_1 . Hence a joint majority including H_1 that upsets x in favor of some y must include a majority of Senate members not involving S_1 . Since there is no Senate majority that lies strictly on one side of the bicameral bisector, the joint majority including H_1 must include Senate members from both sides of the bicameral bisector.

But the Pareto set of the minimal-size joint majority just described—containing H_1 and senators (but not S_1) on both sides of the bicameral bisector—will encompass space on both sides of the bicameral bisector. Now consider the Pareto set of just the Senate members of this minimal-size joint majority. The boundaries of the Senate-majority Pareto set will cross the bicameral bisector at two places, at the top of the set (toward the H_1 end) and at the bottom of the set (toward the S_1 end). By Lemma A-2, one of these lines crossing the bicameral bisector will necessarily be a Senate chamber bisector. If it is not a Senate chamber bisector, either (1) the number of senators above the line is less than a majority, in which case x is not in fact upset by the joint majority—contrary to the assumption that x is upset, or else (2) the number of senators above the line is more than a bare majority, in which case we have more than a minimal Senate majority contributing to the upsetting of x —contrary to the assumption that we are dealing with a minimal-size joint majority.

Now, since we have assumed there is no bicameral core, point x must lie outside the Pareto set of the minimal-size joint majority (Lemma A-1), which necessarily means that it lies below the Senate chamber bisector just described, which crosses the bicameral bisector. But this means that a Senate chamber bisector crosses the bicameral bisector on the same side as the House majority. This in turn means that there are both House and Senate chamber bisectors intersecting the bicameral bisector on the same side of x . This violates Condition 2. Again, in Case 3, we arrive

at a contradiction.

Since a contradiction is reached in all possible cases, it cannot be the case that x is not a core point. QED

Note

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TOCQUEVILLE'S CONSTITUTIONALISM

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For the ancient philosophers, constitutionalism meant classifying regimes and constructing regimes to form virtuous citizens. In the modern world it generally means checks and balances, institutional mechanisms limiting the power of government and protecting private rights. In *Democracy in America* Tocqueville attempts to combine both views in his interpretation of the U.S. constitutional system. He employs the regime analysis of ancient constitutionalism to understand the new phenomenon of popular sovereignty and its potential for despotic control over the minds and characters of citizens. At the same time, he shows how the constitutional devices found in the United States—such as federalism, judicial review, and the separation of powers—can be adapted to inculcate a kind of moral virtue by teaching citizens to exercise liberty with moral responsibility and to govern themselves. The result is a constitutional theory that weaves ancient and modern principles into an original and coherent whole.

Most U.S. citizens and legal scholars think of constitutional government as a system of legal and institutional restraints on power that protect private rights. To them, *constitutionalism* consists chiefly in knowing the number and kinds of rights that deserve constitutional protection or in examining the institutional mechanisms that limit and balance the powers of government. This commonly accepted view, however, is not the only conception of the term.

A different view is presented by Alexis de Tocqueville in *Democracy in America* (1945). As I shall argue, Tocqueville conceives of constitutionalism as more than a device for limiting power and protecting rights. Appealing to an older tradition, he argues that laws and institutions must be examined in light of the regime or whole social order underlying them. Such an analysis reveals that the modern democratic revolution has created a new kind of despotic control known as "popular

sovereignty," which threatens to destroy the intellectual and moral character of citizens and make them unfit for liberty. Accordingly, constitutional government must go beyond the mere concern with constraining power through institutional mechanisms and take a deliberate interest in the moral education of citizens. This insight, I hope to show, leads Tocqueville to an interpretation of the U.S. Constitution that combines ancient and modern principles in an original theory.

Definitions of Constitutionalism

In the voluminous literature on constitutional government, a great deal of effort is devoted to determining what is meant by *constitutionalism*. The ambiguity of the term is such that most scholars acknowledge a variety of meanings, including some that seem to be mutually contradictory. For the purpose of under-

standing Tocqueville's theory, we shall examine three of the most important definitions and then indicate how Tocqueville's conception may be distinguished from them.

The most general definition can be found in the works of such eminent legal scholars as McIlwain and Corwin. They define *constitutionalism*, in the broadest sense, as "the legal limitation on government . . . [whose] antithesis is arbitrary rule" (McIlwain 1947, 21) or "the belief in a law superior to the will of human governors" (Corwin [1928], 1971, 5). The emphasis here is on the limitation of power by law, specifically by a law that stands above the will of the rulers or the ordinary statute law. Such a fundamental law might be a written code or document; but more commonly it is an unwritten "higher law" that could be discovered, articulated, and enforced by a variety of authorities. Hence, both McIlwain and Corwin include a large number of higher-law theories in this category—classical natural right, Roman law, Christian natural law, English common law, modern natural rights, and the U.S. Constitution. So sweeping is this notion that Corwin claims to unfold an unbroken tradition of such theories from Aristotle and Cicero to John of Salisbury to Sir Edward Coke to Locke and Madison (Corwin [1928], 1971, 9-89; see also Wormuth 1949). This definition is obviously the most inclusive; for it treats every type of higher law as a kind of constitutionalism and views the differences among types of higher law as less significant than their status as norms or rules above the human will.

Though useful as a beginning point, this definition does not do justice to the variety of phenomena claiming the title *constitutionalism*. Hence, many scholars distinguish between *ancient* and *modern* constitutionalism, acknowledging thereby that the ancient Greeks and Romans meant by *constitutionalism* something

quite different from what we think of today. McIlwain, for example, points out that the word often translated as "constitution" in the writings of Plato and Aristotle is *politeia*, a term referring to the entire social order of a city or nation as well as the form of government, "its whole economic and social texture as well as matters governmental in our narrow modern sense" (McIlwain 1947, 24; Wheare 1966, 1). In a recent article that attempts to resurrect the ancient meaning of *constitution*, Maddox refers to Cicero's use of *constitutio*—the Latin noun from which the English *constitution* is derived. It means "the 'shape,' 'composition,' or 'establishment' of a people in their political association" (Maddox 1982, 807). In both cases, the words emphasize the ordering of the whole association, the way its various parts are organized into an identifiable form or structure.

One might add to these observations that the classical philosophers and historians trace the form of a political association to the relation between ruler and ruled, specifically to the way the ruling group or dominant class imposes its ideas of what is just and honorable on the ruled and molds the characters and tastes of citizens. Perhaps the best translation of *politeia*, therefore, is not "constitution" but "polity" or "regime": the whole political and social order as it is shaped by the ruling group and its vision of the good life. Using the notion of a *regime*, classical political science seeks to identify the common patterns and to classify them according to the number and types of people who make up the ruling body—a *monarchy* is a regime ruled by one preeminent individual or family; an *oligarchy* is rule by the wealthy few; a *democracy* is rule by the common people, and so on.

The purpose of such a classification, of course, is not merely to describe structures of power but to guide the legislator in choosing the regime which is best or the best possible in the given circumstances.

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Guided by a "higher-law" standard of natural right that prescribes *virtue* as man's natural end, ancient constitutionalism turns the classification of regimes into an exercise in moral choice, a problem of choosing the regime best suited to make men virtuous—to make them courageous, moderate, just, and prudent. For most of the classical philosophers and historians, this means choosing an aristocracy of wise and virtuous gentlemen in the ideal circumstances, a mixed or middle-class regime in less ideal circumstances, and a democracy tempered by rule of law and public spiritedness as a realistic third choice (Aristotle, *Politics* 3, 4, 7; Polybius, *Histories* 6). In speaking of ancient constitutionalism, then, one is referring to *regime analysis*: the classification of regimes and the prudential choice of which regime or combination of regimes is best suited to mold virtuous citizens.

Neither the general definition nor the ancient view, however, adequately captures the way *constitutionalism* is used today. The typical U.S. citizen, as well as the majority of contemporary legal scholars, use the terms *constitution* and *constitutional government* to mean something more precise than a higher law and more limited than the regime as a whole. They mean institutional restraints on power that protect private rights—such as the separation of powers, electoral accountability, or systems of checks and balances. According to McIlwain, this is the distinctively modern understanding of constitutionalism (McIlwain 1947, 115, 142). Another distinguished legal scholar, Sartori, calls this meaning of constitutionalism the *garantiste* meaning (emphasizing the guarantees for individual liberty) and insists that it is the only correct understanding of the word (Sartori 1962, 863; also, Friedrich 1946, 4, 21; Wheare 1951, 10-11; Wörnuth 1949, 3). One should add that this definition expresses the view of James Madison and the other

authors of the *Federalist Papers*. They viewed the U.S. Constitution as a system of checks and balances that limits the powers of the federal government in order to secure the rights of persons and property (*Federalist* nos. 9-10; Madison 1981, 36, 62). According to the modern conception, constitutionalism is grounded in the "higher-law" doctrine of natural or human rights and focuses not on the way regimes mold the characters of citizens but on the institutional mechanisms for limiting, dividing, and balancing the powers of government.

Ancient Constitutionalism and the Modern Phenomenon of Sovereignty

In turning to Tocqueville's analysis of the U.S. Constitution in *Democracy in America*, one senses that it does not fit neatly into any of the previously mentioned definitions of constitutionalism. At first glance, the conceptual framework of the whole of *Democracy in America* seems to follow the lines of ancient constitutionalism. It is a comparison and evaluation of two regimes: democracy and aristocracy (or its European variant, feudalism). Yet, as one scholar has noted, the terms Tocqueville employs to describe the regimes are sometimes confusing because they reflect both "political" and "social" categories (Richter 1970, 90; Richter 1969, 156-59). Thus, Tocqueville defines *democracy* as the rule of the people or popular sovereignty; but he traces the ultimate foundation of democracy to "equality of social condition," which he understands in socioeconomic terms as the absence of hereditary classes or fixed social hierarchies (vol. 1, Chaps. 3, 4). This leads some commentators to the conclusion that Tocqueville is one of the founders of modern sociology (Aron 1968, 237). A more accurate view, however, is that Tocqueville uses some of the

language and categories of modern sociology but does so in the service of an older conception of political science. His emphasis on the role of social classes and their characteristic patterns of behavior is actually an emphasis on the *regime* in the classical sense (Zetterbaum 1967, 52). For the central theme of the book is that all aspects of life in democratic America are shaped by the ruling group, the sovereign people, and by belief in equality, which they establish as just and authoritative. Our first impression of Tocqueville, then, is generally correct: his enterprise is not sociology but the regime analysis of classical political science.

Yet, one cannot deny that Tocqueville continually expresses himself in language that gives primacy to social and cultural phenomena over those that are narrowly political. This tendency is particularly evident in his statements about the organization of *Democracy in America* and his method of analysis. Tocqueville states that his work is divided into two parts: the subject of the first part is the *laws* of the United States; the subject of the second is the *customs* (1:15). A glance at the table of contents of Part 1 indicates that by the *laws* he means more than the legislative acts of the U.S. government; he means the entire constitutional system—the federal and state constitutions, the formal and informal institutions of government, and the conceptions of justice underlying the laws and institutions. Although Tocqueville uses the term *laws* in a comprehensive sense to mean the constitutional system and devotes the first part (18 chapters) to explaining its operation, he clearly states that such matters are only one part of the whole phenomenon of democracy in the United States. He asserts that three causes or factors must be considered in order to understand the democratic phenomenon in America: the accidental circumstances (geography, climate, and historical contingencies), the laws (constitutions and

institutions), and customs (mores or habits, sentiments, and beliefs). Of these three causes, the accidental circumstances are the least important, the laws are of secondary importance, and the customs are most important (vol. 1, Chap. 17). The implication is that laws are subordinate to customs, that political phenomena are shaped primarily by social and cultural phenomena. One might conclude, then, that Tocqueville employs the regime analysis of classical political science but modifies it by making social and cultural factors more important than narrowly political ones in the formation of regimes (Ceaser 1985, 656).

Despite the modern terminology, this approach is not in itself a departure from classical regime analysis because the classical conception of the regime rests on a very broad notion of the political—one that includes the role of social classes and their characteristic customs, mores, culture, or ethos (1:310). Tocqueville departs from the classical approach not in giving primacy to customs but in separating laws from customs. By creating a distinction between the two phenomena, he does something classical political scientists would never do, namely, separate the legal or constitutional system from the regime or whole social order. As a result, Tocqueville makes statements that would sound strange to classical ears: "The political Constitution of the United States appears to me to be one of the forms of government that a democratic people could adopt; but I do not regard the American Constitution as the best, or as the only one, that a democratic people may establish" (1:246). What is unusual about this statement is that Tocqueville equates the constitution with the form of government and distinguishes it from the regime; he then asserts that a democratic regime or democratic people could adopt any one of several forms of government and still remain a democracy. It could create a "democratic republic," as he says

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the American people have done, or it could create a monarchical constitution, as he prefers for France (Tocqueville 1970, 200; Tocqueville 1985, 113-14).

Tocqueville's distinction between law and custom, between constitution and regime, is a significant departure from classical political science because it separates the political realm from the larger social ethos and relegates the political to a matter of secondary importance. Instead of the form of government shaping the social ethos, as the classical philosophers maintain, the social ethos shapes (although it does not completely determine) the form of government. If we search for the reason behind this departure from ancient constitutionalism, it may be found in the modern notion of *sovereignty*. What most impresses Tocqueville about modern democracy is that it resembles the regimes of the past in certain respects but ultimately is based on a new and different kind of authority: it is not merely a regime but a condition of sovereignty. The difference between a regime and a condition of sovereignty, as we shall see, lies in the degree and manner of control exercised by the ruling body.

Tocqueville explains this difference in Chapter four of *Democracy in America*, entitled, "The Principle of the Sovereignty of the People in America." There he asserts that the principle of popular sovereignty actually exists at the bottom of all human institutions, including despotic governments; for the support or acquiescence of the people is required of all established institutions. In the past, however, the sovereignty of the people was concealed or was acknowledged only in empty phrases such as the *will of the nation* or the *commonweal*. By contrast, Tocqueville claims, in modern democracies like the United States "the principle of the sovereignty of the people is neither barren nor concealed, as it is in other nations." This changes the nature of political authority: "In some countries a power ex-

ists which, though it is in a degree foreign to the social body, directs it, and forces it to pursue a certain track. . . . But nothing of the kind is to be seen in the United States; there society governs itself for itself. All power centers in its bosom, and scarcely an individual is to be met with who would venture to conceive, or, still less, to express the idea of seeking it elsewhere" (1:59). In traditional forms of political authority, the ruling group (such as an aristocracy or a clergy) stands outside and above the people, shaping and forming the people to its ends; the state molds society, creating a regime. In modern democratic societies, by contrast, the ruling group does not shape the people, the state does not mold society, but "society governs itself for itself." This kind of authority differs from earlier kinds because it is not based on a distinction between a ruling part and a ruled part; the two parts are identical. No need exists for one part to mold and shape the other (as in a regime) because there is no challenge or resistance. The state does not mold society but is absorbed by society and becomes subordinate to society. This is a condition of sovereignty because there exists an unchallenged, indisputable, and all-absorbing power that dominates without ruling because there is no conceivable alternative to its power.

Obviously, it is from this notion of sovereignty that Tocqueville draws his dire warnings about "the unlimited power of the majority in the United States" (vol. 1, Chap. 15). In expressing his fears about the tyrannical power of the majority, Tocqueville goes well beyond the criticisms of democracy leveled by the classical philosophers or by democratic theoreticians in modern times, like James Madison. Their warnings were focused on the political dangers of majority rule, such as the dispossession of the rich, the ostracism of the great, or the suppression of minority rights (Aristotle, *Politics* 3; *Federalist* no. 10). For Tocqueville (and,

following him, J. S. Mill), the tyranny of the majority is not only a political phenomenon but also an intellectual and moral phenomenon—an enslavement of the mind and a weakening of the will arising from the inability to conceive of any sort of authority except that of the people, or any standard of legitimacy except that of majority opinion. Even the harshest critic of democracy in the ancient world, Plato in the eighth book of *The Republic*, does not single out democracy for being more intellectually and morally despotic than other regimes; he contends that all regimes are restrictive and exclusive in the way they impose their notions of justice and honor on society. In fact, he sees more diversity and openness in democracy (which even tolerates the philosopher, up to a point) than in other regimes. What Tocqueville describes as the sovereignty of the people, the empire of the people, the irresistible pressure of popular taste and culture, is something that is not fully captured by the classical notion of the *regime*. With a sense of profound awe before a newly discovered mystery, Tocqueville says, "When I compare the Greek and Roman republics with these American states . . . When I remember all the attempts that are made to judge the modern republics by the aid of those of antiquity, and to infer what will happen in our time from what took place two thousand years ago, I am tempted to burn my books in order to apply none but novel ideas to so novel a condition of society" (1:327). With a sense of forboding, he also says, "I think, then, that the species of oppression by which democratic nations are menaced is unlike anything that ever existed before in the world" (2:336). The novel condition that Tocqueville witnesses and seeks to understand is the modern phenomenon of democratic sovereignty.

If we search for Tocqueville's explanation of the origin and cause of this new phenomenon, we cannot find it in his

discussion of popular sovereignty or majority tyranny in volume 1 of *Democracy in America*; for the first volume focuses on the laws, the legal or constitutional system as it is shaped by the principle of popular sovereignty. To understand not only the effects but also the cause of popular sovereignty, we must turn to volume 2, where the customs, or mores, of the people are analyzed; for the phenomenon of sovereignty is caused by a way of thinking, a way of conceiving of authority. From his discussion of democratic mores, we learn that attitudes toward authority in the United States are shaped by a mode of thinking and feeling that Tocqueville calls *individualism*. He argues that individualism is largely a product of the modern Enlightenment (as well as the Protestant Reformation), which taught modern men to question all authorities and to look for truth only in their own independent thinking—a mental habit, in other words, of skepticism and individual self-reliance (2:4-5). The effect of this mental habit is a startling and profound paradox in the way that modern democratic men conceive of authority; for it does not liberate the human mind in the way that the Enlightenment thinkers originally hoped and expected. According to Tocqueville, when most men are called upon to free themselves from higher authorities, they become frightened and confused and feel the insignificance of their own thoughts; instead of thinking for themselves, they turn to public opinion or the voice of the majority for guidance (2:11). The result is a paradoxical combination of individualism and conformity—an insistence on the equal right to think for one's self combined with a silent surrender of the mind and bending of the will to a new authority, the sovereign people.

To a certain extent, Tocqueville admits, this is only natural: "men will never cease to entertain some opinions on trust" without questioning or examining them;

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radical and perpetual doubt about first principles leads either to madness or chaos; dogmatic beliefs of some kind are necessary for life. But the new conformity underlying democratic individualism is actually more slavish and dogmatic than, for example, the religious authority of the past precisely because it appears on the surface to be intellectual freedom and is rarely acknowledged as a species of intellectual dogmatism. Its disguises make it difficult to recognize, for it imposes itself not by active assertion but by passivity, complacency, and casual acceptance—by the relaxation of the mind and will that occurs when the demands of higher authority are questioned. Tocqueville suggests that even those "minorities" who might be suspected of disloyalty to democracy, such as the wealthy, the clergy, and the literary or intellectual elite, have difficulty resisting the pressures of popular taste and culture (1:54, 312-20; 2:36-64). For the principle of popular sovereignty, or will of the majority or voice of "the public," triumphs without argument "through a sort of enormous pressure of the mind of all upon the individual intelligence" (2:9-12).

In short, because modern democracy is founded not merely on the rule of the people but on the popular mind as it is filtered through the modern Enlightenment, the whole conception of authority is revolutionized. The sovereignty of the people comes into being, characterized by free-thinking individualism and an uncontested surrender to the will of the people. The result, according to Tocqueville, is a despotism of the mind and weakening of the character such as the world has never seen before, raising the specter of a new kind of democratic tyranny.

Following this analysis, we can say that Tocqueville's *constitutionalism* is a combination of old ideas and new categories adapted to fit the phenomenon of modern democracy (Richter 1969, 159). When Tocqueville observes the political and

social developments of his day, he thinks broadly in terms of regimes, primarily the contrast between aristocracy and democracy or between "the old regime" (to use Tocqueville's phrase for European feudalism) and the new democratic order. But, the regime analysis of ancient constitutionalism cannot fully grasp the new democratic order because the concept of the *regime* does not adequately describe the kind of domination and potential for despotism of modern *sovereignty*. Moreover, the classical concept of the regime does not account for the way that modern societies distinguish between the sovereign and the government, the customs and the laws, the society and the state, or the sovereign people and the *constitution* (in the narrow and limited sense in which the term henceforth is used). As Tocqueville says in the introduction to his work, "a new science of politics is needed for a new world," and his adaption of classical regime analysis to fit the phenomena of democratic sovereignty and the limited constitution is the centerpiece of the new science.

Adapting the Regime of Virtue to Democratic Sovereignty

In presenting Tocqueville's modification of ancient constitutionalism, we have concentrated almost exclusively on its descriptive features, on its use as an analytical tool to describe the nature of power and authority. Yet, ancient constitutionalism is also, indeed it is primarily, a normative theory. It is essentially an exercise in political prudence. The classification of regimes and the ideal of a regime of virtue are intended to be general guides for the legislator, whose task is to choose the best possible political order in the given circumstances, the regime most conducive to the formation of virtuous citizens under the existing conditions.

In *Democracy in America*, Tocqueville

adopts the normative orientation of ancient constitutionalism. He accepts the traditional view that politics should be directed to the formation of virtuous characters through moral and civic education. But he recognizes that new techniques or a modification of old ones are necessary to achieve this goal, for it is no longer possible to create a regime of virtue by educating a ruling class and having it impose its morals from the top down, so to speak, through laws and institutions. Modern democracy prohibits such an "authoritarian" imposition of morals; it prohibits a regime of virtue in the strict sense because it rejects in principle the notion of higher authorities or the rule of moral superiors. In place of a regime imposed from the top, modern democracy creates a sovereign, the people, who in turn create a constitution, their state and form of government, whose purpose is to serve their needs and protect their rights. In these circumstances, the inculcation of virtue must follow a different strategy, one that is indirect and even hidden from view. Most importantly, as we shall see, it must speak the language of democracy, which means it must appear not as a form of virtue or moral superiority but as a form of liberty.

In one of his most explicit statements on first principles in *Democracy in America*, Tocqueville proclaims the highest ethical principle to be virtue and asserts that virtue should be the standard and guide for politics: "After the general idea of virtue, I know of no higher principle than that of right; or rather these two ideas are united in one. The idea of right is simply that of virtue introduced into the political world. It was the idea of right that enabled men to be independent without arrogance and to obey without servility.... There are no great men without virtue; and there are no great nations—it may almost be added, there would be no society—with out respect for right" (1:254). Without actually defining *virtue* in this passage,

Tocqueville asserts that it is the highest ethical principle because it is the source of human greatness for individuals and nations. Virtue is the foundation of individual character, indeed, it is the perfection of human character. In the political world, it is the foundation of political right or notions of legitimate authority that enable men to obey authority freely, through voluntary self-restraint, without being tyrannical or servile. The type of virtue that Tocqueville has in mind here (and that he praises repeatedly in all his writings) appears to be a synthesis of civic and heroic virtue. Following Montesquieu and Rousseau, he defines *civic virtue* as republican virtue: it is patriotism or love of country over love of self, a sentiment that Tocqueville often associates with self-assertion on the one hand and mastery of the selfish passions on the other (1:97; Tocqueville 1955, 17; Schleifer 1980, 242-43). By *heroic virtue*, Tocqueville usually means great feats of courage and daring in war as well as chivalry and great acts of principled statesmanship—ideals he instinctively absorbed from his aristocratic heritage, from his reading of Plutarch, and perhaps from contemporary romantic notions of the Napoleonic hero (2:242-55; Tocqueville 1955, 118-20; Tocqueville 1985, 124-25; Boesche 1981). Whatever his sources of inspiration, Tocqueville holds civic and heroic virtue to be the highest ethical ideal because it produces noble characters, characters whose proud self-assertion and voluntary self-restraint make them true citizens and statesmen.

While appealing to the ancient ideal of virtue, Tocqueville recognizes the need to adapt it to the conditions of modern democracy. For democracy produces many forces that tend to undermine the assertive pride and self-restraint of a virtuous character. We have already seen how the pressure of popular sovereignty wears down the intellectual independence and strength of will of those who claim

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moral superiority to the majority. In addition, Tocqueville argues that democracy unleashes certain feelings or passions—an ethos of individualism and materialism—that endanger virtue. He sees *individualism* not only as a mental habit of questioning traditional authorities but also as a passion that results from the weakening of traditional social bonds; it is a peculiar kind of selfishness that combines egoism with a tendency to isolate one's self from others (2:102). *Materialism* is the love of physical well-being, especially the love of comforts and conveniences that make life softer and easier, a passion most characteristic of the middle classes (2:137). In the democratic age, these two passions are usually joined in an ethic of self-improvement, which drives individuals in a competitive and energetic quest for wealth and success (2:165). Tocqueville's view is that such passions are especially harmful to the moral character of citizens. For they tend to encourage withdrawal from public life by channeling all energy into business and commercial activities; ultimately they may destroy every form of individual initiative and restraint, turning the masses into passive sheep (2:149, 335-37).

Throughout his analysis of individualism and materialism, Tocqueville states explicitly that such tendencies are inimical to virtue: "Selfishness blights the germ of all virtue; individualism, at first, only saps the virtues of public life; but in the long run it attacks and destroys all others. . . . Selfishness is a vice as old as the world, which does not belong to one form of society more than to another; individualism is of democratic origin" (2:104). In a more sweeping statement about the effects of individualism and the love of material well-being, Tocqueville says that "the age of implicit self-sacrifice and instinctive virtues is already flitting far away from us" (2:132). Such statements make abundantly clear that the demise of virtue is Tocqueville's primary concern in

judging the moral character of modern democracy. In one of his notebooks, he states succinctly, "Egoism has replaced virtue"—that is the moral problem of democracy (Schleifer 1980, 241).

While making these judgments, Tocqueville recognizes that traditional civic and heroic virtues can never be resurrected in their pure and original forms. The task of the legislator, therefore, is to find substitutes for virtue compatible with democratic habits and beliefs. In surveying the U.S. scene, Tocqueville sees the greatest hope in the American understanding and practice of liberty. In analyzing this phenomenon, he presents an original and insightful interpretation that shows that American liberty assumes two distinct forms, political liberty and individual rights, and that both kinds of liberty can serve as democratic substitutes for virtue.

Tocqueville's account of political liberty appears at the beginning of *Democracy in America* and is boldly proclaimed as "the germ of all that is to follow and the key to almost the whole work" (1:29). He explains how the tradition of political liberty began in America with the settling of New England by the Puritans. In Tocqueville's eyes, the Puritans were remarkable because they combined strict religious beliefs—which prohibited everything from idleness and drunkenness to kissing in public and long hair—and political freedom. Their laws reflected the severity of Puritan morality; yet, "it must not be forgotten," Tocqueville says, "that these fantastic and oppressive laws were not imposed by authority, but that they were freely voted on by all persons interested in them" (1:40-41). By living according to strict laws that were voluntarily imposed, the Puritans were able to unite two things that other societies have found incompatible: the spirit of religion and the spirit of liberty. Through this unusual synthesis, the Puritans created the ideal of political liberty that permeates the whole of U.S.

society. In New England, it was embodied in townships and town meetings, where citizens participated directly in law making and administered local affairs. Spreading in all directions from its original source, the ideal of political liberty has "interpenetrated the whole American confederation and now constitutes the social theory of the United States" because it is the purest or most direct form of democratic self-government (1:32).

In tracing the origins of political liberty to the Puritan tradition of self-government, Tocqueville also uncovers its moral foundations. It rests, he says, on "a fine definition of liberty," a definition best expressed in a speech by Jonathan Winthrop, the early governor of Massachusetts. The gist of Winthrop's speech (as quoted by Tocqueville) is a distinction between two types of liberty. The first is *natural liberty*, which is the freedom to do what one wants and is no different from animal freedom. The second is *civil or moral liberty*, which is the freedom to do only what is good, just, and honest; it is a type of freedom that arises out of a covenant between humanity and God and must be exercised "in subjection to authority." According to Tocqueville, moral liberty is higher and more noble than natural liberty because it involves self-restraint, or mastery of the passions, and is bounded by moral and intellectual limits that prevent it from degenerating into license or chaos. Such liberty requires religion or some sense of sacred awe "before truths which it accepts without discussion" in order to provide a sense of limit and restraint (1:44-45). Such liberty, in turn, is the basis of democratic self-government; for it gives people the discipline and self-restraint to live by self-imposed laws, as well as the respect for authority and sense of civic duty to participate in public life. Thus, the moral liberty of the stern and pious Puritans is the foundation of political liberty or self-government in the United States.

The significance of this account is in providing a conception of liberty that closely resembles the classical notion of civic or republican virtue. The description of moral liberty provided by Jonathan Winthrop and warmly embraced by Tocqueville is closely akin to virtue because it is based on a hierarchy of the soul in which the higher or divine part rules over the lower or animal part in conformity with a higher law. It is also like virtue in that it requires people to subordinate their private interest to the public good. Moral liberty is different from classical republican virtue, however, because of its Puritan foundations. It arises from the Protestant idea of Christian liberty, which requires people to obey divine law from a free and willing choice rather than from sentiment, habituation, or the coercion of external authority. Moral liberty is self-willed obligation or obedience to a self-imposed law. Thus, it is properly classified as a species of freedom rather than of virtue, although the two are very similar.

The great question for Tocqueville is what becomes of moral liberty and the democratic self-government it sustains once the Puritan tradition grows weak and dies out. When this occurs, moral liberty is no longer a free and voluntary obedience to divine law but obedience to merely human law or perhaps to no law. Moral liberty then loses the inner discipline which distinguishes it from natural liberty, and democracy is left without restraints. In a sense, one could read volume 1 of *Democracy in America* as a moral melodrama, a kind of *Paradise Lost* in which democracy falls from the grace of the original Puritans to the despotic excesses of the tyrannical majority. As the story unfolds, about three-fourths of the way through volume 1, Tocqueville reveals that the old religious faith that once nurtured self-government in the United States lost its power and must be replaced by a new (but far less noble) moral principle: "Do you not see that

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religious belief is shaken and the divine notion of right is declining, that morality is debased and the notion of moral right is therefore fading away? Argument is substituted for faith, and calculation for the impulses of sentiment. If, in the midst of this general disruption, you do not succeed in connecting the notion of right with that of private interest, which is the only immutable point in the human heart, what means will you have of governing the world except by fear?" (1:255). Striking a note of pessimism, Tocqueville reveals the fate of the Puritan ideal. The old faith has lost its power because the advancing tide of modern skepticism has weakened the notion of divine right and replaced it with enlightened self-interest. In this age of skepticism, the only secure basis for political right is self-interest or private interest. A doctrine that builds upon self-interest and is capable of turning it into a moral principle—a principle of right that makes obedience to authority a matter of voluntary compliance rather than of coercion and fear—must be found.

At this point, Tocqueville turns, in a spirit of prudent compromise, to the doctrine of individual rights: "I am persuaded that the only means which we possess at the present time of inculcating the idea of right and of rendering it, as it were, palpable to the senses, is to endow all with the peaceful exercise of certain rights." The most important ones are property rights. For an individual may have "no notion of the property of others; but as he gradually learns the value of things and begins to perceive that he may in his turn be despoiled, he becomes more circumspect, and he ends by respecting those rights in others which he wishes to have respected in himself" (1:254). Tocqueville's claim is that in an age when divine right and traditional virtues are declining, the only effective moral principle is the doctrine of individual rights, especially property rights. This defense of

rights, of course, is purely prudential. For rights find powerful support in human selfishness and thus can be seen as nurturing moral vice or social instability. Yet, when properly understood as things that must be respected in others in order to be enjoyed by oneself, they turn self-interest into a principle of right. Through self-interest, rights lead people to self-restraint, respect for fellow citizens, and respect for authority. Rights thereby become another substitute for virtue, although a less satisfactory substitute than political or moral liberty.

The difficulty that remains for Tocqueville is to find a common moral basis for these distinct notions of liberty; for the two are not entirely compatible. Political liberty is a moral or theological conception derived from the Puritan tradition. It requires people to govern themselves in accordance with divine law; it frees them from the enslavement to their base and selfish passions by making them accountable to a higher law and teaches citizens to subordinate their private selves to the public good. While Tocqueville calls this a "fine," or noble, definition of liberty, he recognizes that its religious foundation will probably not survive in the enlightened and secular age of democracy (although he feels every effort must be made to perpetuate it as long as possible in the customs of Christian America [Kessler 1977]). By contrast, the second notion of liberty, individual rights, is perfectly at home in the enlightened and secular age but lacks a notion of moral restraint or accountability to a higher law; it encourages private selfishness and promotes restraint and respect for authority only through enlightened self-interest. Its foundation is more powerful and lasting but less noble.

To resolve this dilemma, Tocqueville looks to a phenomenon that is already present in American customs and political life, although imperfectly perceived and understood. That phenomenon is a sense

of moral responsibility—the belief that human freedom involves moral obligations because human beings are masters of their fate and must have the willingness and capacity to govern themselves. This notion of moral responsibility, Tocqueville suggests, is a secular counterpart or secular legacy of the old moral liberty of the Puritans (1:67, 94-96). Like the Puritan ideal, it requires self-government in the political and personal sense. It requires people to determine their destiny by participating in law making and freely submitting to their self-made laws; and it requires individuals to take control of their lives and be accountable for the consequences of their actions (Lively 1962, 221-28). However, it locates the ground of responsibility not in obedience to divine law but in human pride—in the pride and dignity of free people who seek to improve their condition in this world by mastering their fate, controlling their destiny, and developing the courage, intelligence, and discipline to govern themselves. Moral responsibility in this sense is a wholly secular notion that combines civic duty and self-interest, thereby bridging the gap between the two notions of liberty.

According to Tocqueville, the most admirable features of democratic liberty can be traced to the notion of moral responsibility. It inspires the cooperative endeavors of local government and civic associations, based on the principle of "self-interest rightly understood." Such actions are motivated by the desire of citizens to improve their condition and benefit themselves, while requiring them to develop some of the virtues of self-government (2:127-32). Moral responsibility also inspires the initiative, enterprise, and daring that drives commercial and technological progress; it engenders the pioneering spirit in people who boldly seize the future and make it work for themselves (1:260-61). The same moral

sense can be found in the patriotism and respect for law that is peculiar to modern democratic nations and is especially pronounced in the United States. Such patriotism is different from the sentimental attachment to birthplace and almost religious reverence for the sovereign found in the ancient monarchies of Europe. Democratic patriotism is neither sentimental nor instinctive but rational and reflective; it is the feeling of a citizen who believes the laws and prosperity of the nation are partly the citizens' own creations and are beneficial to them; it is the possessive pride felt by a citizen who consciously says, "This government is mine and works for me" (1:251). This "rational sentiment" also creates respect for the rule of law among democratic citizens because they recognize that laws are voluntarily self-imposed and may be changed if they are harmful (1:258). According to Tocqueville, all of these activities and attitudes arise from the belief that human beings are responsible for their fate and must have the willingness and capability to govern themselves.

In a grand summary statement, Tocqueville asserts that "the greatest advantages of democracy" may be traced to the belief in moral responsibility that underlies democratic liberty. It imparts to "the humblest individual . . . a certain degree of self-respect" and a desire to be "better informed and more active" and to improve his or her condition. It creates "an all-pervading and restless activity, a superabundant force and energy" in the whole society (1:260-61). From such statements, we may infer that Tocqueville praises the notion of moral responsibility because of its effect on the characters of free people: it produces the proud self-assertion and voluntary self-restraint of virtuous citizens. Moral responsibility, in short, raises democratic liberty to the rank of virtue.

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The American Constitution Correctly Understood

Using these insights about the problems of democratic sovereignty and the formation of virtuous characters, Tocqueville develops a distinctive interpretation of the U.S. Constitution. He recognizes that the modern democratic revolution has fundamentally altered the nature of political authority. It has created a distinction between the sovereign and the government or, more precisely, between the sovereign people and their political constitution, which drastically reduces the scope and power of the constitution. In modern democratic societies, the dominant force is the sovereignty of the people or the omnipotent majority; it acts like a *regime*, ordering and shaping the whole of society, while the *constitution* (in the narrower sense) is a limited and artificial device. A modern constitutional system, such as the one found in the United States, is a set of legal and institutional mechanisms designed to serve the needs and protect the rights of the people. As such, it is a kind of epiphenomenon that is largely shaped by the underlying forces of democratic sovereignty and the historical contingencies from which it arises.

In these circumstances, Tocqueville approaches the U.S. Constitution with two objects in mind: to show the debilitating effects of majority tyranny and to show the methods for promoting morally responsible liberty. As a student and admirer of *The Federalist*, he recognizes that the framers of the U.S. Constitution shared these concerns when they created a Republican constitution based on the principle of limited majority rule (1:143, 158-59). Since the founding, however, it has become evident that the constitutional mechanisms created by the U.S. framers—the whole "Madisonian" system of checks and balances—are insufficient to stop, and may even encourage, the pressure of democratic sovereignty and

majority tyranny. Indeed, Tocqueville warns that the U.S. constitutional system is in danger of being completely absorbed by the social forces of the majority, threatening not only the individual rights it is designed to protect but also the intellectual and moral character of citizens (1:60).

Writing in the 1830s (one generation since the founding), Tocqueville observes that the U.S. constitutional system suffers from a decline in the moral quality of leaders and citizens, reflected in a general weakening of character and a relaxation of moral restraints (1:143). He also notes many specific abuses of power. One is the tendency of the legislative branch to dominate all others, an expression of majority tyranny that the framers sought to, but failed to, overcome: "The Americans have not been able to counteract the tendency which legislative assemblies have to get possession of the government" (1:126). Another problem is the extreme weakness of the executive. Tocqueville calls it "an inferior and dependent power" compared to Congress and describes the process of electing and reelecting presidents as "a national crisis" or "a sort of revolution," which stops the wheels of government and turns the executive into "an easy tool in the hands of the majority" (1:142-43). All of these problems can be seen in the behavior of President Andrew Jackson; Tocqueville views him as a populist demagogue, "a slave of the majority," who attacks the powers of the federal government and thereby contributes to both the despotism of the majority and the fragmentation of the union (1:431-32). This points to a third problem, the weakness of the federal system itself. While it possesses the virtue of combining the power and glory of a great nation with the liberty and well-being of small communities, it has an inherent weakness: the government of the union is an artificial creation, "a legal fiction," and therefore commands less

authority and affection than state and local governments; the dissolution of the union, Tocqueville warns, is more than a remote possibility (1:172, 432). Thus, the existing constitutional system in the United States is deficient because it fails to restrain the majority and to contain the centrifugal forces of competing interests. It suffers, one might say, from both the tyranny and the anarchy of the majority.

The reason for these deficiencies, Tocqueville suggests, is that the framers provided legal and mechanical remedies, such as divisions of power or countervailing forces, for what is essentially a moral problem: the tyranny of the majority is ultimately due to a loss of virtue. In its fullest sense, it is a weakening of intellectual and moral character by the whole ethos of modern democracy; it creates despotic tendencies because the people lose the willingness to take responsibility for their lives and the capacity to govern themselves. One way that Tocqueville responds to this problem in *Democracy in America* is by emphasizing the Puritan origins of the U.S. constitutional system (at the beginning of volume 1) and by continuously stressing the primacy of mores to laws. His second solution, which focuses on the laws themselves, is to combine the wisdom of the Puritans with that of the framers in a vision of the constitutional system that stresses the moral education of citizens rather than the mere division of power through institutional mechanisms or checks and balances.

In a passage reflecting both the limitations and the dignity of laws in the democratic age, Tocqueville says, "Laws cannot rekindle an extinguished faith, but men may be interested by the laws in the fate of their country. It depends upon the laws to awaken and direct the vague impulses of patriotism, which never abandons the human heart; and if it be connected with the thoughts, the passions, and the daily habits of life, it may be con-

solidated into a durable and rational sentiment" (1:97). The function of laws, Tocqueville suggests, is to fill the void left by the decay of religion with the inculcation of patriotism. With its echoes of modern nationalism or even totalitarianism, this statement might sound "illiberal" or dangerous. In the context of *Democracy in America*, however, the rational sentiment of patriotism to which Tocqueville alludes is identical to the morally responsible liberty, or genuine self-government, that Tocqueville defends as the modern form of civic virtue; for it requires citizens to take an "interest in the fate of their country," that is, to possess the assertive pride that inspires people to control their destiny and master their fate. At the same time, it requires them to obey their self-imposed laws and to exercise self-restraint. Next to religion, Tocqueville argues, such patriotism is the only force that can voluntarily unite people's wills without creating new forms of despotism (1:97).

In showing how the laws can serve as instruments of moral education, Tocqueville reinterprets and in some cases modifies the constitutional devices created by the framers, such as federalism, judicial review, and the separation of powers. The thrust of his proposals is at first glance contradictory: he recommends greater decentralization of the American federal system as well as greater concentration of power on the national level. The consistency of these proposals, as we shall see, lies in Tocqueville's insight that morally responsible liberty requires the laws to empower the majority by direct involvement at the local level while restraining the majority by independent authority on the national level.

Tocqueville's praise of local government and a decentralized federal system is undoubtedly the most familiar feature of his constitutional analysis. Yet, as Schleifer points out, Tocqueville's sup-

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port for decentralization is actually rather strange; for the exercise of local government would seem to provide new opportunities for majority tyranny and to support local tyranny rather than local liberty (1980, 212ff). Schleifer's inability to resolve this difficulty is due to insufficient clarity about Tocqueville's notions of majority tyranny and responsible liberty. Tocqueville argues that local government gives even the humblest citizen a chance to hold office or to participate in decision making; without this experience, he says, people become "indifferent to their fate" and "sacrifice their free will," that is, they lose the sense of being masters of their fate (1:96). Through direct participation, they realize that "men must walk in freedom and be responsible for their acts, or remain passive spectators . . . in schemes with which they are unacquainted" (1:94). Thus, decentralization may loosen external controls on local majorities, but it mitigates majority tyranny because the essence of such tyranny is not mere conformity but conformity through weakness of will—through passivity, complacency, mediocrity, and resentment of superiority in any form. By contrast, the sense that human beings can control their own destiny through political participation encourages assertive pride and energetic activism on the one hand and obedience to self-imposed laws and voluntary self-restraint on the other (1:98). Such conformity is not tyranny but morally responsible liberty, the liberty of people who are masters of their fate and who govern themselves through laws freely determined and freely obeyed.

Another puzzle in Tocqueville's praise of decentralization is his claim that local government contributes to patriotic unity rather than to political fragmentation. The solution lies in his understanding of democratic patriotism: it springs not from a sentimental attachment to one's birthplace but from a rational attachment to laws and government; it is the feeling of

someone who consciously says, "This government is mine and works for me." Obviously, such rational patriotism arises most naturally from small communities where citizens have the keenest sense that government is their own and contributes to their prosperity. But the attachment to an abstract political entity rather than to a particular plot of land makes it possible for "every citizen of the United States [to] transfer, so to speak, his attachment to his little republic into the common store of American patriotism" (1:170). Thus, a decentralized federal system helps to mitigate the anarchy as well as the tyranny of the majority because the feeling of possessing or being a part of the government fosters a patriotic pride in the nation as whole.

This analysis of decentralization involves a subtle but important shift from the founders' view of federalism because "decentralization is equated with local government and opposed to state and even county government" (Winthrop 1976, 95). Indeed, Tocqueville downplays the states and denigrates the notion of states rights. He regards most of the state constitutions as defective—the terms of office are too short, the legislative bodies dominate the executive, "the despotism of the majority prevails"—and he views the states as the main source of fragmentation and disunity in the nation (1:160, 431). Tocqueville's intention, therefore, is to weaken the states while strengthening local government and the union as a whole.

Another institution that empowers local majorities by requiring direct participation is the trial by jury. Although Tocqueville clearly recognizes this institution as a form of popular sovereignty, he praises it for two reasons. It is a mechanism of a modern constitutional system that acts like a regime, directly imposing the norms of the ruling body on the ruled. The presiding judge is a moral educator, imparting the majesty of the law and legal

notions of equity and proper procedure to ordinary citizens. In addition, the citizens themselves make decisions with fateful consequences for the accused, which provides a lesson in moral responsibility. In Tocqueville's words, "the jury teaches every man not to recoil before the responsibility of his own actions and impresses him with that manly confidence without which no political virtue can exist. It invests each citizen with a kind of magistracy; it makes them all feel the duties which they are bound to discharge towards society. . . . By obliging men to turn their attention to affairs other than their own, it rubs off that private selfishness which is the rust of society" (1:295). This passage is a clear expression of Tocqueville's view that political participation in making decisions ennobles the character of citizens by engendering "confidence" and requiring transcendence of "private selfishness"—an exercise in democratic liberty that shines like political virtue because it gives citizens the pride to take responsibility for their acts and the self-discipline to step outside the narrow circle of their personal lives.

While Tocqueville's praise of local government and jury duty are well known, his calls for greater centralization and concentration of power in the United States are often overlooked—lost, perhaps, in his warnings about the possibilities for new forms of "Caesarism" in volume 1 (p. 341) or the "soft despotism" of a centralized bureaucratic state in volume 2 (p. 335ff). Yet Tocqueville clearly favors greater centralization, of the proper kind, for the United States. His goal is to empower the majority at the local level while seeking greater independence from majority will on the national level by strengthening the federal judiciary and concentrating more power in the executive branch.

These recommendations involve a subtle reinterpretation of the doctrine of separated powers. Most importantly,

Tocqueville does not see the separation of powers as a balancing of three roughly co-equal branches; rather, he gives primacy to the judiciary. He praises the U.S. framers for creating a powerful federal judiciary, especially by granting judges the power to declare laws unconstitutional (judicial review). This power, Tocqueville points out, is a unique feature of U.S. constitutionalism, not to be found in the nations of Europe where judges only decide matters of fact or interpret the law when it is contested in a specific case. The limitations on judicial power in European nations result from their conceptions of the constitution as either an immutable entity beyond the power of judges to alter (the French view) or one that is changeable by the ordinary legislative powers (the English view). Compared to the French and English views, Tocqueville claims, "The political theories of America are more simple and rational. An American constitution is not supposed to be immutable, as in France; nor is it susceptible of modification by the ordinary [legislative] powers of society, as in England. It constitutes a detached whole, which, as it represents the will of the whole people, is no less binding on the legislator than on the private citizens, but which may be altered by the will of the people in predetermined cases according to established rules. In America, the Constitution may vary; but as long as it exists, it is the origin of all authority, and the sole vehicle of the predominating force" (1:105). The U.S. Constitution, in Tocqueville's telling phrase, is a "detached whole"—a fundamental law that stands outside and above the government and the people. Hence, there are two orders of law in the United States, the constitutional and the legislative. The constitutional law is the more fundamental of the two and functions like a "higher law": it cannot be modified by a legislative act and is the standard to which statute or positive law must conform (Pierson 1938,

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603-4). Yet, the Constitution is not really a higher law in the traditional sense. Unlike divine or natural law it does not exist independently of the human will; it is created by the will of the people and may be altered or amended by the people according to preestablished rules. However, as long as it exists, it is absolutely binding on the people and their representatives; the duty of judges is to remind people of this obligation by striking down (or refusing to enforce) laws contrary to the Constitution (1:106). In the process, the people learn the most important lesson of moral responsibility: that they are free to create a fundamental law but must live within the boundaries of a self-created authority. Understood in this way, judicial review functions not only as a check on legislative despotism but as an instrument of moral education.

In addition to teaching citizens and leaders to live responsibly, the judiciary serves an important political purpose. This political purpose, however, is quite different from the contemporary phenomenon of judicial activism—the creation of law or policy by the courts through a flexible interpretation of the Constitution. Tocqueville states that a U.S. judge preserves his moral authority because he does not "attack the legislator openly and directly . . . [but] is brought into the political arena independently of his will" by deciding a case brought by a third party (1:107). He even warns explicitly against judicial activism by saying that "an essential quality of judicial power is never to volunteer its assistance to the oppressed, but always to be at the disposal of the humblest who solicit it" (2:343). The political role of the courts is very different from pronouncing abstract ideals or initiating social change. As Tocqueville says, although the activity of the Supreme Court is "essentially judicial, its prerogatives are almost solely political; its sole object is to enforce the execution of the laws of the Union." He contends

that without the federal judges "the Constitution would be a dead letter: the executive appeals to them for assistance against encroachments of the legislative power; the legislature demands their protection against the assaults of the executive; they defend the Union from the disobedience of the states, the states from the exaggerated claims of the Union, the public interest against private interests, and the conservative spirit of stability against the fickleness of democracy. Their power is enormous, but it is the power of public opinion" (1:157). The political role assigned to the federal judiciary is the conservative one of preserving the unity and wholeness of the union against the jarring and clashing of competing powers that have been built into the American constitutional system.

In this statement one can discern a simple but powerful criticism of the entire Madisonian conception of checks and balances. When power is divided into many centers—when the three branches of government vie with each other, when the several states compete against each other and against the federal government, when diverse interest groups compete and conflict—what remains to hold the system together? Tocqueville's answer is "public opinion," but it is the opinion of the public as shaped by the judiciary in "enforcing" the Constitution, the humanly created system of higher law.

In order to be effective, Tocqueville adds, the justices of the Supreme Court must rise to the level of impartial statesmen, exercising a special kind of political prudence: 'Not only must the Federal Judges be good citizens and men of information and integrity, which are indispensable to all magistrates, but they must be statesmen, wise to discern the signs of the times, not afraid to brave the obstacles that can be subdued, nor slow to turn away from a current when it threatens to sweep them off . . . [for] if the Supreme Court is ever composed of

imprudent or bad men, the Union may be plunged into anarchy or civil war" (1:157). In the final analysis, then, it is not the separation of powers and its checks and balances that make the constitutional system work. It is a single branch, the federal judiciary, that binds the system together into a whole with the power of public opinion and moderates the clash of competing powers and interests. In a curious way, Tocqueville sees the judiciary as the ultimate "executive" of the laws: its primary task is the enforcement of law and its peculiar skill is to enforce the law through the exercise of moral authority—by prudently guiding public opinion. This vision of judicial power as an enforcement power (which resembles Tocqueville's description of the "justices of the peace" on the county level [1:77]) is more political than the view of the founders and less political than contemporary ideas of judicial activism or administrative law. For it gives the federal judiciary the political mission of preserving the union but does not challenge judges to initiate social change by expanding rights or to enforce laws through creative rule making. On the contrary, Tocqueville's appeal is to a kind of judicial statesmanship whose task is to teach all parties to live responsibly within the limits of their self-imposed law, the Constitution—a lesson in moral restraint.

Tocqueville makes a similar though less conclusive argument in recommending a stronger executive. His primary purpose is to expose the weakness of the U.S. president by comparing him to the constitutional king of France. The latter is a more powerful executive for two reasons. First, in addition to executing the laws, the king plays a dominant role in law making by initiating legislation, exercising an absolute veto, and appointing members in one chamber and dissolving the other, at will. Second, he holds his office by hereditary title and thus does not need to be reelected. Even under such a

powerful executive, the government of France is limited by public opinion and is thus "essentially republican" (1:129). In making this comparison, however, Tocqueville does not indicate precisely how far the United States should or could go in imitating it.

One obvious lesson is that the U.S. president should be allowed to acquire more legislative power, permitting a greater consolidation of powers in the executive (1:127-41). A second seems to be that the presidency, although it could never be transformed into a hereditary office, might be strengthened by extending its tenure beyond four years and eliminating reeligibility (as some presidential reformers of today suggest in supporting a single six- or eight-year term). Such a modification follows from Tocqueville's remark that the founders conceived of the executive as a check on legislative despotism, but "by introducing the principle of re-election they partly destroyed their work." Contrary to their intention, they created a system that "stops the wheels of government" every four years, substitutes "management and intrigue for patriotism," and increases the power of the majority (1:142-43). Thus, Tocqueville's ideal American executive is virtually a constitutional monarch, a kind of "patriot king" (Ketcham 1984), who does not merely check the power of Congress but replaces it as the primary law-making entity.

In addition to modifying the separation of powers, Tocqueville seeks to counteract the Madisonian principle of dividing the majority into a multiplicity of interests. He explains the defect of this principle in a passage that reads like a direct response to *Federalist* no. 10: "All the passions that are most fatal to republican institutions increase with an increasing territory, while the virtues that favor them do not augment in the same proportion. The ambition of private citizens increases with the power of the state; the strength

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of parties with the importance of the ends they have in view; but the love of country, which ought to check these destructive agencies, is not stronger in a large rather than a small republic. . . . [For] the inclinations and interests of the people are more diversified by the increase in population, and the difficulty of forming a compact majority is constantly augmented" (1:167). Tocqueville here affirms Madison's premise that extending the size of a republic multiplies the number of competing interests, thereby decreasing the possibility of a compact majority. But in contrast to Madison he regards this as a bad tendency because it weakens the patriotism or love of country that should restrain the ambition of private individuals and political parties (Schleifer 1980, 112-20). Ironically, then, Tocqueville seeks to strengthen the majority by making it more cohesive or compact. But he does so in order to create an internal restraint, the sentiment of patriotism, rather than to rely on the external control of interest checking interest. What Tocqueville seems to be groping for here is not only a more cohesive union but a notion of the *nation* or the *national will*, recognizing that genuine self-government requires a sense of national responsibility rather than the fragmentation and haphazard play of competing interests.

In historical terms, Tocqueville's view of the American constitutional system stands in opposition to the prevailing ideas of his time—the ideas of Jacksonian democracy featuring states rights and sectional divisions, unlimited territorial expansion, the weakening of the national government through executive veto and contempt for the judiciary, and the glorification of the common man. As an alternative, Tocqueville seeks a combination of the best elements of the anti-Federalist and Federalist traditions (Ceaser 1985, 661)—a view that encourages the virtues of local self-government and strong national leadership in a moderately sized

and cohesive union. This vision, in Tocqueville's judgment, is the best way of transforming the rather weak system of checks and balances in the United States into something that resembles a regime, a coherent moral order that teaches citizens responsible liberty and the discipline of self-government.

Conclusion

Tocqueville's constitutionalism is a novel synthesis of old and new ideas, a prudent adaption of ancient constitutional principles to the conditions of modern democracy. It shows how the classical conception of a *regime of virtue* can be used to critically analyze the effects of democratic sovereignty, as well as to correctly understand and modify the American system of checks and balances. At the foundation of this theory is the insight that democratic citizens who exercise liberty with a sense of moral responsibility attain a nobility of character that shines with the brightness of virtue.

In the last analysis, however, Tocqueville recognizes that moral responsibility is something of an illusion. It rests on the belief that people are the masters of their fate and can find within themselves the courage and discipline to govern themselves by laws of their own making. Yet, as Tocqueville asks in a poignant question, "What can be done with a people who are their own masters if they are not submissive to the Deity?" (1:318). By raising this question, Tocqueville indicates that even the noblest conception of human freedom cannot be limitless. It requires the mind to "bow with respect before truths which it accepts without question"; it requires limits imposed by divine law.

Indeed, Tocqueville ultimately appeals to a "higher law" pointing to a notion of moral perfection beyond human freedom, to an end given by nature or ordained by God. While this "higher law" is loosely

mentioned several times in *Democracy in America* (1:269, 288, 434), it is developed systematically only once, in a remote chapter of volume 2 entitled "Of Honor in the United States." There, Tocqueville claims that morality in most times and places is governed by what the dominant classes honor and dishonor, praise and blame, admire and disdain. Although these codes are almost entirely conventional, people usually accept them without question or hesitation. Yet even while they obey, Tocqueville says, "they feel by a dim but mighty instinct, the existence of a more general, more ancient, and more holy law" (2:242). This intimation of a higher law is "the natural order of conscience" or "the general necessities of mankind revealed by conscience to every man." It stands above all conventional laws and valuations and points to the realm of "mere virtue, which lives upon itself, contented with its own approval" (2:244-55).

This intimation of a higher law, which appears to be a residual form of Christian natural law, is the ultimate foundation of Tocqueville's constitutional theory. It points to a standard above the will of the majority as well as the humanly created but morally binding modern constitution. By grounding human virtue in the divine and natural order, it provides the moral compass for all Tocqueville's prudential judgments in the new world of modern democracy. We thus conclude that Tocqueville's constitutionalism is a grand synthesis of classical regime analysis, modern constitutionalism, and Christian natural law, intricately woven into an original and coherent whole.

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NETWORKS IN CONTEXT: THE SOCIAL FLOW OF POLITICAL INFORMATION

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We examine the effects of individual political preferences and the distribution of such preferences on the social transmission of political information. Our data base combines a 1984 election survey of citizens in South Bend, Indiana with a subsequent survey of people with whom these citizens discuss politics. Several findings emerge from the effort. First, individuals do purposefully construct informational networks corresponding to their own political preferences, and they also selectively misperceive socially supplied political information. More important, both of these individual-level processes are shown to be conditioned by constraints imposed due to the distribution of political preferences in the social context. Thus, individual control over socially supplied political information is partial and incomplete. Finally, these information-transmitting processes interact with the social context in a manner that favors partisan majorities while undermining political minorities.

Politics is a social activity imbedded within structured patterns of social interaction. Political information is conveyed not only through speeches and media reports but also through a variety of informal social mechanisms—political discussions on the job or on the street, campaign buttons on a friend's shirt, even casual remarks. Such political information is processed and integrated not by isolated individuals but rather by interdependent individuals who conduct their day-to-day activities in socially structured ways and who send and receive distinctive interpretations of political events in a repetitive process of social interaction. Thus, political behavior may be understood in terms of individuals tied together by, and located within, networks, groups, and other social formations that largely determine

their opportunities for the exchange of meaningful political information (Eulau 1986). In short, *environment* plays a crucial role in affecting the social flow of political information.

People choose their friends and the content of their conversations, but each of these choices is, in turn, bounded by an environment that, for many purposes, must be taken as given. We explore the ramifications of these two-way streets between individually determined friends and discussion content on the one hand and the socially structured supplies of possible friends and conversation content on the other. Hence our focus is on the content and perception of political information and the influence of the social context within which such information is exchanged. We turn initially to an examination of individual control over the

social flow of political information before considering the role of the environment in limiting such control.

Individual Control Over Information

Political information is conveyed through social interaction, but does political preference determine the supply of information, or does the supply of information determine political preference? The direction of influence undoubtedly runs both ways. People choose information sources subject to their own preferences, but they also take what is available. They consume what social structure and social situations supply albeit guided in acceptance by individual perceptual mechanisms (Jones 1986). Thus, several different individual-level processes might account for the high levels of political homogeneity so frequently reported in studies of associated individuals.

High levels of correspondence between individual preference and the surrounding preferences of associated individuals might arise from discretion in the choice and use of associates. Individuals certainly choose to associate with some individuals and to avoid others, and they talk politics with some associates but avoid discussions with others. To the extent that these choices are motivated by a priori political preference, a high level of homogeneity in the politics of associates is the consequence of preference rather than its cause.

Indeed, the dominance of political preference over social influence corresponds well with a choice-theoretic perspective on political behavior. In his *Economic Theory of Democracy*, Anthony Downs argues (1957, 229) that it is fully rational for individuals to reduce information costs by obtaining political information from personal contacts. Among the benefits of socially obtained

information is the ability of the receiver to choose the information source and thus to select a set of biases and viewpoints on the part of the source that the receiver finds congenial: "It is often relatively easy," writes Downs, "for a man to find someone he knows who has selection principles like his own."

A second source of reported political homogeneity within informational networks arises from measurement procedures. The level of correspondence between a respondent and associates is often determined on the basis of the respondent's own report. A single respondent provides information not only on his or her own political preferences but also on the political preferences of associates. Thus, if individuals selectively screen the political information they receive through social interaction, they may also systematically misperceive its content (Lauman 1973). Political homogeneity produced in this fashion is, from a purely technical perspective, a measurement artifact. Notice, however, that this is a measurement artifact with significant theoretical consequences. It indicates that disagreeable interactions can be reinterpreted (misperceived) in an agreeable light. Even if individuals are unable to control the objective content of socially transmitted political information, they are still able to control the interpretation of that content in a manner that reduces political dissonance, and thereby minimizes the impact of social influence.

Finally, additional subtleties may operate as well. An individual who is well aware of an associate's political preferences and also knows them to be at variance with his or her own beliefs may very well misrepresent, not fully communicate, or deliberately make ambiguous his or her own preferences precisely in order to reduce dissonance in the relationship (Macoby, Matthews, and Morton 1954). Hence, a discussion partner may be quite prepared to misperceive political informa-

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tion when, simultaneously, the transmitter is prepared to obfuscate the message (Jones 1986). It is not only political elites or candidates for public office (Shepsle 1972) who may have motives to heighten communication ambiguity in democratic politics (Calvert 1985; MacKuen n.d.).

In summary, three individual-level factors undermine the potential impact of socially transmitted information. Citizens choose with whom to discuss politics, they reinterpret dissonance-producing information, and they may deliberately misrepresent their true opinions. These individual-level capabilities bring us full circle. Political information is conveyed through social interaction, but individuals choose with whom to interact, and they also suppress dissonance-producing information if it is not already rendered politically inoffensive by the transmitter. On these premises, socially transmitted political information becomes, wholly and completely, an extension of individually based political preference. The potential influence of socially transmitted information is explained away on the basis of choice, misperception, and misrepresentation: sociology gives way to economics and individual psychology.

This position may be juxtaposed with a view that makes a case for the significance of social structure in determining the social transmission of political information. Not only is it worthwhile to inquire into the structural factors that motivate people to conform or to misrepresent their positions, but one might also examine the manner in which social structure determines interaction opportunities or even imposes interactions.

Social Control Over Information

A case for social influence might be made on either theoretical or empirical

grounds. Anticipating empirical support for social influence we ask, Is there a theoretical basis from which to build an expectation for the influence of socially transmitted information?

First, to argue that the selection of information sources takes precedence over social influence is to assert that political preferences dominate social-influence processes running in the other direction. It is a gross simplification to assume that individually defined interests translate directly into individual preferences (Lewis-Beck 1986, 342-44), for preferences must be informed, and information arrives through socially structured channels. In particular, as McPhee (1963) persuasively argues, political preferences are sustained within particular structural locations, and they are configured by the multiple interactions of the individual within those structural locations. Thus, to the extent that political preferences are dependent on (socially supplied) information, they are inherently dynamic and responsive to social influence. Preference is not only inflicted upon socially transmitted information, but socially transmitted information is inflicted upon preference. Mutual causation between social contexts and individual preferences is inherent in this view of mass political-information processing.

Second, it is often difficult for political scientists to remember that politics is not at the forefront of most choices that most individuals make. Assuming for the moment that individuals do indeed choose their own locations within the social structure, the choices are likely to be predicated on matters other than politics and political preference (Brown 1981). People choose a job because it pays better or because they need work. People choose a neighborhood because they can afford one of the houses. People choose a church because their parents raised them in it. The point to be emphasized is that politics and political preferences are ancillary to

most of the significant life choices people make, the choices that locate individuals within the social structure. But these choices, made largely on nonpolitical grounds, may nevertheless impinge mightily on politics. Particular locations within the social structure expose citizens to particular social contexts and the information biases such contexts willy-nilly provide. These social contexts circumscribe the opportunities for social interaction. Thus, individual choice regarding associational patterns and political discussions may be seen as operating within the opportunities and constraints imposed by a given social context.

These processes are stochastic. It is possible to buy into an upper-class neighborhood and find oneself surrounded by Democrats, but a preponderance of Republican neighbors is more likely. Individuals probably reason over these bundles of probabilities in a similar fashion: If I buy into this neighborhood, the chances are my daughter will date good Catholic boys (and ultimately marry one), the reasoning might run. But recognizing the contingent nature of the processes of living does not make the (stochastically determined) impact of events any less. Indeed, events that are not certain are more effective conditioners than those that occur regularly, as Skinner (1938) demonstrated long ago.

This is not to say that individuals are powerless in deciding with whom to discuss politics. In her study of a predominantly Democratic automobile plant, Finifter (1974) demonstrated that Republican autoworkers became socially introverted, resisting association with the numerically superior Democrats. In his study of a working-class suburb, Berger (1960) demonstrated that the middle-class minority was excluded and withdrew from community life. It is not that choice is absent, but rather that associational choice is contingent—a locationally specific response to a particular social mix.

The role of the mass media may be deceptive in regard to these social processes. The nationalization of political-information sources achieved through the disappearance of multiple-newspaper cities and the dominance of television may very well enhance the coercive power of informationally biased social contexts. If individuals in one-newspaper and three-look-alike-network cities are to hear second opinions, the only recourse is probably socially transmitted new information. But this will be biased by its social location, that is, by the social context. Hence, readily available political information from the modern mass media may, because of its increased uniformity and homogeneity, increase rather than decrease the political influence potential of the social context (Laurily K. Epstein, personal communication, 1986).

Evidence will help in assessing the substantive significance of the various processes we have sketched. We turn now to empirical matters with some analyses taken from a study with a research design intended to cast light on the sorts of issues just raised.

Networks and Contexts in South Bend

As a first step, it is crucial to draw an important, if abstract, theoretical distinction between *contexts* and *networks*: contexts are structurally imposed while networks are individually constructed. Contexts are external to the individual even if the composition of the context depends upon the makeup of individuals contained within it. In contrast, networks represent the product of myriad choices made by people who compose the net, but choices that are circumscribed by the opportunities and constraints imposed by context. To present an extreme example, a Mondale voter who resides in a context where all other individuals support Reagan will be unable to discuss politics

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with another Mondale voter. As a practical matter, this distinction between networks and contexts may blur, but it provides a useful analytic tool with which to consider the issues raised above.

The analyses are based on a study undertaken in South Bend, Indiana during the 1984 presidential election. The South Bend study was designed with a particular motive: to locate mass political behavior—political attitudes, candidate preferences, party preferences, policy positions, and political activities—within the social patterns of everyday life. Two structural ingredients are especially germane here: the informational networks through which individuals send and receive political information and the larger but still very immediate contexts within which individuals reside. Both networks and contexts were observed and measured as part of the South Bend study, and clarity about measurement procedures is important for understanding our results.

A three-wave panel survey of approximately 15 hundred respondents was conducted during the summer and fall of 1984 before, during, and after the presidential election. The respondents lived in 16 different South Bend-area neighborhoods purposely selected to maximize social homogeneity within the neighborhoods and social heterogeneity between the neighborhoods. These neighborhoods constituted the primary sampling units, and a high rate of random sampling was achieved in each neighborhood. The neighborhoods vary from extremely affluent planned suburban developments through respectable middle-class neighborhoods, through well-maintained working-class and ethnic enclaves, to poor and deteriorating central-city neighborhoods. All the neighborhoods are composed of predominantly white populations, and the small number of black respondents are excluded from the analyses reported below. (More detailed

information regarding study design, response rates, and related issues is available upon request from the authors.)

Approximately equal numbers of respondents, in turn, were randomly selected within each of the neighborhoods and interviewed through the computer-assisted telephone interviewing facility of the Indiana University Center for Survey Research. Intensive sampling within neighborhoods produces two distinct advantages. First, any survey item can be aggregated to produce a measure of neighborhood context; thus, we can construct survey-derived estimates of neighborhood support for Reagan in the 1984 election. Second, intensive sampling makes it possible to employ procedures developed by Boyd and Iversen (1979) for the analysis of contextual data; we employ a variation of the Boyd and Iversen centering procedure to produce approximately orthogonal explanatory variables.

Contexts are measured at the level of the neighborhood, using an aggregated survey item to provide the contextual measure, and the relevant individual-contextual property is voting behavior in the 1984 election. Thus the context of political behavior, as distinguished from the network of political discussion, is defined geographically and socially on a priori grounds and measured directly from survey respondents. While our measure of context is the geographically based neighborhood, the social context is clearly a much broader concept that encompasses wider domains, such as religious affiliation, recreational activities, and social organizations. Thus the measurement employed here is narrower (and more conservative) than the theoretical construct it represents. Indeed, more comprehensive measures of social contextual experience would likely demonstrate even larger effects than those reported in our tables.

Networks are measured from two vantage points: first, that of the main respon-

dent to the survey; second, that of the political discussant named by the main respondent. Respondents to the third-wave survey, conducted shortly after the 1984 election, were asked to give the first names of three people with whom they were most likely to have discussions about politics. If respondents could not name anyone with whom they discussed politics, a follow-up question asked for the first names of three people with whom they were likely to have informal conversations. The vast majority of the respondents were able to identify political discussants. The nature of the probe is very important because it was intentionally structured around a clear political reference point. We did not ask our main respondents to give us a list of their best friends or their close associates. Rather, we asked them to tell us with whom they discussed politics—their social sources of political information. Not surprisingly, many of the main respondents named discussants who were spouses and other relatives. However, for the purposes of this paper, all relatives by either blood or marriage are omitted; our focus is entirely upon unrelated discussants.

Notice that our definitions of *contexts* and *networks* do not require that the two overlap territorially. That is, a network may lie wholly outside the context, but this fact would not negate interdependence between these two elements of social structure. A person who lives in a context surrounded by Reagan voters may assume that everyone votes for Reagan, even when encountering individuals outside that context. Furthermore, a person imbedded in a Reagan context probably has a higher likelihood of encountering Reagan voters even when the encounter occurs beyond the context. Contexts create opportunities for social interaction which are not necessarily bounded by the original context.

A series of follow-up questions asked main respondents for information and

perceptions regarding the discussants, including the presidential candidate for whom the discussant voted in 1984. A final question asked for the last name and street address of each discussant so that we could ask the discussants "a few short questions regarding the last election." The main respondents, perhaps surprisingly, were quite forthcoming with this information. Based upon their responses, the staff at the Center for Survey Research constructed a sampling frame of approximately 18 hundred discussants, and our interviewing budget allowed us to conduct a fourth interview with more than nine hundred discussants. In addition to these interviews, fifty-five main respondents reported a discussant who had also been interviewed in the third wave. This was not totally unexpected since the neighborhoods were small in size with a great deal of social integrity, and the sampling rate within the neighborhoods was accordingly relatively high. After the relationships between spouses and other relatives are removed the resulting dyadic data set includes about five hundred conversational relationships. Twenty-two percent of the relationships in our analyses involve repeated use of a main respondent—instances when a main respondent has more than one relationship represented in the data matrix.

Congruence and Dissonance Between Discussion Partners

The resulting data set offers a unique opportunity to examine political choices as they are imbedded within the social flow of political information. As a first step we examine (1) the objective correspondence between the preferences of main respondents and their discussants, (2) the perceptions of the political preferences of discussants held by main respondents, and (3) the accuracy of these

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**Table 1. Proportion of Main Respondents Who Correctly Perceive
Discussant's Voting Behavior**

Main Respondent	Nonvoter	Discussant	
		Reagan Voter	Mondale Voter
Nonvoter	.222 (9)	.790 (19)	.818 (11)
Reagan voter	.400 (20)	.912 (170)	.662 (65)
Mondale voter	.333 (15)	.547 (53)	.922 (90)

Note: Omits relatives. Percentage of Reagan voters' relationships that produce agreement = $170/(20 + 170 + 65) = 67\%$. Percentage of Mondale voters' relationships that produce agreement = $90/(15 + 53 + 90) = 57\%$.

perceptions. Table 1 summarizes this information for the relationships included in this analysis.

Voters with similarly voting discussants show a remarkably high level of accuracy in their perception of discussant behavior: 91% of Reagan voters with Reagan discussants correctly identified discussant behavior, and 92% of Mondale voters with Mondale discussants made similarly correct identifications. Accuracy is reduced somewhat within relationships that produce disagreement. Both Reagan and Mondale voters have considerably more difficulty in correctly identifying non-voting discussants, but 66% of Reagan voters correctly identify discussants who voted for Mondale, and 55% of Mondale voters correctly identify discussants who voted for Reagan. This general pattern changes among the nonvoting respondents: they do not accurately identify fellow nonvoting discussants (22%), but they are fairly accurate in their identification of Reagan voters (79%) and Mondale voters (82%). Finally, Table 1 shows the general levels of agreement within relationships: 67% of the Reagan voters' relationships involve discussants who voted for Reagan, and 57% of the Mondale voters' relationships involve discussants who voted for Mondale.

Magnitude in political science is, like beauty, in the eye of the beholder, but Table 1 suggests several things to us. First, these relationships are not overwhelmingly homogeneous in terms of voting preference. They include substantial levels of political disagreement measured in the metric of reported votes. Second, while perceptual accuracy generally increases in politically congruent relationships, clear majorities of voters in dissonant relationships recognize the disagreement.

What factors are responsible for objectively defined congruence and dissonance within the relationships? What factors are responsible for accurate and inaccurate perceptions?

The Structural Basis of Political Information

In the analysis that follows we consider the preferences of discussion partners from two different perspectives: the perceptions of those preferences by the main respondents, and the objective reality of those preferences as they are measured by the discussants' own self-reports. The analysis comes in three

question-stages: (1) Are the main respondents' perceptions of discussion partners affected by the political preferences of, and by the political preferences surrounding, the main respondents? (2) Are main respondents likely to have discussion partners whose objectively defined (self-reported) preferences correspond to the political preferences of, and the political preferences surrounding, the main respondents (in this instance the social context becomes important as it affects the supply of particular political preferences and, thus, the opportunities for discussion with people holding particular preferences)? and (3) Do the main respondents systematically *misperceive* discussant preferences as a function of their own preferences and as a function of the preferences surrounding them?

The relationships between networks, contexts, and individual preference are estimated using a logit model for micro data (Hanushek and Jackson 1977). The model is appropriate to these data where the dependent variables are binary—Reagan discussant or not, perceived Reagan discussant or not. An otherwise straightforward analysis is complicated by two factors: First, the choice and perception of discussion partners is one part of an inherently simultaneous social-influence process. A citizen's own political preferences influence the choice of a discussion partner, but this choice has subsequent consequences for the citizen's preference. These two phenomena are inherently dynamic and interdependent. The resulting technical problem is simultaneity leading to identification problems in the statistical models to be estimated. Discussant choice may be a function of individual preference and individual preference may be a function of discussant choice. Although we do not carry out the analysis for the implicit full simultaneous system, it is appropriate to take the simultaneity into account in estimation.

The simultaneity problem is addressed

in the analysis that follows through the construction of two instruments that measure the likelihood that any individual (1) voted for Reagan or (2) did not vote. Each instrument is constructed from the logistic regressions shown in Table 2. Thus, rather than using dummy variables to represent the main respondents' voting behavior, we employ the probabilities estimated by the instruments (Maddala 1983). The instruments avoid the measurement bias due to simultaneity. They are constructed to correlate highly with reported behavior but to be (relatively) independent of discussant behavior (Wonnacott and Wonnacott 1979).

The second estimation problem arises due to the danger of potentially omitted explanatory factors. Beginning at least with the work of Hauser (1974), critics of contextual research have demanded that alternative, individual-level explanations be taken into account before asserting a contextual effect. In principle, this is laudable, but in practice it has often meant the inclusion of long lists of correlated individual-level control variables (Kelley and McAllister 1985). It should come as no surprise that the inclusion of highly correlated explanatory variables often weakens statistical purchase. That is, additional individual-level controls run the very real danger of producing excessive collinearity and thus a misplaced willingness to accept null hypotheses.

The research design for the South Bend study provides at least a partial solution to this second problem. By centering the individual-level variable around its mean within contexts and by centering contextual means around the mean for the sample as a whole, orthogonality is produced between the centered individual measure, the centered neighborhood mean, and their multiplicative interaction. This does not guarantee orthogonality between these variables and the additional individual-level controls, but it decreases collinearity to a degree sufficient for the

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Table 2. Construction of Instruments for Main Respondents' Voting Behavior

Independent Variable	Probability of Reagan Vote	Probability of No Vote
Constant	-4.46 (4.66)	.92 (.67)
Age ^a	.01 (1.38)	-.05 (3.89)
Education ^b	.13 (2.37)	-.32 (3.72)
Party identification ^c	.83 (12.12)	—
Partisan loyalist ^d	—	.49 (2.68)
Interest ^e	—	1.50 (6.32)
Number of cases	506	506

Note: Logit model. T-values are shown in parentheses.

^aMain respondent's age in years.

^bEducation of main respondent (years of schooling).

^cSeven-point party-identification scale (0 = strong Democrat).

^d0 = strong partisan; 1 = weak partisan; 2 = independent leaning toward a party; 3 = independent.

^eInterest in election: 1 = a great deal; 2 = some; 3 = only a little; 4 = none at all.

present analyses. (See the Appendix for technical details.)

To summarize, the main respondent's voting behavior is measured through the use of two instruments that measure the likelihood of voting for Reagan and the likelihood of not voting. This removes the taint, technically, of causation from the discussion partner's behavior in statistical estimates of coefficients. A mean likelihood of voting for Reagan is then calculated within the neighborhood and for the sample as a whole. The individual-level Reagan vote instrument is centered around the respective neighborhood mean; the mean Reagan vote likelihood within neighborhoods is centered around the overall sample mean; and an interaction variable is formed from the product of these two variables. Finally, two demographic controls for income and education are included in each model as well.

These procedures are all well-understood and straightforward applica-

tions of statistical technique. They have the unfortunate side effect of appearing as a great deal of hocus-pocus; thus, we include a second model in each analysis that uses simple and straightforward procedures: no instruments, no centering, no interaction, no individual-level controls, where the main respondent's right-hand-side explanatory voting behavior is measured through the use of dummy variables. This simplified model corresponds to Alwin's formulation for estimating the effects of social context (Alwin 1976). We are not surprised (and the reader should be reassured) to find that the results are much the same for both models.

The centering procedure is best accomplished by calculating neighborhood means from the sample that is employed for a particular analysis. This means that, on average, the neighborhood means are calculated from approximately 24 interviews within each neighborhood. The reliability of the neighborhood means is

supported by their very high correlation ($r = .95$) with the proportion of the entire third-wave sample in each neighborhood voting for Reagan. And in this latter case, the average sample size in each neighborhood is more than 85. The general reliability of the survey-derived contextual measures is further supported by the correlation of third-wave neighborhood demographic means with demographic measures for the neighborhoods taken from the 1980 census. Once again, the survey measures do quite well, correlating with the census measures at the .9 level.

A final measurement issue involves response bias in the survey. Our respondents report a level of voting turnout that is in excess of the true population value. This is due in part to common overreport problems, but it is also due to the panel design. People who agree to participate repeatedly in a panel study concerned with politics are more likely to be upper-status people, and they are also more likely to be voters. Thus, the question arises whether using neighborhood means provides a reliable measure of Reagan support within the neighborhoods. If the measure is intended to provide a precise estimate of the neighborhood Reagan support to the base of all eligible voters, then it certainly fails the validity test because turnout is undoubtedly overestimated. But this bias does not render the measure useless for present purposes. What is central to the analysis is variation in Reagan support across neighborhoods, and the measure is appropriate for such use. As we showed above, survey-derived measures of neighborhood social demographics compare very well with the census-derived measures of social demographics, and there is no reason to expect that survey-derived measures of political demographics would compare any worse. Thus, in spite of the overreport bias, there is strong reason to believe that it provides a good relative measure of Reagan support across neighborhoods.

Perhaps more importantly, the measure of neighborhood Reagan support has an additional virtue: it provides an estimate of Reagan support to the base of the politically active neighborhood population—the population that should matter both substantively and theoretically. That is, the survey ends up, *de facto*, being a survey of politically engaged citizens, and politically active citizens tend to be surrounded by other active citizens. Thus, from a substantive standpoint, the bias in the neighborhood means corresponds well to the bias in the sample. In terms of anticipated effects from the social context, this is a desirable design feature. The politically active population is the theoretically correct population to sample.

The Perception of Information

Does the perception of a discussant's preference continue to vary as a function of the main respondent's own preference when the social context is taken into account? Does the perception vary as a function of Reagan support in the main respondent's context?

Table 3 displays the results for the logit model that estimates the main respondent's perception of the discussant's vote as a function of both the main respondent's voting behavior and the level of Reagan support in the main respondent's neighborhood. Reagan support at both the individual and contextual levels generates coefficients that lie in the expected direction and possess respectable *t*-values. Respondents who support Reagan and (to a lesser degree) respondents who are less likely to vote, are more likely to perceive their discussants as Reagan voters than the respondents who support Mondale. Furthermore, respondents who live among Reagan voters are more likely to perceive their discussants as Reagan voters.

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The magnitude of these effects are characterized in Table 4, where the probability of perceiving a discussant as a Reagan voter is computed from the model estimates reported in Table 3, across main respondent preferences and across the observed range of neighborhood Reagan support. In order to arrive at main respondent preferences, the mean value of both instruments is calculated within the three possible behavior categories of Reagan voter, Mondale voter, and non-voter. The two demographic controls are

held constant at the sample means. (See the Appendix for details.)

Table 4 shows that regardless of context, Reagan voters are most likely to perceive their discussants as Reagan voters and Mondale voters are least likely to do so. The table also shows that main respondents who reside among Reagan voters are more likely to perceive their discussants as Reagan voters, regardless of individual preference. This latter effect is substantial, and it compares quite favorably to the effect of individual

Table 3. Projected Reagan Vote for Discussant by Vote of Main Respondent and Reagan Support in Main Respondent's Neighborhood

Independent Variable	Instruments with Controls ^a	Original Data
Constant	-.40 (.52)	-3.15 (5.96)
Main-respondent Reagan support ^b	2.79 (7.97)	1.82 (7.78)
Non-voting main respondent ^c	.80 (1.08)	1.31 (3.51)
Neighborhood Reagan support ^d	4.58 (4.69)	4.04 (4.10)
Individual-context interaction ^e	3.88 (1.48)	—
Education of main respondent ^f	.003 (.06)	—
Income of main respondent ^g	.08 (1.06)	—
Number of cases	444	468

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is projected Reagan vote. *T*-values are shown in parentheses. Projected Reagan vote = 1 if main respondent thinks discussant voted for Reagan; 0 otherwise.

^aIn this column, main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

^bThe original variable equals 1 if main respondent voted for Reagan; 0 otherwise. The instrument varies from .05 to .98.

^cThe original variable equals 1 if main respondent did not vote for president; 0 otherwise. The instrument varies from .001 to .92.

^dMean level of Reagan support in main respondent's neighborhood, calculated from survey (mean instrument in left column; proportion Reagan voters in right column).

^eMain respondent Reagan support multiplied by neighborhood Reagan support.

^fYears of school completed by main respondent (possible range = 0-17; approximate mean = 13).

^gFamily income (possible range = 1-8; approximate mean = 5, or 20-30 thousand dollars per year).

Table 4. Probability That Main Respondent Perceives Discussant as a Reagan Voter by Main Respondent's Vote and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Main Respondent's Vote	Neighborhood Reagan Support ^a	
	Low (.31)	High (.78)
Reagan	.38	.87
Nonvoter	.32	.76
Mondale	.21	.57

^aThese levels represent the range, on the neighborhood proportions, who reported voting for Reagan, for 1,390 nonmissing third-wave respondents. The corresponding centered neighborhood means for the Reagan support instrument, upon which the estimates are based, were -.21 and .24. Data from Table 3, col. 1.

preference. Differences of 17 and 30 points are shown between the perceptions of Mondale and Reagan voters, and the difference between main respondents living in Reagan and Mondale neighborhoods ranges between 36 and 49 points.

It is important to realize that the table displays something other than marginal effects. In an 80% Reagan neighborhood, if all that was involved was the increased supply of Reagan voters, the expected value should be the same—.8 for all three classes of voters. It is not. Indeed, the table shows either a discussant-selection bias based upon individual party preference—a consequence of personal preference imposed on social structure (Finifter 1974), or some form of misperception that is perhaps complex (Jones 1986), or both.

A common question looms behind both sets of effects. Do individual preferences and contextual properties affect the perception or the reality of the socially transmitted information? Put another way, Are these perceptions rooted in an accurate assessment of discussant preferences or in an interpretation that distorts discussants' actual (self-reported) preferences? We divide this problem into two components by (1) investigating the discussants' self-reported preferences before (2) examining the main respondents' perceptions in light of discussants' self-reported preferences.

The Content of Information

Is the objective content of socially transmitted information affected by individual preference? Is it affected by the social context? These questions are attacked indirectly by considering the discussant's self-reported vote (interpreted here as the objective political content of socially transmitted information) as a function of the main respondent's vote and the level of Reagan support in the main respondent's neighborhood. Our concern is not with the conditions that affect perceptions of discussant preferences but rather with the conditions that affect the reality from which these perceptions are developed.

The logit model of Table 5 estimates the choice of a Reagan voting discussant as a function of the main respondent's voting preference and the main respondent's social context. Essentially, the table answers the question, How powerful are your preferences and the preferences that surround you in determining the likelihood that you will have a discussion partner who reports a particular preference? The coefficients of central interest lie in the expected direction and each possesses a satisfactory *t*-value. Reagan supporters are more likely to have discussants who voted for Reagan, and main respondents living among Reagan voters are more

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Table 5. Self-reported Reagan Vote for Discussant by Vote of Main Respondent, and Reagan Support in Main Respondent's Neighborhood

Independent Variable	Instruments with Controls ^a	Original Data
Constant	.28 (.41)	-1.69 (3.72)
Main-respondent Reagan support	2.11 (6.43)	1.03 (4.91)
Nonvoting main respondent	1.03 (1.48)	.52 (1.56)
Neighborhood Reagan support	2.72 (3.04)	2.34 (2.68)
Individual/context interaction	2.84 (1.13)	—
Education of main respondent	-.08 (1.46)	—
Income of main respondent	.16 (2.19)	—
Number of cases	466	491

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is discussant Reagan vote. *T*-values are shown in parentheses. Discussant Reagan vote = 1 if discussant reports voting for Reagan; 0 otherwise.

^aIn this column, main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

likely to have discussants who voted for Reagan. The table provides evidence for the selective choice of discussants based on the preferences of main respondents and also evidence for social coercion arising from the partisan supply available in the social context of the main respondents. It is not only the perception of discussant preferences that is affected by context but the reality underlying these perceptions as well. Not only are citizens who live in contexts dominated by Reagan supporters more likely to perceive their discussion partners as Reagan voters, but these citizens are also more likely to have discussion partners who are Reagan voters.

The magnitudes of these effects upon the probability of having a self-reported Reagan voter for a discussant are displayed in Table 6. As before, the effect of context compares favorably to the effect

of individual preference, but the effects demonstrated in Table 4 are somewhat more pronounced. Reagan voters are 15 and 26 points more likely than Mondale voters to have a discussant who voted for Reagan. And in comparison to main respondents living among Mondale voters, main respondents who live among Reagan voters are between 22 and 33 points more likely to have a discussant who voted for Reagan. Thus, the objective political content of socially transmitted information (the true partisanship of the discussant) depends both upon associational choice on the part of the receiver and upon the constraints on supply arising from the political composition of the social context that circumscribes that choice.

Note that there is no reason to expect the partisan behavior of discussion partners to reflect the contextual distribution

Table 6. Probability That Discussant Voted for Reagan by Main Respondent's Vote and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Main Respondent's Vote	Neighborhood Reagan Support	
	Low (.31)	High (.78)
Reagan	.46	.79
Nonvoter	.44	.71
Mondale	.31	.53

Note: Data from Table 5, col. 1.

of partisanship if discussion partners are chosen only on the basis of the selector's individual partisanship. The latter condition predicts the same cell entries in both columns of a given row in Table 6, which manifestly is not the case. In short, then, the objective content of the partisan signal in the political discussion process is a function of both the individual's preferences and the preferences in the individual's surroundings. But does this make any difference? If misperception is sufficiently systematic and sufficiently strong, then objective content may not matter. We now turn to an investigation of this final issue.

The Perception and Misperception of Information

Is the misperception of socially transmitted information affected by the political preferences of the receiver? Is it affected by the social context? More importantly, does misperception reinforce individual political preference in the choice of discussion partners and thereby offset the potential of the social context to expose individuals to sources of dissonant information?

To address these issues, it is necessary to take account of the objective (self-reported) voting behavior of both the discussant and the main respondent when examining the main respondent's percep-

tions of the discussant's vote. The logic of the analysis is to investigate the main respondent's perception of the discussant's vote while controlling for the discussant's objectively defined (self-reported) vote. With objective content controlled, it is possible to assess whether individual misperception overwhelms the effect arising from information exposure to partisan distributions in the individual's social context.

Table 7 displays the logit model that is obtained when the main respondent's perception is treated as the left-hand-side dependent variable, and the self-reported voting behaviors of both the main respondent and the discussant, as well as Reagan support in the neighborhood, are treated as right-hand-side explanatory variables. The explanatory variables of central interest produce coefficients that lie in the expected direction with crisp *t*-values. In general, main respondents are more likely to perceive a discussant as a Reagan voter if the discussant voted for Reagan (objective reality), the main respondent resides among Reagan supporters (social reality), and the main respondent supported Reagan (subjective reality). The table shows that controlling for the discussant's objective partisanship does not eliminate either individual-preference effects or social-context effects on perception.

These results become more provocative when the magnitudes of effects are considered in Table 8. This table is striking

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**Table 7. Projected Reagan Vote for Discussant by Vote of Main Respondent,
Self-reported Vote of Discussant, and Reagan Support in
Main Respondent's Neighborhood**

Independent Variable	Instruments with Controls ^a	Original Data
Constant	-2.33 (2.40)	-4.39 (6.52)
Discussant Reagan vote	2.81 (9.52)	2.94 (10.17)
Nonvoting discussant ^b	.49 (1.11)	.46 (1.08)
Main-respondent Reagan support	2.10 (4.95)	1.77 (6.00)
Nonvoting main respondent	.62 (.66)	1.39 (2.97)
Neighborhood Reagan support	4.20 (3.59)	3.45 (2.93)
Individual-context interaction	3.26 (1.02)	—
Education of main respondent	.07 (1.01)	—
Income of main respondent	-.01 (.11)	—
Number of cases	428	450

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is projected Reagan vote. T-values are shown in parentheses.

^aIn this column main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

^b1 if discussant did not vote; 0 otherwise.

**Table 8. Probability That Main Respondent Perceives Discussant as a
Reagan Voter by Main Respondent's Vote, and the Proportional Reagan Vote
in the Main Respondent's Neighborhood**

Vote of the Main Respondent and Discussant	Neighborhood Reagan Support	
	Low (.31)	High (.78)
Reagan-Reagan	.68	.95
Reagan-nonvoter	.18	.65
Reagan-Mondale	.12	.53
Nonvoter-Reagan	.64	.91
Nonvoter-nonvoter	.15	.51
Nonvoter-Mondale	.10	.39
Mondale-Reagan	.54	.84
Mondale-nonvoter	.10	.34
Mondale-Mondale	.07	.24

Note: Data from Table 7, col. 1.

Table 9. Probability That Main Respondent Correctly Perceives Whether or Not the Discussant Is a Reagan Voter by Main Respondent's Vote, the Discussant's Vote, and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Vote of the Main Respondent and Discussant	Neighborhood Reagan Support	
	Low (.31)	High (.78)
Reagan-Reagan	.68	.95
Reagan-Mondale	.88	.47
Mondale-Reagan	.54	.84
Mondale-Mondale	.93	.76

Note: For main respondents and discussants who voted for Reagan or Mondale. Data from Table 7, col. 1.

for the probability differences it reveals between objectively homogeneous versus objectively dissonant dyads and, similarly, between politically supportive and nonsupportive social contexts. These results can be made even more vivid by focusing on only the dyads of central theoretical interest and by transforming the dependent variable. In Table 9 the dependent variable has been transformed into the probability of correctly identifying whether the discussant is a Reagan voter. Thus, a main respondent who perceives a Mondale voter as a Reagan voter is incorrect, as is a main respondent who perceives a Reagan voter as a non-voter, and so forth.

Table 9 is worth some study. It shows, first, that voters tend to be accurate in their perceptions of discussion partners who agree with their own preferences: Reagan voters tend to recognize other Reagan voters, and Mondale voters tend to recognize other Mondale voters. This is especially the case when surrounding preferences are supportive of the voter's preference. In both instances the social context has at least a modest effect upon perceptual accuracy. But now examine disagreeing dyads.

Clearly, the greatest opportunity for contextual effects upon perceptual accuracy arises in discussion dyads that involve heterogeneous preferences. This is

consistent with theory about the dynamics of the political discussion process (Huckfeldt 1986; MacKuen and Brown 1987; McPhee 1963; Sprague 1982). Reagan voters are more likely to perceive Mondale discussants accurately in Mondale contexts and less likely to perceive them accurately in Reagan contexts. Similarly, Mondale voters are more likely to perceive Reagan discussants accurately in Reagan contexts and less likely to perceive them accurately in Mondale contexts. Thus, majority versus minority status for a particular political preference in the social context emerges as a crucial consideration in cases where there is objective political disagreement between discussion partners (Noelle-Neumann 1984).

Finally, notice that the holders of a *minority* preference are more likely to perceive the *majority* preference accurately: they are more likely to perceive their discussion partner's political preference correctly if *disagreement* is present within the dyad. Thus, the social context transforms the nature of discussion dyads even between discussion partners who share the same preference. Members of the minority evidently come to expect political dissonance during social encounters and frequently fail to recognize political agreement even when it is present.

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Interpretation

Two interpretive comments are central to understanding these results. First, a politically supportive social context sustains the receiver's misperception of dissonance-producing, socially transmitted political information. Second, members of the political minority accurately perceive members of the majority, while members of the majority do not accurately perceive members of the minority.

More analysis is necessary to sort out the nature of these contextual effects, but the substantive political consequences seem clear. Minorities are debilitated (Miller 1956) because they are often acutely aware of their own minority status. They are likely to recognize the variance between their own preferences and those of their surroundings. In contrast, and for whatever reason, dissonant information is frequently ignored by members of the majority.

In short, the majority enjoys a double benefit while the minority suffers a double liability. Not only are members of the minority more likely to encounter dissonance-producing information, they are also more likely to recognize it as such. Not only are members of the majority less likely to encounter dissonance-producing information, they are also less likely to recognize it as such.

Conclusion

The empirical analyses above emphasize the interdependence of private preference and politically relevant distributions in the individual's social context. The interesting conditions, as expected on the basis of theory, are those of dissonance—either between the main respondent and the discussant, or between the main respondent and the preponderance of political opinion in the respondent's social context. It is clear,

especially from Table 9, that these conditions interact:

It would appear that individuals do purposefully attempt to construct informational networks corresponding to their own political preferences. This exercise of choice, however, is not independent from socially determined conditions of supply—from the contextually imposed opportunity (or lack of opportunity) for social interaction with people who hold various political preferences. Similarly, selective misperception is apparent in the analyses, and it, too, is socially conditioned. (These issues are addressed in Huckfeldt and Sprague, n.d.)

The most interesting feature of the empirical work, perhaps the most surprising result, and certainly the most significant for the operation of democratic politics, is the asymmetry in choice and perception arising from majority or minority political status in one's social context. Political majorities are able to ignore dissonant information. Political minorities, in contrast, suffer from a heightened level of vulnerability for the simple reason that their members accurately perceive the incoming flow of dissonant information. This analysis reveals, then, a process of informational coercion of political minorities achieved through mechanisms of social interaction.

Appendix

The analysis conducted here benefits greatly from the statistical framework for contextual analysis set forth by Boyd and Iversen (1979), but it also marks a significant departure from their model. (For another application of the Boyd-Iversen model see Esser 1982.) They develop their argument within the general framework of the linear model, but we adapt part of their centering procedure to this analysis using the nonlinear logit model. The differences are sufficient to warrant a brief explanation.

First, a great virtue of the Boyd and Iversen approach is that variance may be partitioned between individual effects, contextual effects, interaction effects, and error. Since the logit model has no error term, this is impossible in the current application.

Second, Boyd and Iversen develop some useful techniques to check for appropriate model specification that cannot be used in this instance. The root cause of this inability is, once again, the lack of an error term. In particular, we are unable to check for homogeneity of error across contexts, or for correlation between explanatory variables and error across contexts. An especially crucial test in the Boyd and Iversen model is the ability to recover equivalent coefficients for the two-equation and the single-equation specifications, but the two-equation specification cannot be estimated using the logit model. Thus, while we cannot carry out this check using the logit model, we have carried it out with the South Bend data for other specifications of the linear model with different dependent variables. These tests have been uniformly successful.

Our inability to carry out the Boyd and Iversen tests for misspecification is unfortunate, but it is not crippling, at least by contemporary standards. The vast majority of contextual studies, and indeed every empirical study, make assumptions regarding the behavior of error terms. The utility of Boyd and Iversen's approach is that it offers a way to check many of these assumptions, but few empirical studies are able to employ such a technology.

Finally, the Boyd and Iversen centering procedure not only transforms the explanatory variables, but the dependent variable as well, in order to maintain the original metric on the estimated coefficients. This cannot be done in the current instance; thus, the metric for coefficients is transformed. This is not a problem, however, because the metric for coeffi-

cients in a nonlinear model, such as the logit model, offers little interpretive insight. Instead of interpreting the magnitudes of coefficients, we use the logit model to calculate change in probabilities across values of one explanatory variable while other explanatory and control variables are held constant. As was stated in the text, social demographics are controlled at their sample means, and the instruments are controlled at their mean levels for the respective reported vote categories. The range on centered neighborhood means for Reagan support is calculated on the basis of the entire third-wave survey in order to take advantage of the larger sample size. The following outline shows the values for the controlled variables, as well as providing the central formulas for the centering procedure:

A. Centering procedure

Centered Reagan support at the individual level = $R_{ij} - \bar{R}_j$

Centered Reagan support within neighborhoods = $\bar{R}_j - \bar{R}$

where

R_{ij} = the score of the i th respondent in the j th neighborhood on the Reagan support instrument

\bar{R}_j = the mean score on the Reagan support instrument in the j th context

\bar{R} = the mean score on the Reagan support instrument for the sample

B. Control values used to generate estimated probabilities in Tables 4, 6, 8, and 9

1. Demographic means

Education = 13

Income = 5

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2. Means for instruments across reported vote categories

Instruments	Reported Vote		
	No Reagan	No Vote	Mondale
Centered Reagan vote probability	.18	-.08	-.26
Nonvote probability (not centered)	.06	.36	.09

3. The range on centered neighborhood Reagan support (calculated from the entire third wave)

highest neighborhood level of Reagan support = .24

lowest neighborhood level of Reagan support = -.21

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WHO WORKS WITH WHOM? INTEREST GROUP ALLIANCES AND OPPOSITION

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Interest-group interactions may be examined in ways comparable to the analysis of conflict and coalition in other areas of political science. We seek to measure and compare the structure of interest-group participation and conflict in four domains of U.S. domestic policy: agriculture, energy, health, and labor. Data are drawn from a survey of 806 representatives of organizations with interests in federal policy, supplemented by interviews with 301 government officials in the same four domains. Several types of data are adduced regarding the intensity and partisanship of group conflict in each domain and the range and variety of group participation. Coalitional patterns are described and the mutual positioning of different kinds of organization—peak-association groups versus more specialized trade, professional, or commodity groups, for example—are examined.

Some fields of intellectual endeavor enjoy the luxury of exploring well-defined problems whose difficulties, however intractable, are themselves part of a self-conscious culture of inquiry. Other fields of academic effort may well have substantial common ground, but its contours and possibilities have not yet been fully recognized by those committed to its exploration. One such field is interest-group politics. For two decades now we have gradually been coming to appreciate that the questions and concepts we had thought were central to this area of investigation were substantially misspecified and that quite

another tack was needed if we were to make sense, theoretical or descriptive, of the burgeoning mass of activity to which we attach the label of *interest groups*.

Since Mancur Olson (1965) turned us in a different direction, we have been the beneficiaries of important work on the origins and maintenance of voluntary associations (*inter alia*, Berry 1977; Moe 1980; Salisbury 1969; Walker 1983). Concepts have been reformulated in order to encompass institutions within the interest-group frame of reference (Salisbury 1984). We know much more than we did before about lobbying tactics and strategy (Berry 1977; Schlozman and Tierney 1985). We

have accumulated sophisticated treatments of both individual group and sector-level development and change over time (Browne and Salisbury 1972; Hanson 1985; Laumann and Knoke 1987; Starr 1982; Walker 1983). And we have useful cross-national comparisons of the structural ties among business, labor, and the state (Schmitter and Lehbruch 1979; Wilson 1985). Here we propose to extend the range of our attention to a topic central to many fields of political science but not yet addressed by students of interest groups—the structure of conflict.

Whenever a more or less well-defined system of interactions can be observed or inferred—in legislatures, electoral politics, or international relations, to take three good examples—we move quickly to characterize that system in terms of the conflicts over scarce resources that have so long been held to be the defining feature of politics. We may focus on particular episodes—the struggle over a bill in Congress, a specific presidential contest, or the Falklands War. We may select out some feature of the overall structure for attention—the committee system, third-party challenges, or coalition patterns in the United Nations. We have developed quite a few fragments of theory regarding both generic conflict patterns and behavior conditioned thereby and the operation of specific institutional systems. It may be that the linkages between the more abstract and the more particular are sometimes weakly forged, but it seems plain that substantial progress has been registered by growth in cumulative understanding, theoretical and empirical.

We propose to argue that interest-group politics in the United States constitutes a system, or set of systems, defined by interaction among organized groups and between those groups and public officials. The structure of this system, and of subsystems within it, may usefully be characterized in terms of the extent and shape of conflict among par-

ticipants. This structure is not evident in every instance of policy dispute, nor is it necessarily revealed by any particular case example of interest-group activity. But, as with voting studies or congressional roll calls, the interest-group system or systems can be discerned at more aggregated levels of observation.

The Research Design

It is ironic that it should have taken so long to get at the structure of interest-group conflict. Since the conceptualizations of Arthur Bentley (1908) began to work their yeasty way into political science, it has been commonplace to put group conflict at the very core of our thinking. Having done so, however, we have been so far unable to go beyond examples to reach more comprehensive formulations of how group interactions are structured. Is there much conflict or little? Are the main group players aggregated into two opposing camps or fragmented among several "parties"? Does the structure vary from one policy area to another? We cannot speak to these questions from existing literature. Recent work has greatly improved our understanding of how groups are formed and what they do, but it has not addressed the structure of conflict (for a partial exception, see Schlozman and Tierney 1985, 283-88). Part of the reason may be the absence of any readily observed defining behavior. Party systems can be examined by looking at the distribution of votes. So can legislatures. So can the Supreme Court. The executive branch is more difficult because criterion actions that could be the basis for characterizing their relationships are not visibly undertaken by all participants. Interest groups presented the same difficulty. No set of observations of group activity with enough scope of coverage or iterations of behavior has been made to allow reasonable inferences

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to be drawn regarding the existence and shape of systemic structure. Our data overcome these difficulties.

The design of our research began with the construction of lists of organizational participants in each of four policy areas—agriculture, energy, health, and labor. It was our firm belief that unless we linked our analysis of interest representation to the substantive policy concerns of the groups involved we would have a bloodless set of responses, interest-group activity without interests. In order to secure enough respondents in a given policy area to enable our analysis effectively to incorporate these substantive considerations, it was necessary to limit ourselves to a few policy domains. Our selection was bound to be somewhat arbitrary, but we have sought to tap some of the important dimensions of interest representation and to capture a reasonable portion of the range and diversity of representational activities.

The policy domains we examine exhibit very different patterns of institutional and policy change over recent decades. In agriculture and labor there has been a relatively stable set of executive agencies and congressional committees and subcommittees; and the programs in these fields, while not without controversy, have displayed substantial continuity in general design. Health and (especially) energy, by contrast, have undergone much greater changes. The past decade has seen each come under the jurisdiction of a new or redesigned cabinet-level department. Congress has developed new mechanisms to deal with energy issues, and the issue agendas of these two fields have been in great flux.

Both level and form of conflict were expected to vary. Labor policy typically pits two broad constellations of interests—business and labor—against each other, and the struggles are sometimes of epic proportions. Health matters, by contrast, were once the focus of great struggle, but,

although health-care-cost containment continues to inspire sharp disagreement, most health issues in recent years have attracted quite muted political voices. Apart from the labor field, we expect to find a general tendency toward interest proliferation and fragmentation as the interests of various types of specialized producers—doctors, insurance companies, medical schools, and so on in health; oil and gas, coal, nuclear, and other fuel interests in energy; various crop and commodity interests in agriculture—grow increasingly differentiated. To be sure, there are peak associations in some domains—agriculture and health, as well as labor—but no organization can claim even this status in energy, and in the other fields the hegemony of the American Medical Association or the American Farm Bureau Federation has been badly eroded in recent years.

We expect our domains to display diversity, therefore, in the patterns of interaction among organizational actors, and we will be able to investigate to what extent this diversity reflects differences in the substantive policy and organizational composition of the domains. Alternatively, we can estimate how much of what individual-interest representatives do reflects their personal backgrounds and present job circumstances and how much is, in effect, imposed by the contemporary context of political and institutional forces within which interest representation must operate.

Our selection of policy domains is skewed toward domestic policy. Issues of foreign trade and other international concerns do arise, but they are not of central concern in any of the domains in our study. Interest representation on domestic matters may be larger in volume and differently structured than on matters of national defense, foreign aid, and other issues involving international considerations. Even those differences are hardly total, however, and we would hope that

our findings sufficiently reflect the bulk of the policy process to be of some relevance to the understanding of private-interest representation well beyond the four domains of policy examined.

The procedures employed to identify organizations active in a particular policy domain required substantial documentary investigation and preliminary interviewing. No existing list adequately defines the population of interested participants. Each policy area includes several subsystems that attract somewhat different types of participants, and any single method of locating participating organizations has a systematic bias toward identifying certain kinds of groups and neglecting others (Salisbury 1984). We used four different listings of organizations for each policy domain, and the resulting rosters are broadly inclusive not only of the highly active groups but also of the marginal and intermittent participants. The sampling methods are more fully described in the Appendix.

Having defined the universe of group participation (and welcoming the natural weighting that resulted from multiple nominations), we drew a sample of 311 organizations, equally distributed across the four domains. By telephone we interviewed an appropriate official from each, asking, among other things, who represented their interests in Washington. Each group could name up to four individuals within the organization and up to four people from outside. These nominations (totaling 1,716 individuals) then became the basis for sampling respondents. A total of 774 were interviewed in person, a response rate of 77%. Interviews averaged about 75 minutes in duration. These data form the heart of our research. We asked each respondent to name three government officials with whom he or she regularly interacted and then interviewed 301 of those officials. Finally, we added 32 persons who had been among 72

nominated by informants as notable participants in the four policy domains but who, unlike the other 40, had not appeared in our sample.

Some of the analysis that follows treats respondents as individuals constrained by operating within a policy domain and other factors as well but giving their personal reactions and estimates. In some of the analysis, however, we let the individuals stand for the organizations that employ them and treat the organizations as the actors. This is appropriate for purposes of examining the patterns of organizational conflict, but it generates one unanticipated and unfortunate side effect. Approximately 20% of our respondents are external lawyers and consultants. Each was nominated by an organization, but in a fair number of instances they work also for other clients. We found that when asked about allies and adversaries or about actions and attitudes relating to specific events, these respondents gave replies that could not dependably be assigned to the nominating organization. Too often we could not tell what interests were reflected. Consequently, we determined to treat external lawyers and consultants as employees of their respective firms and not try to include them in the ally-adversary structures.

Quite a few of the larger organizations are represented by more than one respondent in our sample; indeed, there are as many as four from the AFL-CIO, the chamber of commerce, and a few others of comparable significance. There are also some state and local chapters or branches of national organizations, which we treat as part of the same category as the parent group.

We turn now to our empirical findings regarding the structure of group conflict in the four policy domains. We have three distinct sets of data bearing on the general question. First, for both Washington representatives and government officials,

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we report perceptions of conflict in each domain. We compare domains to see what differences appear in these perceptions and also examine some likely indicators of individual variation in perception. Second, we look at the patterning of alliance and opposition as revealed by responses to questions asking interest representatives to nominate other groups they worked with or considered as adversaries. A third set of data, to which only brief reference will be made in this paper, is drawn from responses to questions regarding actual participation in each of some 20 very specific events that occurred during the five years prior to our interviews in 1983-84. If a group had been active and taken a particular position, it could then be located, through smallest-space analysis, in relation to all the other groups active in that domain. From these quite different data sets we derive reasonably consistent pictures of the extent and shape of group conflict in each domain. In turn, we can compare those structures and draw inferences regarding both policy-domain characteristics and, more generally, the character of interest-group conflict in the United States.

The Perception of Issues

As we attempt to assess the nature of interest conflict in our four policy domains, it will be helpful to have some idea of how the interest representatives themselves characterize the issues they deal with. In this section we examine three dimensions: *stability*, *scope of participation*, and *intensity of conflict*. Our data are drawn from responses to pairs of opposing statements. Respondents placed themselves on a five-point scale, but in the presentation that follows, rather than treat them as interval data, we simply subtract the percentage of *disagree* responses from the percentage of *agree* choices. In no instance was there any discernible bimodality of responses; thus

we are satisfied we have not lost or obscured anything substantial in opting for this simple procedure. The data are presented in Table 1.

We present two measures of issue stability, one on the long-livedness of issues in the domain and one on the stability of group coalitions. A large majority in each policy area sees the issues as long lasting, and both government officials and interest representatives share this view. Indeed, the agreement is sufficiently great that it might be accepted as settled that most of the business of policy-making entails working and reworking issues with a long history. Not many "new" items get on the agenda, and few matters are ever truly disposed of with dispatch.

There is far less agreement regarding the stability of issue coalitions, though in each domain a modest majority perceives stability rather than flux. The size of that majority is reasonably consistent across policy domains, though higher in labor than elsewhere. Except in the health field, government officials perceive substantially less stability than do interest representatives. We will explore the possible meaning of these differences after we have reviewed the other two sets of issue characteristics.

To assess the scope of interest-group activity in each domain, we asked respondents to estimate the amount of attention given by the general public to domain issues and the relative number of groups actively interested in them. Two points can be made. First, the energy field is seen as considerably broader in participation and attention than the others. Second, agriculture is seen as the narrowest. Doubtless this reflects the excitement surrounding energy issues in the period of our inquiry, but it may also be true that there is a particularly wide array of substantive concerns in this domain. It is interesting that energy representatives offer such high scope estimates. In the health field the representatives give much

Table 1. Perceptions of Issue Characteristics (in Percentages)

Domain Issue Characteristics	Agriculture		Energy		Health		Labor		Total	
	Repre-sentatives	Government Officials								
Issue stability Issues on long-lived Coalitions are stable	68 26	52 5	78 21	17 17	71 28	59 28	68 35	56 19	72 27	64 22
Scope of domain participation High public visibility Large number of actions	38 17	47 35	78 52	65 53	49 33	68 61	54 34	63 47	53 34	60 49
Level of domain conflict High intensity of conflict Strong partisanship	49 -18	6 -11	70 11	88 31	44 -7	45 3	70 37	69 46	48 4	67 15
Number of respondents	189	74	181	80	214	73	190	74	774	301

Note: Figures are the percentage of positive responses minus the percentage of negative responses.

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lower figures than the government officials. This suggests that the health representatives are more substantively specialized than the officials, and hence encounter a smaller slice of the domain's business. In energy, it appears, this distinction is not present. Energy politics have been in such turmoil that it is not surprising that both officials and representatives have been exposed to a broad scope of activity.

A third dimension of domain-issue characteristics involves the degree of group conflict. We asked about the intensity of conflict in general and also about the extent to which domain issues were contested in partisan terms. The two questions draw sharply different responses—conflict is generally said to be high and partisanship to be low. Government officials rather consistently perceive somewhat greater conflict than do private-interest representatives. As one moves from one domain to the next, however, responses on the two questions move more or less together and display rather startling domain differences. Agriculture and health are less conflict-dominated and much less partisan than energy and labor. The labor domain appears to be intensely partisan in comparison with the other domains, and this characteristic ought to appear in the data on allies and adversaries as well. Partisan conflict should be expressed as essentially bipolar opposition whereas the ubiquitous but less partisan disputes of the energy realm might be expected to take a more dispersed, pluralistic form. When we examine the ally-adversary data, we can see whether this expectation is confirmed.

In an effort to probe more deeply into the sources of variation in the issue perceptions of individual representatives, we undertook an extensive probit analysis, employing a considerable number of independent variables. The results were not sufficiently robust to warrant their full

presentation here, but they did generally support one important conclusion. We employed several different indicators of individual characteristics, including organizational position, prior government experience, present degree of policy representation effort, and personal values. A contrasting set of considerations consisted of the policy domain involved and the substantive type of organization represented, including labor unions, farm groups, and so on. In virtually every case the latter considerations had much more substantial effects. Agriculture is quiet and nonpartisan; labor is noisy, conflictual, and partisan. Energy and health are highly visible with many participants, while agriculture is perceived to be otherwise. Peak-association representatives see themselves more embroiled in controversies of broader scope and attention (apart from the actual range of their issue involvement) than do those who serve more specialized interests. These differences make reasonable sense and give support to our hope of capturing some diversity by our choices of policy domains.

Allies and Adversaries: Who Works with (and against) Whom?

In this section we examine responses to two questions. First, we asked whether the organization employing the respondent regularly encountered other organizations, as either allies or adversaries, in the particular policy domain. If they answered affirmatively, we asked the respondent to name up to three of each. The responses allow us to chart in considerable detail the patterning of inter-organizational relationships. We will distinguish between organizations that are deeply embroiled in group conflict and those that remain on relatively friendly terms with others in the domain. We will

Table 2. Nominations of Interest-Group Allies and Adversaries

Domain	Allies	Adversaries
Agriculture		
Total nominations	422	287
Organizations named	186	116
Nominators	161	133
Energy		
Total nominations	449	321
Organizations named	143	95
Nominators	165	142
Health		
Total nominations	497	244
Organizations named	215	96
Nominators	185	125
Labor		
Total nominations	446	349
Organizations named	184	99
Nominators	167	151

also examine how domains differ in their conflict structures and the extent to which those differences are embedded in the substantive differences among interests in different domains.

Overwhelming majorities in each domain are able to identify both allies (89.9%) and adversaries (74.8%). Labor is most completely defined in ally-adversary terms, health and agriculture somewhat less so. Responses do not differ much by organizational position, though government-affairs specialists are a bit more likely than others to name adversaries.

Probit analysis of the same kind referred to in the previous section indicates that indeed the labor domain is especially likely to generate adversaries. Moreover, labor groups in whatever domain they participate, are significantly more likely to identify adversaries than are business peaks, business corporations, citizen or externality groups, and energy producers. Time spent by the representative in the domain also makes it likely that adversaries will be named. The full equation produces an estimated r-square of .27 and correctly predicts 77.5% of the cases.

That seems a reasonably robust result and one in which most of the predictive power is found in the categories of organizations rather than in individual-level variables or in the domain. As we examine the interconnections among these organizational types, we will thus have good reason to regard them as meaningful, apart from other considerations.

When we turn to the actual nominations, we find a very sizable array of organizations named. Table 2 presents the total numbers for each domain. We really cannot say whether the domain differences in Table 2 are large or small because we lack any comparative standard, but it is clear from the ratio of nominators to nominee organizations that in the energy domain the players agree somewhat more fully about both friends and enemies. In the labor area, the adversary list is relatively concentrated, while in both agriculture and health allies and adversaries are more diffuse.

In order to comprehend the specific alliances and antagonisms in each domain, we have sorted both the nominating organizations and their nominees into groups of similar organizational types.

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Table 3. Allies and Adversaries in Agriculture

Nominee Organization	Nominating Organizations			
	Farm Peaks	Commodity Groups	Trade Associations	Externality Groups
Ally				
Farm peaks	30	26	9	6
Commodity groups	27	60	13	5
Trade associations	8	10	41	4
Externality groups	5	0	6	38
Number of respondents	26	35	27	24
Adversary				
Farm peaks	20	18	3	9
Commodity groups	12	3	2	5
Trade associations	2	6	1	15
Externality groups	6	25	41	9
Number of respondents	22	28	19	20

We wished to make the process as parsimonious as possible, but we also felt it necessary to adjust our categories somewhat to take account of elements specific to each policy domain. Many organizations defy easy categorization. Energy firms are often involved in the production or sale of more than one kind of fuel, for example, and a classification of organizations based on fuel type, of critical importance in energy politics (Chubb 1983), encounters difficulties. In agriculture some commodity groups are composed entirely of farmer-producers while others include processors, shippers, commodity brokers, and so on (see Guither 1980). We do not claim to have escaped error, but we believe that most of the more than one thousand organizations named were sensibly classified, and that the findings, accordingly, are credible.

The agriculture domain presents the simplest array of categories, though not the plainest structure (cf. Bonnen 1980; Browne 1986; Hanson 1985). The four categories in Table 3 are *farm peak organizations*, the general organizations of farmers and their subsidiaries; *commodity groups*, producers or producer-dominated organizations specializing in a

single crop or farm commodity; *trade associations*, organizations of corporations not directly involved in agricultural production; and, borrowing Hadwiger's (1982) term, *externality groups*, including environmental, welfare, labor, and consumer groups concerned about the externalities of farm policies.

Table 3 presents the number of nominations received by organizations in each category (omitting the considerable number scattered across other kinds of groups). It shows several things of importance. First, the groups in each category tend very strongly to find their allies within their own organizational category. As we will see, this is true also in every other domain. Secondly, farm-peak organizations are internally divided. Both friends and enemies are found there, by the peak organizations and by each of the other sectors. To a lesser extent this is true also of the externality set. Neither commodity groups nor trade associations, on the other hand, often find adversaries internally or in the other. Despite the great diversity within each category and the real potential for conflict between, say, feed producers and livestock interests, the reported organizational alignments do not

Table 4. Allies and Adversaries in Energy

Nominee Organization	Nominating Organizations				
	Business Peaks	Oil and Gas	Nuclear and Electric	Trade Associations	Environmental Groups
Ally					
Business peaks	3	10	1	15	3
Oil and gas	11	57	4	23	0
Nuclear and electric	4	3	38	11	0
Trade associations	6	7	5	24	0
Environmental groups	0	1	8	1	18
Number of respondents	9	32	27	31	8
Adversary					
Business peaks	0	0	0	0	0
Oil and gas	1	14	0	5	4
Nuclear and electric	0	1	5	1	5
Trade associations	0	2	3	1	4
Environmental groups	14	37	51	46	3
Number of respondents	8	27	26	29	8

suggest such conflict. The primary object of adversarial concern among these two sets of specialized interests, commodity groups and trade associations, is the externality category. Apart from the externality groups themselves, few organizations choose them as allies, and they loom very large as opponents. As we will see, a comparable tendency can be observed in other policy domains as well.

The energy field reflects a pattern much like that of agriculture, with one principal exception. In energy politics the role of peak associations—those that transcend particular industries, like the chamber of commerce or the National Association of Manufacturers (NAM)—is quite modest. Here again, however, we see specialized producer interests working closely with one another and with relatively few adversaries within the industry. Trade associations are less differentiated from producers in energy than they are from those in agriculture, though in both domains they seem to avoid making enemies among producer groups. Hostility from the producer interests and peak business organizations is visited on the environmentalists. Three-fourths of the adver-

saries named by the energy groups included in Table 4 were environmental groups. Rarely were they said to be allies. Thus they occupy the same functional position in energy as externality groups in agriculture, a category that in that domain also includes environmentalists.

The health-policy domain presents some features resembling energy and agriculture. Here, too, we find that interests select their allies primarily from within their own organizational categories. The externality groups—citizens organizations of various sorts including the Nader-related groups, and labor unions—are often the targets of adversarial relations. More often than in other domains we have examined, however, these groups are also chosen as allies. We take this to mean that the health domain is somewhat less rancorous than others. We noted earlier that a smaller proportion of health respondents had indicated that they regularly encountered opposing groups. Moreover, health representatives perceived their issues to be less conflict ridden and less partisan than did participants in other domains. The diversity of relationships with the citizen and labor

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Table 5. Allies and Adversaries in Health

Nominee Organization	Nominating Organizations					
	Medical Associations	Academic Groups	Hospitals	Disease Groups and Nonfederal Officials	Trade Associations	Citizens and Labor
Ally						
Medical associations	41	10	6	3	0	6
Academic groups	15	39	8	0	0	0
Hospitals	14	10	42	3	2	4
Disease groups and nonfederal officials	6	1	0	29	0	10
Trade associations	1	0	0	0	46	1
Citizens and labor	5	5	6	8	6	50
Number of respondents	31	25	23	19	20	25
Adversary						
Medical associations	19	1	0	0	0	7
Academic groups	1	0	0	0	0	0
Hospitals	3	2	1	5	0	11
Disease groups and nonfederal officials	0	0	0	3	2	0
Trade associations	2	1	2	4	3	8
Citizens and labor	17	9	11	6	21	16
Number of respondents	21	13	14	10	16	19

category is further evidence on the same point.

In the health domain there are no genuine peak associations. The American Medical Association might be so regarded, but only for doctors, and it is obvious that doctors are only one among several sets of interest-group participants, albeit a highly significant set. No health group successfully transcends the several major categories of interested parties and lays a persuasive claim to speak for the health-policy community. It may well be that this very fact helps account for the somewhat lower level of acrimony characterizing health politics. Indeed, in an earlier era, when the AMA did largely dominate the full agenda of health issues, the domain seems to have been considerably more conflict ridden (Starr 1982). Analogies to superpowers in international politics may be farfetched, but it is not implausible to argue that the existence of

superpowers tends to polarize conflict and that polarization tends to lead to intensification.

Other points of interest in Table 5 include the fact that academic and hospital groups are especially reluctant to name any adversaries. Veterans, a group category not included in the table, carry this strategy to perfection; they claim to have no organized group opponents at all. The congressional politics of health issues is often strenuously contentious, of course, but what these data indicate is that the conflict is not so much among competing interests represented by adversary organizations as with the more amorphous forces of budget constraints and administration priorities. Group politics is an important, sometimes even decisive, part of the overall policy-making process; but it is seldom, if ever, all one needs to know to explain policy outcomes.

We turn now to the labor-policy do-

Table 6. Allies and Adversaries in Labor

Nominee Organization	Nominating Organizations				
	Labor Peaks	Unions	Citizens Groups	Trade Associations	Business Peaks
Ally					
Labor peaks	2	32	8	2	0
Unions	9	37	11	11	2
Citizens groups	15	17	30	3	4
Trade associations	0	1	5	54	15
Business peaks	0	0	5	30	42
Number of respondents	10	35	26	43	23
Adversary					
Labor peaks	0	0	6	20	22
Unions	1	3	3	20	12
Citizens groups	4	24	24	2	4
Trade associations	1	11	5	7	0
Business peaks	15	41	17	4	2
Number of respondents	10	33	24	33	21

main. Here it is plain that we encounter a bipolar conflict structure. The peak associations of labor and business dominate the poles, and there is almost no trafficking with the other side. Individual unions and trade associations are slightly less implacable foes. The latter, especially, have worked out some alliances with the unions. Citizens groups in this domain are a diverse lot, primarily aligned with labor but including a significant number of anti-labor groups, producing substantial intra-category conflict. One revealing feature of Table 6 is that in the conflict-charged climate of labor policy, unlike the other three domains, nearly every respondent was willing to name adversaries as well as allies.

These findings confirm our data regarding the characteristics of issues, where the labor domain evidenced the highest levels of conflict and partisanship. The ally-adversary structure mapped in Table 6 is congruent with the perceptions of labor issues. It is also confirmed by the patterns of event-specific activity reported by our respondents. Groups' relationships to one another in respect to what positions, if

any, they had taken on particular policy issues revealed a clear bipolar structure on labor issues. In the other domains, the event-triggered patterns were segmented along lines essentially similar to the organizational groupings identified in Tables 3-6. (For further discussion of these events-based data see Laumann et al. 1986.)

Conclusions

We began by suggesting that the interest-group universe could be regarded as a system of structured conflict (and cooperation). Our data suggest that this is certainly valid at the subsystem, or policy-domain, level (though we are really not able as yet to portray or characterize in any detail the complete "pressure system," to employ Schattsneider's [1960] convenient, if misleading, term). Domain subsystems have been seen to have relatively stable patterns of interaction, quite sharply ideological and bipolar in the labor-policy domain but fragmented, primarily among sets of

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specialized producer organizations, in the other areas.

Specialized producers, with relatively narrow policy agendas, tend to avoid becoming embroiled in adversarial encounters. As becomes the protagonists in a system of distributive politics, they try instead to confine their efforts to building whatever support they can for their primary policy goals. These live-and-let-live efforts are complicated in some domains by the presence of peak associations, organizations that seek to transcend the limited membership potential of specialized groups like trade associations and to formulate broader policy agendas as they try to speak persuasively on behalf of an entire economic sector. Peak associations tend to express their policy goals in the more encompassing language of doctrine and ideology, using abstraction to rise above the tangible differences of interest among more specialized producers.

The more prominent and unified the peak associations in a policy domain, the more polarized its group structure and policy struggles become. The limiting case in our data is labor, where the peak associations of business and labor do battle and pull the more specialized groups into one orbit or the other. In agriculture, by contrast, the peak associations, once the basis of cohesive bloc politics, are themselves divided. Farm policy-making tends now to be dominated by narrower and often shifting coalitions and to be articulated with less partisan or doctrinal fervor than was the case, say, in the 1950s.

In each of these policy domains, the role of externality groups—environmentalists, consumers, and other “citizens” aggregations—is a kind of mirror image of the peak associations. Where, as in labor, the latter are powerful, the externality groups are diverse, divided, and often serve as satellites to the “superpowers.” Where peak associations are divided or nonexistent, the externality in-

terests provide the principal focus of opposition, perceived as getting in the way of the realization of specialized producer interests (see also Schlozman and Tierney 1985, 285-86). Externality groups often like to think of themselves as the guardians of the “public interest” against the diverse claims of multifarious “special interests.” With or without that normative language, we find that this structure does indeed characterize three of our four policy domains.

Our interpretation of the role of peak associations in structuring interest-group conflict moves in a very different direction from the principal lines of argument regarding societal corporatism and from the widely discussed contention of Mancur Olson (1982) regarding the significance of encompassing groups. In the corporatist model, effective peak associations of labor and business are necessary elements along with the state in a tripartite structure of cooperative negotiation (Schmitter and Lehbruck 1979). The singularity of the United States in failing to develop along these lines has been examined elsewhere (Salisbury 1979; Wilson 1982). Our data suggest that even when we do find reasonably effective peak associations, group relations are more rancorous, not more cooperative.

Olson's argument also would lead one to expect that the more encompassing the group (i.e., the more effective the peak associations), the more efficient (i.e., less dominated by narrowly focused producer interests) the policy decisions. The finding that intense conflict develops when the group politics moves from a fragmented, “special” interest-dominated pattern to one in which more broadly encompassing groups are the main players, does not seem compatible with most notions of efficiency. There may, of course, be a stage beyond bipolar group politics in which a single encompassing “group” is able to accomplish fully Pareto-optimal results, but at that point it would appear that we had

passed beyond the sphere of democratic politics.

We believe that our findings are relevant to the ongoing debate concerning the validity of pluralist models of U.S. politics. It is obvious that our design has been premised on the assumption that a multiplicity of groups compete in meaningful ways for the rewards at stake in public decisions. But the patterns of group action we have reported go well beyond that. In his classic study of New Haven, Robert Dahl (1961) rests much of his pluralist interpretation on the discovery that business interests (and other interests also) pursued agendas that stopped far short of the full range of public issues and policy decisions. Consequently, their resources, though imposing, were often not brought into play. In his more recent statements, characterized by a considerable degree of recantation, Dahl (1985) and his colleague Lindblom (1977, 1983) place less emphasis on the theme of selective participation as the crucial factor limiting power and protecting pluralism. They stress instead the structural distinctiveness of business interests and the inherent political advantage derived therefrom.

Our data probably cannot illuminate the debate regarding the more abstract properties of business power in a capitalist order. They do show, however, that there is great variety in the forms of politically active business interests (as well as of labor, farmers, professional groups, and others), and that the variety of form is systematically related to the agendas and strategies of action. Trade associations and specialized producers are not simply smaller, narrower segments of a larger class or sector. They often have different goals and operate in different ways, not as anomalies or deviations from some class-defined normalcy but through rational strategy calculated to advance genuine interests.

If, as we have found, the substantive

policy domain is a dominant factor in determining the way interest representatives perceive the political context in which they operate and if, further, the structures of domain conflict and cooperation are built from the particular configurations of interest organizations that participate in domain politics, it follows that an adequate understanding not only of interest-group politics but of the full policy process requires us to keep a careful descriptive watch on the group composition of a domain. Unless we know who the organizational players are, how they relate to one another, and what dynamics of organizational development and change are at work, we are unlikely to secure an adequate analytical grasp of the situation. Neither research monographs nor beginning texts should continue to talk about "the farm bloc," the dominant role of the American Medical Association, or the unity of "business" interests when events have long since rendered those empirically invalid. Our effort to describe the structures of conflict and cooperation among organized groups may thus be taken as a contribution to an ongoing obligation to attend systematically to the *substance* of interest-group politics.

In the end, of course, we must be concerned not only with the structures of interest-group relations but with their policy outcomes as well. What is the reflection in substantive decisions of the fragmented politics of agriculture or the bipolar conflicts over labor issues? Does a "play-it-safe" trade association accomplish more of its objectives than a contentious citizens group? In the next phases of our research, we hope to be able to speak to these questions. Meanwhile, however, showing that interest-group interactions display patterns interpretable in their own terms and comparable in structure to those in other realms of political analysis provides an avenue of potential linkage to the broader bodies of theory on

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which a coherent discipline of political science must rest.

Appendix: Methods of Sampling Client Organizations

We arrived at our sample of client organizations by drawing on four sources of nomination that could measure levels of policy-making activity (Shapiro 1984). Our first method for locating interested actors in a policy domain was especially attentive to issues attracting mass-media attention because of their controversiality and broad popular interest. Such issues are especially likely to appear in the congressional arena. Organizations taking partisan public stands on popular issues are especially likely to be identified by this technique. We conducted a computerized search of stories in national and regional newspapers and magazines dealing with federal policy-making in each domain from January 1977 to June 1982, noting the number of stories mentioning each organization. The data regarding newspaper coverage of organizational participation were compiled from the Information Bank, a data base of the New York Times Information Service (NYTIS). To compensate for regional effects, the data base included the *Chicago Tribune*, *Houston Chronicle*, *Los Angeles Times*, *New York Times*, *San Francisco Chronicle*, *Seattle Times*, *Washington Post*, and *Time* magazine. The product of the computer search was an extensive set of news-story abstracts from the source newspapers. These abstracts were then searched and tagged for the names of organizations, and lists of organizations in each domain were compiled. The number of mentions for each organization was recorded, a mention being defined as an appearance in one newspaper abstract. Duplicate abstracts of stories reported in two or more news sources were eliminated.¹

Second, we searched the abstracts of congressional hearings held by committees and subcommittees with jurisdiction in each of the four domains during the 95th-97th congresses, noting the number of hearings at which organizations testified. Less publicly partisan and more specialized organizations are revealed by this procedure. In light of the enormous number of hearings covered in the Congressional Information Service (CIS) database, we restricted the search to the first sessions of the 95th, 96th, and 97th congresses and to select major committees active in each domain.

Third, during July 1982, we interviewed 20-23 government officials in various policy-making positions in each domain in the Congress and the executive agencies, noting the number of officials mentioning an organization as one that frequently contacted them or which was representative of organizations that contacted them episodically. This method is especially likely to identify organizations having direct dealings and concerns with particular executive agencies and their regulatory initiatives. Two criteria were used in selecting individuals to be interviewed: (1) the position of the individual in the unit and (2) his or her tenure in the unit. An attempt was made to avoid relying exclusively on politically appointed officials or on those with less than two years' tenure in office.

Finally, for each domain we compiled a list of organizations appearing under the industry headings raised to the domain in *Washington Representatives* (1981), an annual publication that canvases various public sources and surveys organizations in an effort to list organizations represented before the federal government. Organizations that are self-professed lobbyists before Congress and the executive agencies are especially likely to be identified.

Each source method of nominating organizations for inclusion in the popula-

tion of claimant organizations has distinctive features that sets it apart from the others (Salisbury 1984). If we had relied only on the listing produced by the *Washington Representatives*, we would have underestimated the population of interested organizations by more than half in each domain. (The percentage of organizations not contained in the *Washington Representatives* directory was 70.4% in agriculture, 52.8% in energy, 67.1% in health, and 80.3% in labor.) Domains differ substantially among themselves with respect to the amount of newspaper attention given them, the raw numbers of organizations identified as active, and the number of organizations engaged in repeated efforts to influence policy outcomes in their respective domains. The energy domain seems to have the largest number of organizations attracted to its concerns, with agriculture and health roughly tied in scale of participation with almost a third fewer organizational participants, and labor placing a rather distant fourth. While the CIS ratios of the number of mentions to the number of organizations mentioned are remarkably stable across the four domains, the NYTIS ratios vary considerably across domains. There is a high ratio in energy, for example, suggesting the relative prevalence of "repeat players" in the domain.

For each domain we drew a random sample of one hundred organizations, with each of the four sources contributing equally, but with each organization in the first three sources weighted by the number of mentions in that source. Because, for all but the listing from *Washington Representatives*, the probability of selection increased with the number of mentions, our sampling procedure reflected each organization's level of activity in the domain. We had strong reasons to adopt this weighting procedure. Knoke and Laumann (1983) had demonstrated that there was a high corre-

lation (.75 and .72 for the energy and health domains, respectively) between the number of organizations active in a domain that named an organization as among the most influential actors in the domain and the number of mentions in newspaper stories, appearances in congressional hearings, and mentions by government officials. Hence, there was solid evidence for treating the number of mentions in the various sources as a measure of perceived organizational influence. The sampling procedure thus generated a list of organizations to be interviewed that disproportionately included the more influential and active organizations in the domain, while also allowing the selection of less influential and active organizations. Simple inspection of the samples reveals that we were quite successful in including many of the most prominent and influential organizations, with an admixture of less visible, more peripheral organizations. This mixture of organizations will permit us to investigate what, if any, systematic differences are to be observed in modes of participation in policy deliberations across different types of organizational actors.

Having sampled the client organizations, we then identified the representatives they employed or retained by conducting telephone interviews with client informants in a minimum of 75 organizations in each domain, 316 organizations in all.² These informants were identified by using published listings of organization officers, supplemented with direct inquiries of the organizations to determine who had operating responsibility for the organization's involvement in federal policy in the appropriate domain. The informant was asked to name up to four individuals inside the organization (employees, officers, member volunteers) and up to four individuals external to the organizations (employed in outside firms, trade associations, and so forth but not falling within the definition of an internal

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representative) who acted as key representatives for the organization in the policy area. It is this listing of nominated individuals that constituted the population of representatives.

The client interviews generated between 400 and 450 names of representatives in each domain, for a total of 1,716 individuals. (Representatives could appear on the lists as many times as they were mentioned; about 5% appeared in more than one domain.) Random selection from these lists produced samples of 257-61 representatives per domain, with a realized sample ranging from 192 to 216 across the four domains. The overall response rate of representatives was 77%. About 10% of the representatives contacted declined interviews; the rest could not be scheduled for various reasons. Slightly over two-thirds of the respondents were based in Washington and were personally interviewed there. Another 15%, based in other major cities, also were interviewed personally. The remainder of the sample was interviewed by telephone using an adapted format.

The final set of actors to be identified were the government officials most often the targets of representational activity in a given domain. These target government officials (including those holding elected, appointed, and career positions) were identified by asking the representatives we sampled to give the names and positions of "the five government officials or staff members you contact most often in the course of your work" in the appropriate policy domain. From these lists we sampled from 101 to 108 names in each domain, and successfully interviewed about 72%. About 8% of the government officials declined to be interviewed.

Notes

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1. For a presentation of the specific data see Nelson et al. 1987.

2. The response rate was 78% with 10% of the organizations refusing interviews. Another 12% of the organizations could not be located, were located overseas, or had ceased to exist. Refusals did not follow any discernible pattern and were not regarded as a significant source of bias.

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NATIONALIZATION AND PARTISAN REALIGNMENT IN CONGRESSIONAL ELECTIONS

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The relationship between nationalization of the U.S. electorate and partisan realignment is explored. Concepts and measurement of nationalization are examined, as well as definitions of electoral change. The British concept of swing is utilized as an appropriate measure of electoral change. Examination of long-term trends in the variances of the congressional vote and swing from 1842 to 1980 shows they are related to the electoral dynamics of realignment. Analysis utilizing a variance-components model shows there has never been a nationalization in terms of configuration of the electorate. But nationalization in the movement of the electorate has taken place cyclically, corresponding to the partisan realignments of the 1890s and the 1930s, rather than monotonically, as suggested by previous research.

One of the major problems of U.S. political science has been to identify the various forces working to shape partisan votes in the electorate and to chronicle their change over time. Among the most important efforts has been Donald Stokes's model, which partitioned the variance of the congressional vote into national, state, and district components (Stokes 1965). Stokes found that since the Civil War era there has been a growing nationalization of the electorate in the United States (Stokes 1967). He attributed this nationalization to the changing structure of mass communication.

During the two decades since its appearance, only two works have directly assessed the adequacy of Stokes's method (Claggett, Flanigan, and Zingale 1984; Katz 1973). Claggett, Flanigan and Zingale questioned the validity of Stokes's conclusion by showing that the estimated "national component," which Stokes did

not present in his paper, has not grown for a century (1984, 79, n. 1 and Fig. 2). Reanalyzing the question, they claimed to use "basically the same methodological approach to the problem" as Stokes (p. 84). However, they changed handling procedures originally set by Stokes, using county-level election returns instead of district-level data. They used regions instead of states as an intermediary aggregate level and extended the period of analysis to include the decades from the 1840s to the 1960s. In this reanalysis they found no evidence of growing nationalization.

That Claggett, Flanigan, and Zingale reached different conclusions from Stokes is intriguing because they felt sure their measure of the national effect was "strictly comparable to the 'national effect' in Stokes's analysis" (p. 84). Yet the results associated with the second measure in their paper are quite different from those associated with the first. (Compare

Stokes's estimated national effect with their national effect in Claggett, Flanigan, and Zingale 1984, Figs. 2, 4.) Claggett, Flanigan, and Zingale did not refer to this discrepancy at any point. In light of such discrepancies, their statement that the "differing conclusions rest on examination of additional and more appropriate data, not on any variations in method that may have been introduced" (p. 89), is not persuasive. It seems that Claggett, Flanigan, and Zingale cast serious doubt on the validity not only of Stokes's argument but of their own as well, for valid methodology applied to similar data should produce similar results.

Not only has some doubt been cast on the reliability of Stokes's conclusions, but his findings do not mesh very well with other literature on the U.S. party systems (see Niemi and Weisberg 1976 for a similar observation). There were two partisan realignments in the period Stokes analyzed: the realignment following the election of 1896 and the realignment of the New Deal era. Yet these political cataclysms do not seem to have affected the constant process of nationalization that Stokes identified. A natural question is, What has been the relationship between the degree of nationalization of the U.S. electorate and partisan realignments?

Electoral analysis of realignment has focused mainly on presidential elections and has tried to identify the breakpoints in partisan support in a series of elections (Key 1955). Therefore, the national partisan vote has been the center of attention. This approach has led to the refinement of critical realignment theory by identifying distinct stages in party systems (for example, see the t-score analysis in Burnham 1970). But there are some problems. To focus on the national partisan vote deflects our attention from the diverse nature of U.S. politics. The national partisan vote is a summary measure of votes cast in various districts within states. Therefore the national "cutting points"

marking transitions between party systems were the outcomes of complicated processes at the subnational levels (see, for example, a study of realignment in New York state in Benson, Silbey, and Field 1978). The nationalization approach Stokes initiated can be useful in analyzing this aspect of realignment.

A recent article by David Brady (1985) suggests one clue for the application of the nationalization approach to the analysis of realignment. In an effort to redirect our attention to a policy-oriented approach to realignment, Brady has proposed a theory of policy change. One of his propositions is of particular interest here because it posits a link between nationalization and realignment. In periods of realignment, he has written, "the election results were more national, less local" (p. 30). In order to test this proposition, he checked the coefficients of variation of the aggregate congressional-vote swings. He found that coefficients of variation tended to be smaller during a realignment period than at other times, indicating that across-the-board movement of the electorate was more prevalent at such times. This analysis builds upon Brady's insight and approach, showing that the degree of nationalization in elections is related to the dynamics of realignment, and presenting a model appropriate to analyzing this relationship.

Concepts of Nationalization

Previous research on the nationalization of the electorate has sometimes been ambiguous conceptually or methodologically about exactly what has been nationalized. For example, Stokes (1965, 1967) was concerned with the political levels at which forces moving the electorate arise. Stokes (1965) initially partitioned the total variance of the *change of voting* into between-state and within-state (district-level) variances (Stokes 1965,

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63–65). Yet when he extended his model to incorporate the national term, he replaced the *change of voting* with the *vote itself*, over a series of elections. Stokes argued as if the transition from the first model to the second were a simple extension of his methodology. Yet it was in fact a transformation of his model and perspectives, for the two variables Stokes employed in the two models are conceptually distinct.

Claggett, Flanigan, and Zingale (1984) clarified the two concepts of nationalization. They identified one type of nationalization as convergence in the levels of partisan support. This concept is related to the partisan vote at each district. If reaction to the national party plays the most important role in an election and the composition of the electorate in most districts is similar, then it is probable that every district will show a similar level of partisan support. The fortune of a party candidate in one district will be closely connected to that of the party's other candidates across the country. Nationalization as convergence refers to the homogeneity of the electorate. I will use the term *configuration* to indicate this attribute of the electorate.

The electorate with a nationalized configuration is one that shows few regional and district differences in partisan support. But one of the important characteristics of U.S. electoral politics is that local factors have always played a significant role. It cannot be hypothesized that the U.S. electorate was truly nationalized at any time in history. The traditional existence of regional difference in partisan support, clearly seen in presidential elections, militates against the existence of a nationalized electorate (Burnham 1981a, 1981b). Moreover, the decline of competition during the last 20 years in districts where congressional incumbents are seeking reelection suggests that there has been, if anything, denationalization in recent decades (Mayhew 1974).

The second concept of nationalization is that of a uniform response to political forces. This concept considers the *movement* of the electorate as distinguished from its basic configuration. Claggett, Flanigan, and Zingale described this aspect of nationalization as a "situation in which all subunits are responding with a uniform surge toward (or away from) a particular party" (1984, p. 82). Thus districts may show concordant movement even though they are not homogeneous in configuration. If there is the same direction and amount of electoral change in every district, then the electorate is perfectly nationalized in movement. Stokes was only concerned with nationalization as a uniform political response, not as a uniform configuration.

Measurement of Electoral Change

In order to assess the degree to which the nation's vote is nationalized in configuration, I will analyze how the partisan vote is dispersed at different levels of aggregation. As to nationalization in the movement of the electorate, one must turn to some indexes of electoral change.

One of the influential definitions of aggregate electoral change in the U.S. electoral-research literature is that of Flanigan and Zingale (1974).¹ They elaborated on types of electoral change, paying attention to the uniformity and the persistence of change. But their basic idea was to define *aggregate electoral change* as "movement away from normal voting patterns" (p. 56). The normal voting pattern was operationalized as the grand mean of the array of elections. It was assumed that "the fluctuations in the actual vote over a period average out to an approximation of the underlying partisanship of the electorate" (Clubb, Flanigan, and Zingale 1980, 89). The vote was considered to move away from the underlying grand mean at each election.

Defining *change* as movement away from the normal voting patterns creates some difficulties. First, the normal vote pattern should be estimated somehow, which is hard to operationalize in electorally turbulent years. The second difficulty arises with the use of this electoral change in the analysis of variance techniques; that is, if the amount of electoral change in one year is measured by the deviation of the vote from its over-time mean, then it follows that the amount of change in the entire set of elections analyzed may be measured by the over-time variation of the vote (see Flanigan and Zingale 1974, 76-77, n. 5). Thus, Claggett, Flanigan, and Zingale (1984) employed the over-time variation of the national mean (normalized election sum of squares) to measure national effect as a uniform response. Stokes's (1965) national component is basically the over-time variance of the national mean vote. I do not think this is an appropriate way to measure electoral change.

Let me illustrate the problem by an example, where a series of elections yields 40%, 40%, 40%, 50%, and 55% for a certain party. The support for this party is the same for the first three elections, and then it increases monotonically. The over-time mean of the vote is 45%. The amount of change in the period is measured by the over-time variation of the vote, that is, 3.5 according to Claggett, Flanigan, and Zingale's method. Consider another series of elections where one party gets 40%, 50%, 40%, 55%, and 40%. The support for this party is quite unstable in the period. A significant portion of the electorate changes partisan allegiance at each election. However, this set of elections has the same amount of over-time variation as that of the previous one, for only the sequence of the values of the vote is different between the two sets. The above examples clearly show that the sequence of elections does not affect the over-time variation of the vote. But

change is a time-related concept. I think that the sequence of elections should be taken into account when measuring change.

I suggest defining *change* as the difference between two electoral results. My preference is grounded both on the simplicity of the definition and the similarity of this definition to the British concept of *swing*. British scholars define *swing* as the average of the Conservative percentage gain and the Labour percentage loss (Butler 1951; see also Steed 1965). If there is no strong third party to be counted, then *swing* will simply be the Conservative gain, for the gain of the one party is the loss of the other. Thus *swing* is equivalent to my definition of electoral change for one party when there is no third party. *Swing* captures net aggregate movement of the vote from one election to a second one. Stokes (1967) and Brady (1985) also utilized *swing* in their analyses. Hereafter, I will use the expressions *my measure of electoral change* and *swing* interchangeably because of their similarity. They mean the partisan vote percentage in each district in one election minus the partisan vote percentage in the previous election.

By using *swing*, one can measure the amount of change for the period in the above examples more appropriately. Two points should be made clear beforehand. First, variance is a measure of dispersion. It measures the degree to which values differ from each other. Thus, the over-time variance of the vote does not measure the amount of change. It rather measures the *configuration* of the vote within the period analyzed, that is, how each election is distinct from others in the period. Second, in order to capture the electoral change for the period, I will look at both the mean and the variance of *swing* over time. The over-time variance of *swing* measures the dispersion of *swing* in the period analyzed.

For the first series of elections in the above example, the *swing* is 0%, 0%,

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Table 1. Variance of the Democratic Vote across Districts

Year	Variance	Number of Cases	Year	Variance	Number of Cases	Year	Variance	Number of Cases
1842	94.4	181	1888	155.1	304	1934	204.1	339
1844	94.8	147	1890	205.4	314	1936	281.7	373
1846	121.0	180	1892	157.8	329	1938	260.9	341
1848	242.9	201	1894	223.2	334	1940	271.1	351
1850	127.8	175	1896	210.5	257	1942	227.7	327
1852	185.6	206	1898	281.8	294	1944	280.8	367
1854	161.1	153	1900	286.5	317	1946	240.9	354
1856	218.0	209	1902	315.9	327	1948	238.0	353
1858	212.6	221	1904	367.1	342	1950	213.4	340
1860	118.4	145	1906	345.0	324	1952	194.5	343
1862	159.3	163	1908	265.5	348	1954	194.9	350
1864	96.4	164	1910	331.6	327	1956	180.5	358
1866	150.7	172	1912	329.8	353	1958	204.9	344
1868	163.0	217	1914	385.0	351	1960	191.0	360
1870	144.6	225	1916	309.4	347	1962	211.6	380
1872	136.6	269	1918	239.0	308	1964	199.2	394
1874	139.0	242	1920	375.3	364	1966	257.2	386
1876	135.5	285	1922	361.9	357	1968	262.4	390
1878	291.9	271	1924	412.3	355	1970	303.5	386
1880	152.1	278	1926	431.6	338	1972	302.8	389
1882	189.0	292	1928	274.2	360	1974	278.4	382
1884	133.2	298	1930	350.3	328	1976	315.1	401
1886	204.8	286	1932	287.4	344	1978	337.0	359
						1980	380.1	369

Note: Districts where Democratic candidates earned more than 98% or less than 2% of the total vote were excluded from the analysis.

10%, and 5%. The over-time mean swing is 3.75%, and the variance of swing is 17.2. The second series has greater swings: 10%, -10%, 15%, and -15%. The over-time mean swing is 0% and the variance is 162.5. The over-time mean indicates that there is an increase in the party support in the first series but not in the second. But the variance of swing indicates there is more change in the second than in the first.

Variance of the Vote across Districts and Variance over the Decade

The first step in my strategy for analyzing configuration and movement in U.S. congressional elections is to show the variances of the Democratic vote and

swing. Table 1 shows the variance of the Democratic vote across districts in each congressional election since 1842. Every contested result available was used for the calculation.³ The variance shows the degree to which there was diversity in partisan support. A small amount of variance means that the nation was relatively homogeneous in the level of support the Democratic party received. The larger the variance, the more diverse the level of partisan support in the districts.

There are two types of change in the variance. First, there is short-term fluctuation lasting only several years. For example, the amounts of variance went up and down slightly through the 1940s. One reason for this fluctuation was the change in the number of contested districts. When there have been many contested districts, variance has tended to be

large; smaller variance has often been associated with a smaller number of contested districts. Generally speaking, midterm elections tend to have smaller numbers of contested districts because congressional aspirants do not expect to benefit from the coattail effects of presidential candidates. Thus the decisions of congressional aspirants seem to affect the amount of variance.

Second, there are long-term trends in the amount of variance. Table 1 shows that variance decreased around the Civil War and then increased, reaching its highest point in the 1920s. Variance declined in the 1930s for the first time since the Civil War. It hit bottom in the 1950s and again increased over the last 20 years.

One might posit that the long-term trend can be explained by changes in the number of contested districts too. For example, the growth in the amount of variance from the 1860s to the 1920s might have been caused by the growth of the nation and the consequent increase in the number of contested districts. But the trend after the 1930s militates against this explanation, for the decrease in the amount of variance did not accompany a decrease in the number of contested districts.

Thus the long-term trend probably really reflects changing diversity in Democratic support across districts and states. There was a trend toward greater variance through the Reconstruction era, and the realignment of the 1890s accelerated it. The regional cleavage that ran across the nation at that time was between the northeast metropolitan industrial area and the South and West. The Northeast was growing progressively more Republican, while the South and West were becoming more Democratic, producing ever higher variance. One should expect the realignment of the 1930s to have had a strong impact on the configuration of the electorate, because to some degree U.S. citizens divided politically along class

lines. This greater class polarization weakened regional bases of political parties, producing smaller variance.

The trend from the 1950s to the present needs special attention, for congressional elections have become increasingly insulated from presidential elections (Burnham 1975b; Mayhew 1974). In his analysis of presidential elections, Kleppner has shown that the variance across regions of Democratic longitudinal means for 1894–1930 was 136.08, while that for 1932–72 was 39.93 (Kleppner 1981, 19, Tbl. 1.2). Moreover, the variance for 1932–60 was 73.59, while variance for 1960–72 was only 10.73. Judging by Kleppner's analysis, presidential elections have become increasingly nationalized in recent decades. In light of this, the growing variance in congressional elections over the past 20 years indicates an insulation of congressional votes from the national political scene.

To further analyze the degree of nationalization in elections over time, I have calculated *decadal variance* to show the configuration in each decade. Decadal variance is calculated by using the partisan vote in each district over five consecutive elections after redistricting. It measures the degree to which the vote is dispersed from the grand mean of the whole decade. This differs from the variance across districts just displayed. That variation is the deviation of the district vote from the national mean in each election year. In decadal variance, the variation stems both from the deviation of the national mean in each election year from the grand mean of the decade and from the deviation of the district vote from the national mean. Thus by using decadal variance I bring the national variation into consideration of configuration. But although it is possible to distinguish national variation from sub-national variation conceptually, it is not so easy to operationalize, since the national partisan vote is the outcome of

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Table 2. Indicators of the Democratic Vote and Swing

Decade	Decadal Variance of the Vote	Range of Variance of Swing	Mean of Variance of Swing	Decadal Variance of Swing
1840s	141.7	156.5	134.2	151.1
1850s	185.8	27.9	115.7	127.9
1860s	147.8	106.5	64.1	74.4
1870s	179.1	110.4	124.2	141.2
1880s	180.3	51.4	98.5	101.3
1890s	242.5	14.6	61.6	87.7
1902-10	326.5	27.6	56.2	67.6
1912-20	335.3	29.0	73.6	87.4
1920s	370.9	52.4	77.8	89.9
1930s	271.1	24.6	46.7	53.4
1940s	250.1	40.4	46.1	67.6
1950s	199.7	15.9	33.8	50.6
1960s	252.1	21.0	65.4	85.0
1970s	326.5	69.2	123.6	137.3

various votes counted at district level. It is simply an aggregated value, and it reflects more than a uniform or "national" element. Thus, variance of the national mean from the grand mean of the decade reflects both the national variation and the subnational variation. In order to measure the amount of national variation, one has to remove the part that reflects the subnational variation.

In Table 2 the decadal variance of the Democratic vote since the 1840s is shown. One can find almost the same secular trend as that shown in Table 1: the increase in variance from the 1860s till the 1920s, followed by the decrease for the next three decades and then the recent increase.

Variance of the Swing across Districts and Variance over the Decade

Brady (1985) calculated the mean swing and the variance of swing to analyze electoral change in prerealignment, realignment, and postrealignment periods. Brady's choice of elections for the calculation of swing seems arbitrary, in that dif-

ferent time periods went into the calculation. In order to avoid this arbitrariness, I decided to use a fixed time period of two years for calculating variance of my measure of electoral change, in the manner of Stokes (1967), and I expanded his analysis to include 1842-1980.

Table 3 shows variance of the Democratic swing across districts from 1844. The values for the first years after each reapportionment are not shown because the change in district boundaries makes the calculation unreliable. Every contested pair of district election results was utilized for the calculation. This procedure is almost the same as the one Stokes (1967) used in his analysis, and I obtained similar results to his for the 1950s.

A pattern is not so easily discernible as it was with variance of the vote itself. But again there are two types of change. One is the short-term fluctuation over a few years' time. The movement of the electorate was in no way uniform across districts, and the degree to which local swing diverged from the national mean swing fluctuated. This fluctuation was the result of various local responses that sometimes coincided by chance to produce a rather

uniform response and thus small variance. But these usually did not amount to a nationally uniform movement toward or away from one party. Examples of this type of fluctuation are abundant in Table 3 where no two adjacent figures are the same.

The second change involved longer time periods. Variances of the swing in one decade fall within some range, and thus the decades show certain levels in the size of variances. From Table 3, I calculated the *range* of values in each decade (see Table 2). In the 1840s, the 1860s, and

the 1870s the range was larger than 100. But in the rest of the decades, the range was rarely larger than 50. Interestingly, one finds smaller range in the 1850s, the 1890s, the 1930s, the 1950s, and the 1960s than in other decades.

Then I calculated a *mean* of the variances of the swing over each decade (see Table 2). The mean hit bottom three times in 140 years. The first was in the 1860s, just after the realignment of the 1850s. The second time was in 1902-10, just after the realignment of 1896. The third was in the period from the 1930s through

Table 3. Variance of the Democratic Swing across Districts

Period	Mean Swing	Variance	Number of Cases	Period	Mean Swing	Variance	Number of Cases
1842-44	.4	61.3	125	1912-14	-.9	78.5	314
1844-46	-3.0	64.2	129	1914-16	1.4	62.2	312
1846-48	-2.7	193.4	151	1916-18	-.5	62.4	266
1848-50	5.0	217.8	154	1918-20	-8.4	91.2	288
1852-54	-4.3	127.7	129	1922-24	-5.6	65.8	324
1854-56	5.6	101.0	139	1924-26	2.4	49.5	316
1856-58	.7	128.9	200	1926-28	-.2	93.8	316
1858-60	-1.1	105.2	140	1928-30	3.6	101.9	308
1862-64	-4.2	57.0	141	1932-34	.3	62.6	294
1864-66	.7	45.3	156	1934-36	.9	43.8	332
1866-68	2.7	23.7	171	1936-38	-5.4	38.0	333
1868-70	1.1	130.2	213	1938-40	1.0	42.4	328
1872-74	5.0	115.8	215	1942-44	2.1	73.5	316
1874-76	-.8	67.0	232	1944-46	-4.9	40.7	337
1876-78	-5.7	177.4	256	1946-48	7.2	36.9	329
1878-80	2.2	136.7	251	1948-50	-2.5	33.1	319
1882-84	.1	120.2	269	1952-54	4.4	27.1	323
1884-86	.6	115.0	264	1954-56	-2.1	27.5	335
1886-88	-.8	90.0	267	1956-58	6.8	37.5	328
1888-90	4.1	68.8	287	1958-60	-2.9	43.0	329
1892-94	-7.9	55.4	317	1962-64	4.6	54.4	361
1894-96	1.7	56.7	252	1964-66	-6.6	65.6	360
1896-98	6.1	70.0	240	1966-68	-.5	66.1	361
1898-1900	-.6	64.2	284	1968-70	3.6	75.5	368
1902-04	-5.5	68.3	311	1972-74	6.1	166.2	354
1904-06	3.2	45.2	302	1974-76	-1.2	97.0	361
1906-08	-.1	41.9	317	1976-78	-1.3	124.7	337
1908-10	2.2	69.5	318	1978-80	-3.8	106.5	335

Note: Districts where there was no competition in either of a pair of elections were excluded from analysis for these years.

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the 1950s, when the New Deal realignment and its aftershocks spread over the country. Students of U.S. party systems will easily arrive at a hypothesis about the relationship between these changes in variance and the electoral dynamics of realignment.

Next I will turn to the decadal variance, as I did in the analysis of configuration of the vote. As mentioned earlier, decadal variance incorporates national variation and subnational variation. If the decadal variance of swing is small, then it is likely that the movement of partisan support is uniform across districts and over a decade, suggesting the existence of nationalization.

Table 2 shows the decadal variance of swing for the Democratic party. It shows similar trends to range and mean of the variance of swing in each decade. The decadal variance of swing hit bottom in the first decade following a realignment (the 1860s, 1902-10, and the 1930s). And the decadal variance was small in the 1940s and the 1950s. With the reservation that decadal variance incorporates both the deviation of district swing from the national mean in each year and the deviation of the national mean from the grand mean of the decade, findings are neatly summarized by decadal variances.

A Model for the Assessment of Electoral Forces

The previous sections introduced decadal variances of the Democratic vote and swing, indicating that decadal variances are closely connected to the dynamics of electoral realignment. I would like to establish a model for the assessment of electoral forces. As explained above, the decadal variance is the mean of squares of the deviations from the grand mean. The vote and my measure of electoral change upon which decadal variance is based have three levels of aggregation:

district, state, and national. It is possible to partition the decadal variance into these components by a nested model of analysis-of-variance technique. By doing so, one may estimate how much of the variance is attributable to national, state, and district components.

My model can be described verbally as follows: *decadal variance = national variance + state variance + district variance*. *National variance* is the variance of the national mean from the grand mean, while *state variance* is variance of the state mean from the national mean, and *district variance* is variance of the district value from the state mean. But because the model is a nested one, means at each higher level of aggregation must be adjusted so that they do not reflect the variation at lower levels. Thus, one can estimate national means and state means only through the model.

The structure of my model resembles those of Stokes (1965) and Claggett, Flanigan, and Zingale (1984). But there are major differences: While Stokes used the difference between the vote percentage and its mean over the decade in each district in his calculation of decadal variance, I use the vote percentage itself to analyze configuration, and swing to analyze movement (see the Appendix for a further discussion and criticism of Stokes's model). Stokes's data corresponds to what Flanigan and Zingale (1974) called *electoral change*, because it is movement away from the normal voting patterns. For the reasons discussed above, I did not use Flanigan and Zingale's definition of *electoral change* in the analysis. Since the decadal variances of the vote and the swing are consistent with the understanding of realignment and electoral-history analysis, it seems appropriate to utilize them as the data for the nationalization analysis. The results indicate that this approach is fruitful.

A reader who is not interested in the statistical detail may skip the following

formal description of the model and proceed to the next section.³

To put the model formally, I first assume that the vote or the swing in the j th district of the i th state in the k th year Y_{ijk} can be described as a sum of four elements:

$$Y_{ijk} = M + A_k + B_{ik} + C_{ijk} \quad (1)$$

In the equation, M is a constant, which is the grand mean $Y_{...}$, and A_k , B_{ik} , and C_{ijk} are national, state, and district effects. It is also assumed that there are J_i districts in the i th state, that I states make up the nation, and that the results of K years are in the analysis. For the sake of explanatory convenience, I assume here that every district is contested in each year analyzed. A_k , B_{ik} , and C_{ijk} are assumed to have a mean of zero. Also, they are assumed to be uncorrelated.

Because the three random variables are independent,

$$\begin{aligned} \text{var}(Y_{ijk}) &= \text{var}(A_k) + \text{var}(B_{ik}) \\ &\quad + \text{var}(C_{ijk}) \end{aligned}$$

The left-hand-side expression is the decadal variance of the vote or the swing. The purpose is to estimate the three variances in the right-hand side of the equation.

By the method of analysis of variance, total SS of Y_{ijk} is partitioned into three SSs in the following way:

$$\begin{aligned} \Sigma_i \Sigma_j \Sigma_k (Y_{ijk} - Y_{...})^2 &= \Sigma_i \Sigma_k (Y_{..k} - Y_{...})^2 \\ &\quad + \Sigma_i \Sigma_k (Y_{i..k} - Y_{..k})^2 \\ &\quad + \Sigma_i \Sigma_k (Y_{ijk} - Y_{i..k})^2 \quad (2) \end{aligned}$$

(A dot in the subscript of Y_{ijk} means that Y_{ijk} is to be averaged over the subscript that is replaced by the dot.)

Degrees of freedom associated with the terms in Equation 2 are $(K\Sigma_i J_i - 1)$, $(K - 1)$, $K(I - 1)$, and $K\Sigma_i (J_i - 1)$, respectively. Thus, the mean squares of

the three effects are described as follows:

$$MSA = \Sigma_i \Sigma_j \Sigma_k (Y_{..k} - Y_{...})^2 / (K - 1) \quad (3)$$

$$MSB = \Sigma_i \Sigma_j \Sigma_k (Y_{i..k} - Y_{..k})^2 / K(I - 1) \quad (4)$$

$$MSC = \Sigma_i \Sigma_j \Sigma_k (Y_{ijk} - Y_{i..k})^2 / K\Sigma_i (J_i - 1) \quad (5)$$

Next, I will express the expectations of the three mean squares by linear combinations of the variances of the random variables. They are obtained by substituting the right-hand-side expressions of Equation 1 for terms in Equations 3-5. For example, $Y_{..k}$ can be written as

$$\begin{aligned} Y_{..k} &= \Sigma_i \Sigma_j (M + A_k + B_{ik} \\ &\quad + C_{ijk}) / \Sigma_i J_i \end{aligned}$$

Hence,

$$\begin{aligned} E(MSA) &= \Sigma_i J_i \text{var}(A_k) \\ &\quad + (\Sigma_i J_i^2 / \Sigma_i J_i) \text{var}(B_{ik}) \\ &\quad + \text{var}(C_{ijk}) \quad (6) \end{aligned}$$

$$\begin{aligned} E(MSB) &= [1/(I - 1)](\Sigma_i J_i \\ &\quad - \Sigma_i J_i^2 / \Sigma_i J_i) \text{var}(B_{ik}) \\ &\quad + \text{var}(C_{ijk}) \quad (7) \end{aligned}$$

$$E(MSC) = \text{var}(C_{ijk}) \quad (8)$$

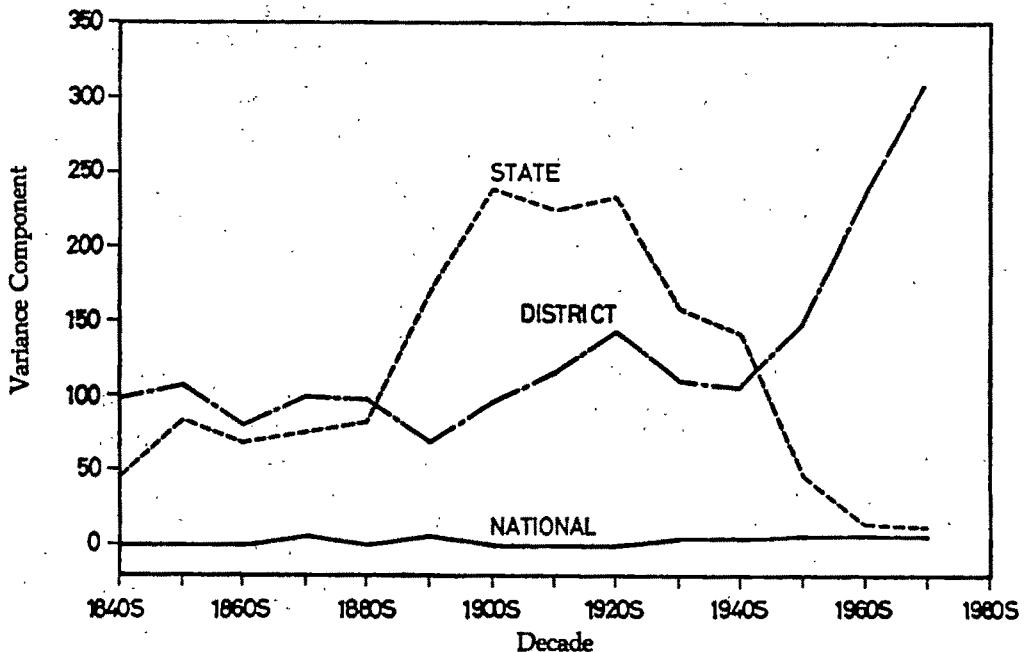
By equating the mean squares obtained by Equations 3-5 to the expected mean squares of Equations 6-8, we get the estimates of the three variances.⁴

Results—Configuration of the Electorate

Figure 1 shows the results of the analysis of the configuration of the Democratic vote from the 1840s to the 1970s. The lines represent the amounts of variance attributable to national, state, and district components. The sum of the three com-

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Figure 1. Configuration of the Democratic Vote



ponents is the decadal variance of the vote.

There has never been a nationalization of the U.S. electorate in terms of convergence. The national component explains very little, if any, of the total variance of any decade. This means that the national mean in one year was not much different from the mean in other years, after adjusting for differences at the district and state levels. But at the sub-national level, the configuration looks quite different. What emerges here is the changing shape of the U.S. electorate during the past century.

The United States had a fairly homogeneous electorate before the Civil War. In this period the total variance of the vote was evenly divided into state and district components. It seems that the Republican

realignment did not much affect the configuration of the electorate. Another analysis was conducted using the data that included uncontested districts, since the effects of a growing number of uncontested districts in this period were anticipated. The results showed an increase in the state component in the 1850s and the 1860s, indicating that the regional difference between the South and the North affected the contestation status of many districts.

The realignment of 1896 led to a further strengthening of the Republican party along a clear regional division: Northeast versus South and West. Figure 1 shows the impact of the realignment as the sharp increase in the state component beginning in the 1890s. A large majority of states ceased to be competitive, with one party

or the other establishing predominance around the turn of the century (Burnham 1981b; David 1972).

The New Deal realignment began to undermine the difference in the partisan vote across states from the 1930s. But it was not until the 1950s that the relative importance of state and district components was reversed. After the New Deal, states that had been predominantly Republican gradually became competitive, although predominantly Democratic states did not change much (David 1972). Beginning with the 1950s, Figure 1 indicates a dramatic increase in the district component along with a decay of states as a meaningful level of aggregation. This was caused by the convergence of party strength among states from the 1950s through the 1970s (Sundquist 1983, 344-48). Erosion of the Democratic predominance in the southern states, illustrated by a growing number of successful Republican candidates in statewide and national elections, was especially noteworthy (Ladd and Hadley 1978, 147).

The increase in the district component reflects another important change in the competitive structure of congressional elections. Mayhew (1974) showed that the shape of the distribution of partisan vote in the districts where incumbents were running changed from unimodality centering around 50% Democratic in the 1950s to bimodality centering around 35% and 65% in the 1970s.⁶ The bimodal distribution produced greater variance than the unimodal distribution. My analysis shows that the district component was virtually the only factor to shape partisan votes in the 1970s.⁶

Results—Movement of the Electorate

Figure 2 displays the results of analysis of the movement of the Democratic vote. This is the decadal variance of the swing,

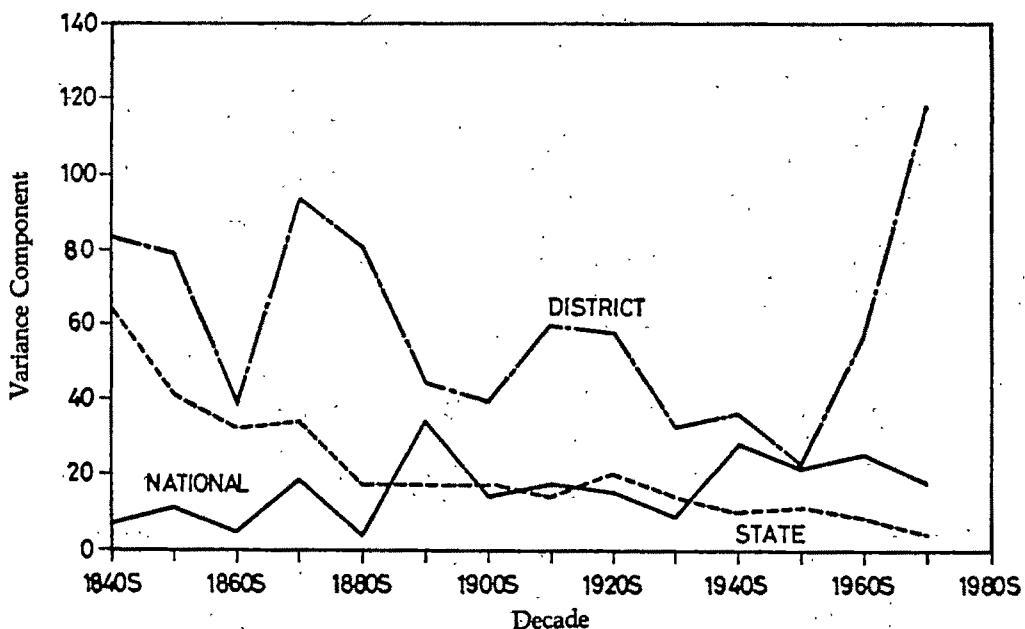
partitioned into national, state, and district components. Some proportion of the decadal variance of swing could be attributed to the national component in every decade. But this proportion increased during critical realignments, as shown by the peaks in the 1890s and the 1940s. As mentioned earlier, the decadal variance of swing was smaller in realignment decades. Thus, during realignments there was less variation in swing across the country, and more of that variation was attributable to the national component. These results validate Brady's proposition that in realignment periods "the election results were more national, less local" (1985, 30). Although the configuration of the electorate was never nationalized, the dynamic forces of realignment brought about a degree of uniform, national swing.

Districts responded very sensitively to realignment. In decades of realignment, the district component of swing shrank to almost half what it was at other times. A small district variance suggests that a state moved as a unit toward or away from a major party during realignment. In some states majority parties were overthrown. Although the state component did not change conspicuously over time, states always played a role in a realignment. For example, during the realignment of the 1860s, more than two-fifths of the variance was attributable to the state component.⁷

Thus I find a cyclical pattern to "nationalization as a uniform response," rather than the monotonic nationalization found in previous research. This suggests a reconsideration of the causes of nationalization. The stimulus promoting nationalization cannot be the changing structure of mass communication, as asserted by Stokes (1967). Instead the occasional movement of the vote uniformly across the nation seems to be considered genuinely political. Voters are mobilized nationally when they receive certain cues

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Figure 2. Movement of the Democratic Vote



from the national political scene. These affect the subsequent fortunes of the major parties. After the heat of a realignment, each congressman once again tries to secure his or her seat by establishing a personal appeal in his or her district. Consequently the district component grows, reflecting the various degrees of success with which congressmen insulate their districts from the national political scene.

The explosive growth in the district component of swing from the 1950s onward poses an interesting question. My analysis of configuration indicated that districts have become more and more diverse in their partisan outlooks. Now the districts show similar diversity in swing. Burnham (1978) has offered an explanation. He argues that incumbents are given "an increasing, personalized protection" (p. 14). In his "incumbent-insulation model," he predicted how the vote would swing in the three types of dis-

tricts: in districts where incumbents have served two terms or more, the swing will parallel the national swing but have little effect on the outcome; in districts where freshmen are up for reelection, strong countercyclical swing will favor those of both parties; and in districts where there is no incumbent, strong convergent swing toward 50% will take place (p. 15). The mixture of these swings naturally leads to the maximum heterogeneity of swing across districts. The increased district component my model has found since the 1950s supports Burnham's model.

Conclusion

I have tried to fill the lacuna between electoral analysis of nationalization and realignment. I hope that these results will provoke some further questions about the nature of U.S. party politics. Particularly,

the results from the 1950s through the 1970s suggest the need of an in-depth analysis of the electoral dynamics currently at work, which may provide an insight into how they will act in the near future.

I have not undertaken an analysis of turnout. But the criticism I have made of previous conceptualizations clearly suggests the unreliability of the conclusion that turnout is highly nationalized (Claggett, Flanigan, and Zingale 1984; Stokes 1965). Further analysis of the relationship between presidential and congressional elections should also be conducted in light of the finding that critical realignments affect the district, state, and national contributions to voting patterns in elections.

The United States is an extremely diverse nation and this diversity must be captured in an appropriate model of electoral behavior. In a system where the basic voting configuration is determined district by district, the movement of the vote has been affected by both local and national influences. The understanding of voting behavior must reflect this fact.

Appendix: Stokes's Variance-Components Model

Stokes's model is described by the following equation:

$$Y_{ijk} = \alpha + \beta_i + \gamma_j + a_k + b_{ik} + c_{jk} \quad (A-1)$$

where Y_{ijk} is the vote or turnout in the j th district of the i th state in the k th year, α is the fixed national effect, β_i is the fixed effect of the i th state, γ_j is the fixed effect of the j th district of the i th state, a_k is the effect of national forces in the k th year, b_{ik} is the effect of forces in the i th state in the k th year, and c_{jk} is the effect of forces in the j th district of the i th state in the k th year (Stokes 1965, 66).

Stokes calls this a nested, mixed model. It is nested, because districts are grouped within states and states are grouped

within the nation. It is mixed, because some effects within the model are fixed, others random. He stipulates the three random effects as follows:

$$a_k = Y_{..k} - Y_{...} \quad (A-2)$$

$$b_{ik} = (Y_{i..k} - Y_{i..}) - (Y_{..k} - Y_{...}) \quad (A-3)$$

$$c_{jk} = (Y_{ijk} - Y_{i..k}) - (Y_{ij..} - Y_{i..}) \quad (A-4)$$

Substituting the terms in Equation A-1 by Equations A-2-A-4, we get

$$Y_{ijk} = \alpha + \beta_i + \gamma_j$$

Thus, the model may be described as

$$Y_{ijk} = Y_{..} + a_k + b_{ik} + c_{jk} \quad (A-5)$$

I am not interested in following Stokes's precise formulation for the calculation of the national, state, and district components. Instead, I would like to point to some implications of Equation A-5. First, Stokes's model as stated in Equation A-5 is quite close to my model. The difference is that while Stokes's model contains the term $Y_{..}$, my model contains M , which is the grand mean of the vote $Y_{...}$. If we put $X_{ijk} = Y_{ijk} - Y_{..}$, then Equation A-5 will be $X_{ijk} = a_k + b_{ik} + c_{jk}$. It follows immediately that $\text{var}(X_{ijk}) = \text{var}(a_k) + \text{var}(b_{ik}) + \text{var}(c_{jk}) + 2[\text{cov}(a_k, b_{ik}) + \text{cov}(a_k, c_{jk}) + \text{cov}(b_{ik}, c_{jk})]$. But because Stokes argues that covariance terms are of negligible importance, $\text{var}(X_{ijk}) = \text{var}(a_k) + \text{var}(b_{ik}) + \text{var}(c_{jk})$. This is exactly the same as my model except that the left-hand-side expression is the variance of X_{ijk} and not that of Y_{ijk} . Thus, the difference between Stokes's model and my model centers on which variance to partition into national, state, and district components. In my model, the variance of the vote or the swing is to be partitioned. In Stokes's model, the variance of $X_{ijk} = Y_{ijk} - Y_{..}$ is to be partitioned. X_{ijk} is the deviation of the vote at a district from the mean over a series of elections. This corresponds to Flanigan and Zingale's (1974)

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definition of electoral change as movement away from normal voting patterns. Therefore the question is whether to use swing or Flanigan and Zingale's measure to estimate variance components of movement in the electorate. My choice is based on the conviction that swing is the better measure of electoral change, while the vote itself is the best indicator of the configuration of the electorate. Thus, the variance-components model serves to measure the two types of nationalization.

Notes

I would like to thank Michael Les Benedict for always encouraging me and commenting on my work.

1. Clubb, Flanigan, and Zingale (1980) utilized this definition of electoral change extensively. Brady (1985) also depends on a technique based on this definition.

2. The data utilized hereafter is Inter-University Consortium for Political and Social Research (ICPSR) study no. 7757, Candidate and Constituency Statistics of Elections in the United States, 1788-1982 supplied by the ICPSR. The ICPSR bears no responsibility for the analysis or interpretations presented here. A few words about handling procedures are in order. Scholars are divided on whether uncontested districts should be included. Therefore, I did another set of analysis using the data that included uncontested districts. The results showed that there was an overall increase in the level of variance, but over-time trend was not much affected.

3. My model, as well as Stokes's, is sometimes called a *variance-components model*, which is referred to as Model 2 by Kendall and Stuart (1966) and as Model 5 by Graybill (1961).

4. I chose to handle the data in a slightly different way from Stokes. First, I chose the *total vote cast* as the basis for calculating percentages and examined the configuration and the movement of both major parties, because one party is not exactly the mirror image of the other. Second, every contested data available in each decade was used for the calculation of the variance components. This differs from previous approach (Katz 1973; Stokes 1965), which excluded one-district states and included districts only where there were contests throughout the decade being analyzed. Thus, in my analysis the number of cases in the cells relating to year and state is different from one year to another. This is called "complete unbalancedness of the data." Most of

analysis of variance programs will handle this kind of data.

5. For arguments related to Mayhew's findings and the behavioral changes of congressmen, see Burnham 1975b; Cain, Ferejohn, and Fiorina 1984; Ferejohn 1977; Fiorina 1977a, 1977b; Hinckley 1981; Jacobson 1983; and Tufte 1973. For arguments from a broader perspective on the transformation of the party system, see Burnham 1975a, Ladd and Hadley 1978; and Sundquist 1983.

6. Configuration of the Republican vote, not shown here, was similar to that of the Democratic vote. However, one point is of interest. There was a relatively large national component in 1912-20. This was probably caused by the Theodore Roosevelt Progressive revolt. In that decade, the national component was 129.41 while the state and district components were 101.81 and 196.20.

7. The results of analysis on the movement of the Republican vote were only slightly different. Thus the movement of the Democratic vote tells most of the story. But there was one major difference. In 1912-20, the state and district components jumped in the Republican analysis, while the district component did and the state component did not increase in the Democratic analysis. Burnham, Clubb, and Flanigan (1978) found a similar increase in an analysis of state averages of absolute values of first differences of the Republican proportion of the vote for congressmen. Their explanation seems pertinent to my result. That is, high scores in the 1912-20 reflect the Bull Moose schism in the party, and they will become smaller by combining vote returns of the old guards with those of the Progressives.

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DEMOCRATIC REPRESENTATION AND PARTISAN BIAS IN CONGRESSIONAL ELECTIONS

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The translation of citizen votes into legislative seats is of central importance in democratic electoral systems. It has been a longstanding concern among scholars in political science and in numerous other disciplines. Throughout this literature, two fundamental tenets of democratic theory, partisan bias and democratic representation, have often been confused. We develop a general statistical model of the relationship between votes and seats and separate these two important concepts theoretically and empirically. In so doing, we also solve several methodological problems with the study of seats, votes, and the cube law. An application to U.S. congressional districts provides estimates of bias and representation for each state and demonstrates the model's utility. Results of this application show distinct types of representation coexisting in U.S. states. Although most states have small partisan biases, there are some with a substantial degree of bias.

The relationship between legislative seats and citizen votes is a longstanding concern in democratic theory (e.g., Balinski and Young 1982; Dahl 1956, 147-49; Farrand 1911; Locke 1965, 419-20; Rae 1967; Schattschneider 1942). Through this relationship, legislative majorities are formed and minorities protected. Constitutionally mandated reapportionment and shifting patterns of partisanship have created opportunities for state legislatures and partisan gerrymanders to alter the congressional seats-votes relationship (Cain 1984; Grofman et al. 1982; Polsby 1971). Over the last century, scholars in political science, sociology, economics, mathematics, statistics, and political geography have studied these normative theoretical questions and sought empirical estimates of bias and unfairness (Hay and Rumley 1984; Ken-

dall and Stuart 1950; March 1957-58; Theil 1970; Tufte 1973). Furthermore, the recent spate of court challenges, the courts' willingness to hear political gerrymandering cases, and the Supreme Court's interest in a threshold of political discrimination, have rekindled the seats-votes controversy (*Karcher v. Daggett*, 462 U.S. 725 [1983]; *Davis v. Bandemer*, 478 U.S. [1986]; *Brown v. Thomson*, 462 U.S. 835 [1983]).

Concern with and, we believe, confusion over two fundamental tenets of democratic theory, partisan bias and democratic representation, dominate this literature. Partisan bias introduces asymmetry into the seats-votes relationship, resulting in an unfair partisan differential in the ability to win legislative seats: the advantaged party will be able to receive a larger number of seats for a fixed number

of votes than will the disadvantaged party. Although *bias* is easily defined, it is not always as apparent or as easily measured (Grofman 1983). Even in the absence of partisan bias, several forms of democratic representation are possible: strict proportional representation—in which the percentage of seats equals the percentage of votes—and winner-take-all elections are the pure forms, with many other possibilities in between.¹ Whereas the extent of bias is a separate problem, the precise effect of partisan bias depends on the specific form of democratic representation.

Much of the literature either treats partisan bias and democratic representation as one concept or mistakenly confuses different democratically legitimate forms of representation with clearly invidious partisan bias. Because bias and representation are related, the separate estimations in most previous research can be shown to be statistically inconsistent. By expressing each of these concepts as a separate parameter in a unified model, we show that it is possible and useful to emphasize the analytical and empirical distinctions by developing a model that jointly estimates both of these parameters. In a single and conceptually simple equation, we incorporate the full range of possible values for bias and representation. The result is a general form that is useful in understanding both the fairness of legislative reapportionment and the democratic character of legislative representation. The validity and utility of this model is then demonstrated with an application to state level congressional seats and votes for the period 1950–84.

The essence of bias and representation in democratic regimes is realized in the translation of votes into seats.² Assume initially that there are only two parties, Democratic and Republican, and that the legislature is composed of a set of single-member, winner-take-all districts. We begin with a few standard definitions. Let

- v = the number of votes cast for Democratic party candidates
 T = the total number of votes cast for candidates of both parties
 v_R = $T - v$ = the number of votes cast for Republican party candidates
 V = v/T = the proportion of votes cast for Democratic candidates
 V_R = $1 - V$ = the proportion of votes cast for Republican candidates
 s = the number of seats allocated to the Democratic party candidates
 s_R = the number of seats allocated to the Republican party candidates
 D = the total number of single-member legislative districts
 S = s/D = the proportion of seats allocated to the Democratic candidates
 S_R = $1 - S$ = the proportion of seats allocated to the Republican candidates

We express the absence of partisan bias as partisan symmetry. In general, this means that in an election system where $x\%$ of the Democratic votes produces an allocation of $y\%$ of the seats to the Democrats, then in another election under the same system $x\%$ of the Republican votes would yield the same $y\%$ Republican allocation of seats. This is the situation that Grofman (1983) calls "completely unbiased."

Stated more formally, if $V = x \Rightarrow S = y$, then $V_R = x \Rightarrow S_R = y$, for all x and y . This completely unbiased system requires only one point at which the percentage of votes equals the percentage of seats: when each party receives 50% of the votes, the seats must be divided equally between them. The partisan fairness expressed by this symmetry does not restrict x to equal y at any but this point. When it is true that $x = y$, for all x and y , we have the situation of unbiased proportional representation. However, there are many other interesting types of unbiased

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representation systems, and our model explicitly incorporates the full range of these.³

Bias, formalized as partisan asymmetry, makes it possible for one party to receive 50% of the votes but not necessarily 50% of the seats. This situation is modeled similarly: If $V = x \Rightarrow S = y$, then $V_R = x \Rightarrow S_R = z$, where y is not necessarily equal to z . However, in the United States, even a biased system would not allocate any seats to a party without any votes. This means that both biased and unbiased systems are restricted to pass through the (0%, 0%) and (100%, 100%) points on the votes-to-seats curve; it is near the middle range of votes and seats that the potential for bias is greatest.

Our model of representation and bias will now be developed and explained in more detail. We introduce the mathematical form and explain the substantive significance of the bias and representation parameters of each. Those preferring a nonmathematical exposition are referred to Figures 1-3.

Modeling Representation

There are a number of plausible functional forms that could be used to model the full range of representation while still restricting the system to be unbiased. Most of these forms lead to nearly identical conclusions, if not to the same models. We believe our model best matches the definitions above and has the additional advantage of being a generalized form of the best-known model of the votes-seats relationship, the "cube law" of electoral politics. Our generalization also extracts the hidden features of this formal "law," known at least since 1909 (see Kendall and Stuart 1950), and expresses them in a more interpretable form. We show how this form can realistically and flexibly model concepts and relationships of fundamental importance to democratic theorists, political scientists, and the courts.

Equation 1, with ρ set equal to 3, is the classic cube law:

$$\left(\frac{S}{1-S} \right) = \left(\frac{V}{1-V} \right)^3 \quad (1)$$

Some time after its inception, investigation with actual election results indicated that values for ρ other than 3 were better descriptions of many electoral systems (see Taagepera 1973; Tuft 1973; and the citations in Grofman 1983, 317). While this mathematical relationship is straightforward, it is difficult to interpret in a theoretically meaningful way without either knowledge of analytical geometry or specific applications.

As an alternative, consider the algebraic characterization that follows. First, by taking natural logs, we rewrite Equation 1 as

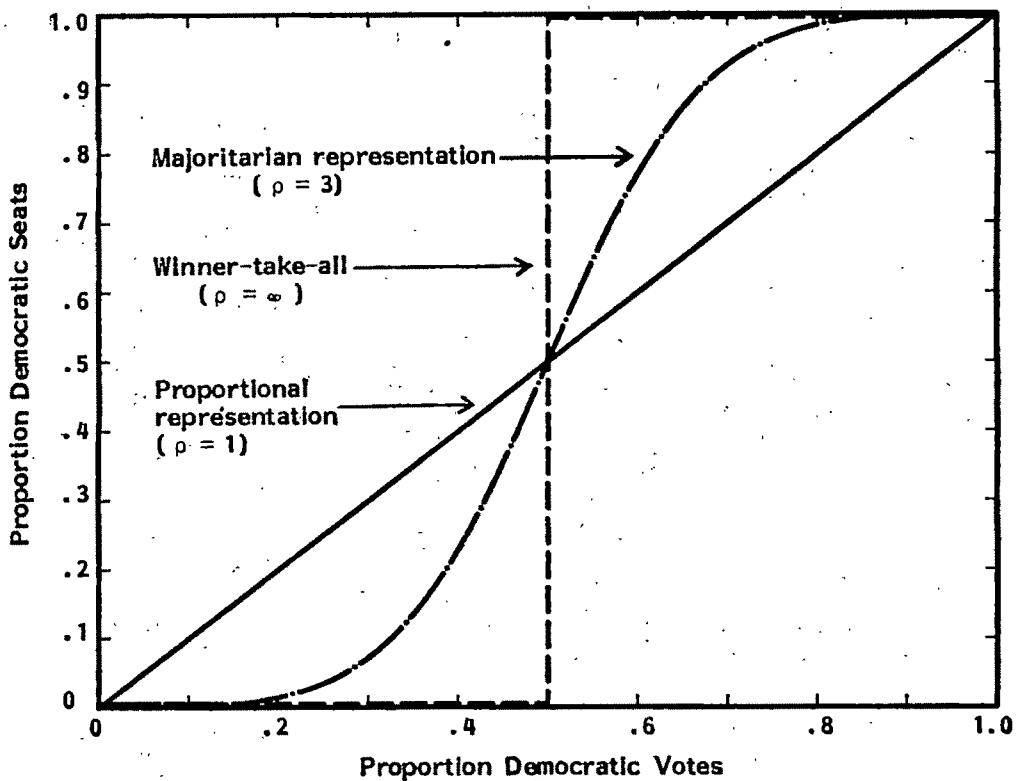
$$\ln \left(\frac{S}{1-S} \right) = \rho \ln \left(\frac{V}{1-V} \right) \quad (2)$$

Tufte (1973, 545), making unrealistic assumptions about the disturbance term (see Linehan and Schrot 1978), estimated ρ by running a linear regression of $\ln[S/(1-S)]$ on $\ln[V/(1-V)]$ and including a constant term in the equation.⁴ With some additional algebraic manipulation, Equation 2 can be expressed as a modification of the dichotomous logit model (see King 1986a). Thus, from the perspective of models common in political science, Equation 3 should be more directly interpretable:

$$s = D \left\{ 1 + \exp \left[-\rho \ln \left(\frac{V}{1-V} \right) \right] \right\}^{-1} \quad (3)$$

There are two differences between Equation 3 and the logit model commonly used to analyze dichotomous dependent variables: $\ln(V/1-V)$ is a log-odds function of V ; and there is no constant term.

Figure 1. Forms of Unbiased Representation (Based on Equation 3)



However, the inverse of the term in braces still ranges between 0 and 1, and, when multiplied by D , the entire right-hand side is restricted to vary between 0 and D , the number of districts. Since a log-odds transformation is the inverse of a logit, we call Equation 3 the *bilogit* functional form. Thus, we have in Equation 3 a model that generates the forms of representation depicted in Figure 1.⁵

Figure 1 demonstrates the range of functional forms that can emerge from Equation 3 and depend on the value of ρ (rho), the representation parameter. We discuss winner-take-all representation ($\rho = \infty$), majoritarian representation ($1 < \rho < \infty$), and proportional representation

($\rho = 1$). Equation 3 can also be used to model antimajoritarian or unresponsive representation ($0 < \rho < 1$), not discussed here.

When $\rho = 1$, the translation of votes into seats is by *proportional representation*. As the figure indicates, some proportion of votes will yield exactly that proportion of seats for the Democratic party. Exact proportional representation is unlikely in actual U.S. district-based elections, if only because there are fewer seats than votes: it would require a proportional increase in seats ($1/T$, to be precise) for even one additional vote; this is impossible, unless there were one seat for each voter. Other more realistic condi-

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tions, such as incumbency and party competition, can also lead to results that are not proportional.

Majoritarian representation is the situation where $1 < \rho < \infty$. The further ρ is from 1, the further the electoral system is from proportional representation. A common example of this is the cube law ($\rho = 3$), portrayed as the curve in Figure 1 in the shape of an escalator. This relationship between seats and votes helps majorities to form. When a party approaches 50% of the votes, each additional increment of voters increases the proportion of seats by a larger amount, as evidenced by the steep slope at $V = .5$; thus, parties are encouraged in their search for majorities. There is also an incentive for more partisan competition, since the marginal benefit, in terms of seats, of an additional increment of votes is greatest as both parties approach 50% of the votes. But one aspect of this relationship has been overlooked: although this form of representation favors majority and near-majority parties, there is a sense in which it protects minorities. This can be seen by looking near the top and bottom of the graph, where the slope of the line becomes progressively flatter. After a party gets 50% of the vote (and a majority of seats), each additional increment of votes yields a smaller incremental proportion of seats. The increase in the proportion of majority-party voters it would take to eliminate the last percentage point of minority seats (i.e., from 99% to 100% for the majority party) is far greater than the increase in voters it would take to reduce minority representation by one percentage point near the middle of the curve (say, from 55% to 56% for the majority party). This majoritarian electoral system thus encourages majorities to form but simultaneously makes it more difficult for minority representation to be eliminated. Although only one type of majoritarian representation is pictured in Figure 1, there are an infinite number of

possibilities—from just beyond proportional representation ($\rho > 1$) to just before winner-take-all ($\rho < \infty$).

A third situation is *winner-take-all*, which occurs when $\rho = \infty$. This is also portrayed in Figure 1. In this case, 50% plus one vote translates into 100% of the seats. Although this situation exists for each congressional district, for example, it does not usually apply to aggregates of them.⁶

Although proportional representation is most often proffered as the standard of fairness, we see no *a priori* reason to believe that one form of representation is inherently more fair than the others, provided that there is partisan symmetry. Convincing arguments can be made in favor of each of these types of democratic representation. At first glance proportional representation seems fair, since the translation process reflects underlying voter preferences most directly. But representation systems need not only reflect to be fair and meaningful (King and Ragsdale 1987; Pitkin 1967). Winner-take-all systems, for example, have some elements of reflection but also recognize that only one party can, and assume that only one party should, govern. These electoral systems thus emphasize ability to govern and reflection in the method of translating citizen votes into legislative seats. In general, there is a trade-off between these two criteria, but since winner-take-all systems do not favor one political party over the other, there is no real reason to consider it unfair. In fact, one can argue that majoritarian representation, falling between proportional and winner-take-all representation, best describes many popular notions of U.S. democracy: majorities are encouraged, but small minorities are protected and thus represented. An "optimal" value of ρ is therefore a matter for political or judicial decision. Thus, there appears to be no *a priori* or axiomatic basis on which to choose one system over another.⁷

Modeling Bias

Although there are many types of "fair" democratic systems of representation, partisan bias is usually condemned. Unfortunately, although partisan bias is often discussed, it is seldom estimated. Indeed, it is not even included as part of the cube law. In order to incorporate the possibility of bias into this model, we must choose a form that still restricts the votes-seats curve to pass through the (0,0) and (1,1) points; this means that a party with no votes will receive no seats. At the same time, this model must still allow for the full range of forms of representation already explicated.

Our solution is to augment Equation 1 (reinterpreted as Equation 3) with a bias parameter; this allows the joint estimation of both partisan bias and democratic representation. Letting β (beta) be the bias parameter, the new generalized cube law can be written as⁸

$$\left(\frac{S}{1-S} \right) = \beta \left(\frac{V}{1-V} \right)^\rho \quad (4)$$

This form allows the curves drawn in Figure 1, from Equation 3, to be asymmetric, our definition of partisan bias.⁹

Almost every empirical study of the cube law has implicitly assumed that $\beta = 1$, the situation of no bias. When there is in fact no bias, the $\beta = 1$ constraint causes no problem. However, when there is bias toward one of the political parties, the constraint will result in statistically inconsistent estimates. The model in Equation 4 allows for bias without the possibility of statistical inconsistency. Tuft (1973) was probably the first to recognize that bias and representation could be modeled in one equation, and his was a linear approximation to Equation 4. We believe our nonlinear model is a more realistic version than Tuft's in that we allow for every possible degree of partisan bias and every possible form of demo-

cratic representation. Unlike the linear model, even systems with widely varying and quite extreme values of S and V can be incorporated in this model.

Equation 4 can also be written in an algebraically equivalent, but more substantively interpretable, bilogit form:

$$s = D \left\{ 1 + \exp \left[-\ln(\beta) - \rho \ln \left(\frac{V}{1-V} \right) \right] \right\}^{-1} \quad (5)$$

We incorporate the bias parameter in this model because of widespread concern about the fairness of the congressional reapportionment process. We therefore provide a more realistic model of both bias and representation, allowing for the exact form of the bias to depend upon the specific type of electoral representation. Consider now what happens when bias is added to the unbiased forms of representation pictured in Figure 1.

Generically, bias refers to asymmetry in the seats-votes relationship for the two parties. The easiest type of bias to understand is for winner-take-all systems ($\rho = \infty$). A bias in favor of the Republicans is the case where the discontinuity in the curve (the vertical part of the line in Fig. 1, $\rho = \infty$) is moved to the right: the proportion of the vote that the Democrats would have to win in order to take all the seats would be greater than half. Similarly, if the discontinuity moved to the left, the bias would help the Democrats. Bias in winner-take-all systems is so apparent that it rarely occurs in U.S. elections, except in the presence of fraud or other abuses.¹⁰ The particular forms of bias associated with the other two types of representation are quite distinct and more complex. The empirical results presented below indicate that bias coexists with many forms of representation in a number of U.S. states.

Table 1 summarizes the possible ranges

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of the bias parameter and their respective interpretations. Since β is log-symmetric (ranging from 0 to ∞ with 1 at the center), it will be convenient to express the coefficient in terms of its natural log, that is $\ln\beta$ (ranging from $-\infty$ to ∞ with 0 at the center).

Figure 2 plots three types of bias under a proportional representation system.¹¹ $\ln\beta = 0$ is obviously the case of no bias: .5 Democratic votes yields .5 Democratic seats; .6 Democratic votes yields .6 Democratic seats. This is proportional representation because the proportion of seats equals the proportion of votes; it is unbiased because these same figures also hold for the Republican party. The 45°

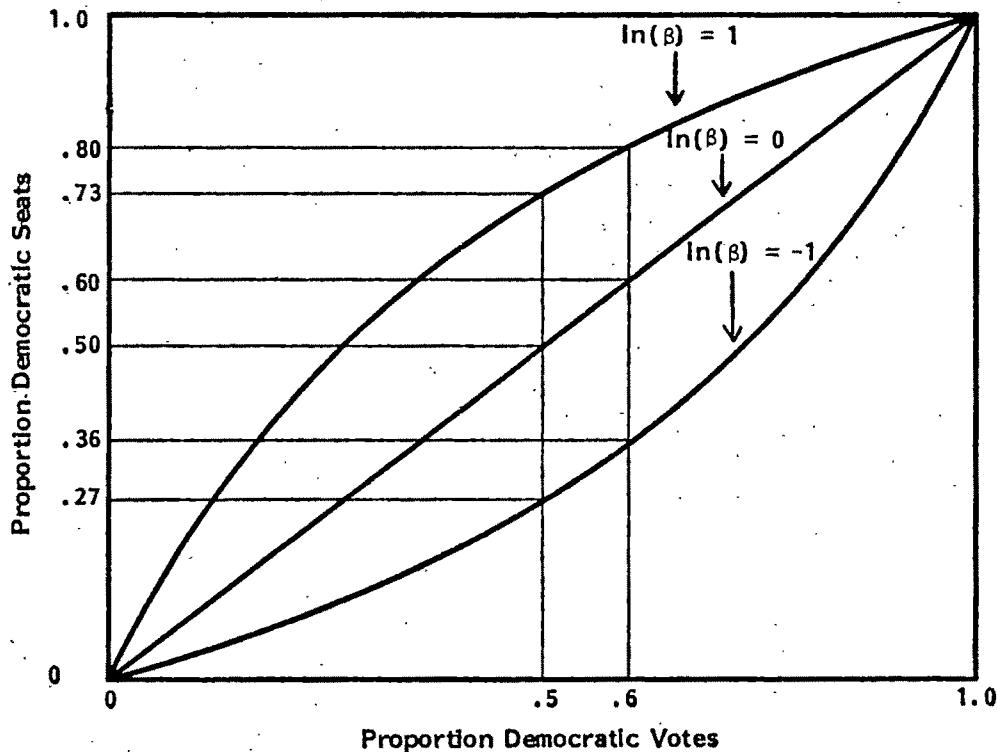
Table 1. Bias Coefficient Values

Coefficient Value	Direction of Bias
$\ln\beta > 0 \Leftrightarrow \beta > 1$	bias towards Democrats
$\ln\beta = 0 \Leftrightarrow \beta = 1$	unbiased
$\ln\beta < 0 \Leftrightarrow \beta < 1$	bias toward Republicans

line embodies this relationship; a given percentage change in seats yields an equal percentage change in votes throughout. In evaluating the other two biased lines, it is useful to consider this unbiased plot as the baseline.

For example, under proportional representation, .5 Democratic votes should

Figure 2. Bias and Proportional Representation (Based on Equation 5)



yield .5 Democratic seats (because this is where the vertical line meets the unbiased line), but the line marked $\ln\beta = -1$ only allocates .27 Democratic seats and the line marked $\ln\beta = 1$ allocates .73. A similar situation exists at all other points on this graph. At .6 Democratic votes, the fair outcome is .6 Democratic seats, but the upper line, $\ln\beta = 1$, is biased toward the Democrats, yielding .8 of the Democratic seats. The lower line, $\ln\beta = -1$, is biased toward the Republicans, yielding only .36 Democratic seats. The asymmetry defines our theoretical notion of bias.

Note that for each point on the horizontal axis, there is a different absolute bias for different parts of any biased line. For $\ln\beta = 1$, the absolute bias is $|.5 - .73| = .23$ at .5 of the Democratic votes, but it is only $|.6 - .8| = .20$ at .6 votes Democratic. In our model, the maximum absolute bias occurs at or near the .5 mark because the curves converge as they approach 0 or 1. This provides some justification for formal statistical models (Quandt 1974) and more intuitive analyses (Tufte 1973) based on bias measured only at .5. However, since there are many electoral systems where the percentage Democratic is rarely near 50%, it also suggests that we should look past this point to incorporate the full range of bias. For proportional representation systems, a measure based on the Gini index of the area between the biased and unbiased curves is possible, but this does not generalize as easily to the majoritarian representation or winner-take-all cases. Grofman's (1975, 1983) "normalized measure" of bias could be utilized here, but for present purposes the most natural way to measure the range of bias existing in the system is to use β or, equivalently, $\ln\beta$.

Figure 3 expresses bias for majoritarian representation systems of the specific type $\rho = 3$. The line in the middle, marked $\ln\beta = 0$, is the unbiased line included for reference. At .6 votes Democratic, the fair proportion of Democratic seats is .77.

This is fair under the $\rho = 3$ majoritarian system, but not necessarily under other representational schemes. For the $\ln\beta = -1$ line, there is a bias favoring the Republicans, so that the same .6 votes Democratic yields only a .55 proportion of Democratic seats. The $\ln\beta = 1$ line, biased toward the Democrats, yields .9 of the Democratic seats. For this figure, the absolute bias is also different for each point on the horizontal axis. For the line marked $\ln\beta = 1$, the absolute bias at .5 votes Democratic is $|.5 - .73| = .23$, but the absolute bias at .6 is only $|.77 - .90| = .13$.¹²

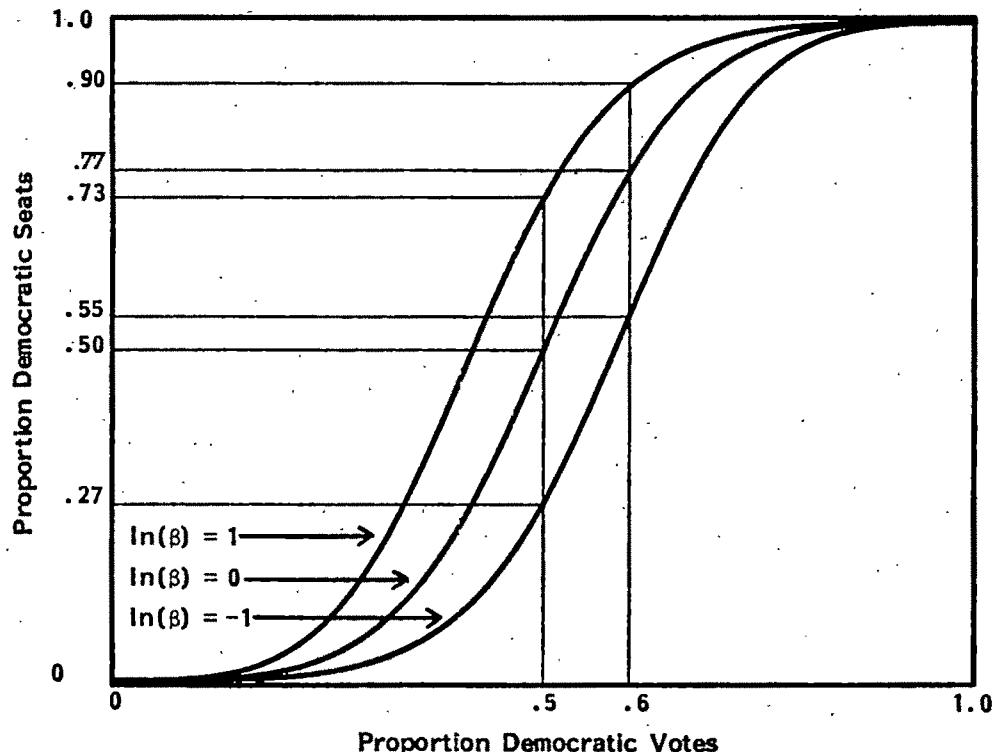
Deterministic Laws and Probabilistic Realities

Some of the most significant contributions to the literature on seats-votes relationships imply that the cube law, or some relevant variant, is deterministic. Whether this relationship is deterministic or probabilistic is an empirical issue, and it has important consequences for theoretical understanding and data analyses. We believe it is difficult to find even one meaningful example of a deterministic law anywhere in the social sciences.¹³ There is also strong evidence that a deterministic relationship between seats and votes does not exist: "While the cube law is stated deterministically, even a cursory examination of election statistics shows that it does not hold perfectly" (Schrodt 1981, 33). Michels's (1911) "Iron Law of Oligarchy" and many others have also failed the test. In making a general point (coincidentally using the cube law as an example), Achen (1982, 15) writes, "Any attempt at specifying exact causal functions must necessarily result in oversimplified explanations."

The Appendix develops a number of original, but somewhat technical, points: we justify a binomial disturbance term and add it to Equation 5, propose a

Representation and Partisan Bias

Figure 3. Bias and Majoritarian Representation (Based on Equation 5; $\rho = 3$)



method of estimation, and provide some empirical estimates. The sections that follow use the results from the Appendix.

Data and Measures

To demonstrate the empirical utility of this model, it is applied to data on U.S. congressional elections, 1950–84.¹⁴ For most analyses of the U.S. Congress, the entire nation has been used. But there are significant findings that demonstrate substantial variation across states (e.g., Scarrow 1983; Tufte 1973). We will therefore conduct separate analyses for each state, using the 18 elections between 1950 and 1984. Alaska and Hawaii were excluded

because they were not states during the entire period. Small changes in the elections included in the analysis did not materially alter the results because of the generally slow changes in state electoral systems.

The implied assumption in this approach is that β and ρ vary more across states than over time within any one state. We believe that this is justifiable on two grounds. First, the whole process of reapportionment is conducted separately by each state legislature. Aside from significant variation in the decisions by state legislatures, there are also large discrepancies in political geography and political culture among the states. Surely, the

variation over time would be less than these cross-state differences. Second, we report large variations in both the degree of bias and the type of representation across states (see Figs. 4 and 5). At the same time, empirical tests indicated substantial stability within states over time.¹⁵ If β and ρ do vary more than expected over time for a particular state, then our estimates are average values for that state.

These data were coded from, and cross-checked against, the *Statistical Abstract of the U.S.* (annual volumes), Congressional Quarterly's (1975) *Guide to U.S. Elections*, Cox's (1972) *State and National Voting, 1910-1970*, and Scammon and McGillivray's *America Votes* (annual volumes). As Niemi and Fett (1986) point out, the data collection is not as straightforward as it might seem. For example, we delete the very few representatives who had won seats under the independent-party label and subtracted the votes received by their Democratic and Republican opponents from the statewide total. We ignore resignations, deaths, and special elections. The member receiving the most votes in the general election was presumed to be elected. At-large districts, used by a few states immediately following reapportionment, are excluded unless there is only one district in the state. Several states have laws that do not require votes to be tabulated in uncontested races. For these states, we included only those districts and votes that were contested and reported. All other districts and votes are included.

Some have argued that the form of democratic representation is a function of the number of districts and voters, the geographical distribution, party competition, and the number of incumbents. Our approach follows two steps. We first estimate the bias and representation parameters for each state using the number of votes and seats for each party and election from 1950 to 1984. Once these parameters have been estimated, we show how differ-

ences in these estimates across states can be explained by measures of state political characteristics. This two-stage approach is as good as a simultaneous estimation because β , ρ , and many of these exogenous state characteristics change considerably more across states than over time.¹⁶

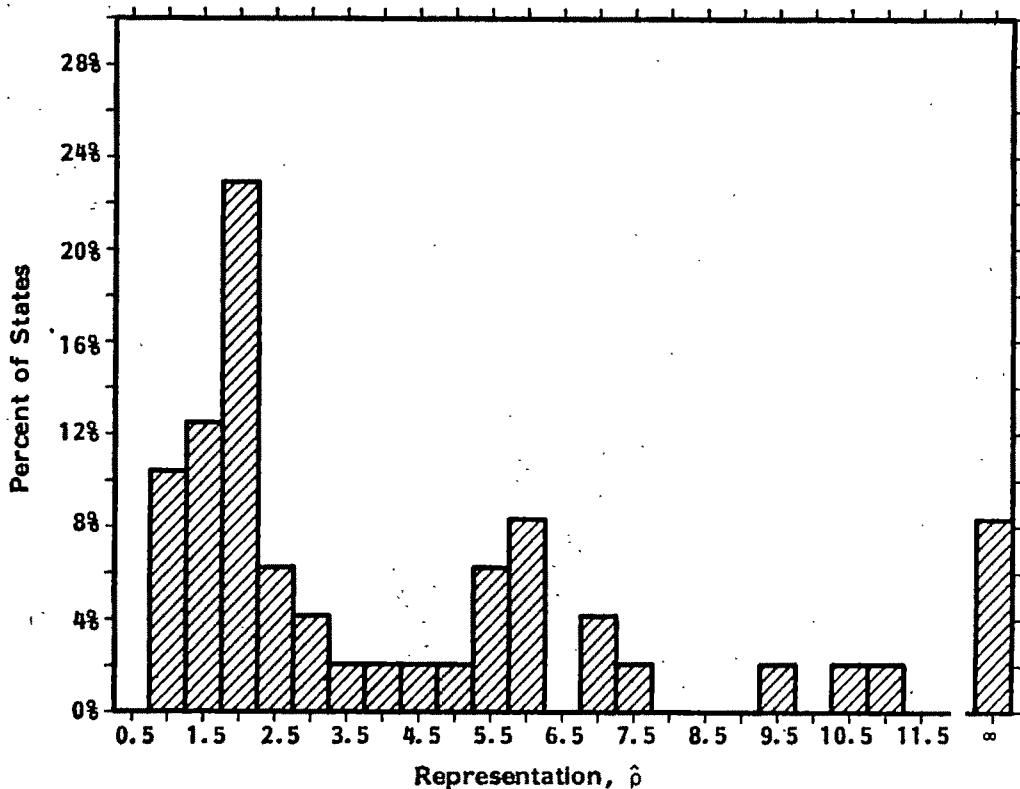
Estimating Representation and Bias in U.S. House Elections

Using Equation 5 as the general model, the estimation procedure described in the Appendix, and the data introduced below, bias and representation coefficients were estimated for each state. We present the results in Table A-1. For easier interpretation, Figure 4 presents a histogram of the representation coefficients.¹⁷ Note that the distribution of these coefficients are trimodal, with modes at or near 1, 6, and 10. Most of the states are quite near to proportional representation, but significant numbers are strongly majoritarian, and several are approximately winner-take-all. Note that although ρ ranges up to infinity, ρ coefficients greater than 8 or 9 are essentially winner-take-all.

This finding demonstrates the utility of the model in three ways. First, it shows the large variance in the type of representation across states. Second, it demonstrates that the plurality of states are just above proportional representation ($\rho = 1$). Although a number of studies have shown that ρ is not equal to 3, the cube-law value, most find that ρ falls between 2 and 4, with an average of about 3. Finally, the results indicate that previous estimation procedures may have been statistically inconsistent due to the exclusion of a bias parameter. The extent of inconsistency in previous research is also quite substantial: although deriving an analytical expression for the inconsistency appears intractable, it is possible to get a feel for the extent of the problem. To

Representation and Partisan Bias

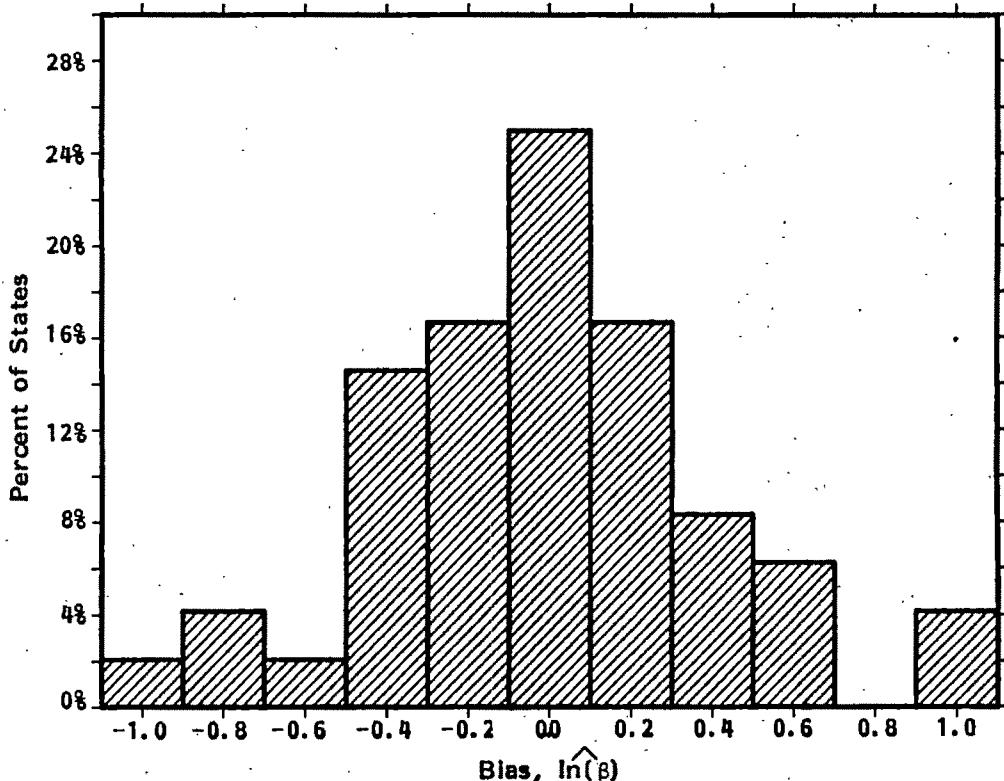
Figure 4. Representation in U.S. States



accomplish this, the state bilogit estimations were also run while constraining $\beta = 1$ [i.e., $\ln\beta = 0$]—exactly as if the bias parameter had not been included. In these constrained estimations, ρ was too large about 1.5 times more often than it was too small. It thus appears that previous research has overestimated the degree to which U.S. democracy tended away from proportional representation and toward majoritarian representation. Although these are the tendencies, in any particular example it is unclear whether the inconsistency will cause the estimate of ρ to be too large or too small; the proper way to provide a consistent estimate of ρ is to use the joint model and estimation method proposed here.¹⁸

There is also substantial variation in the degree and direction of bias in U.S. congressional elections across states. Figure 5 presents a histogram of the estimated β coefficients. Note that the mean is almost exactly 0, and there is an approximately symmetric normal distribution around this point. This implies that the average of the states is not too biased toward one party more than the other. However, not all points fall on or about $\ln\beta = 0$, suggesting that at least some bias exists in individual states. In fact, even allowing for deviations from unbiasedness due to sampling variation and measurement error, the point estimates indicate that some states have quite large biases (see Fig. 5). There are two well-defined groups

Figure 5. Bias in U.S. States



of outliers at the ends of Figure 5. Kansas, Michigan, and Ohio have a substantial Republican bias, whereas Texas, California, and Florida have a substantial Democratic bias.¹⁹

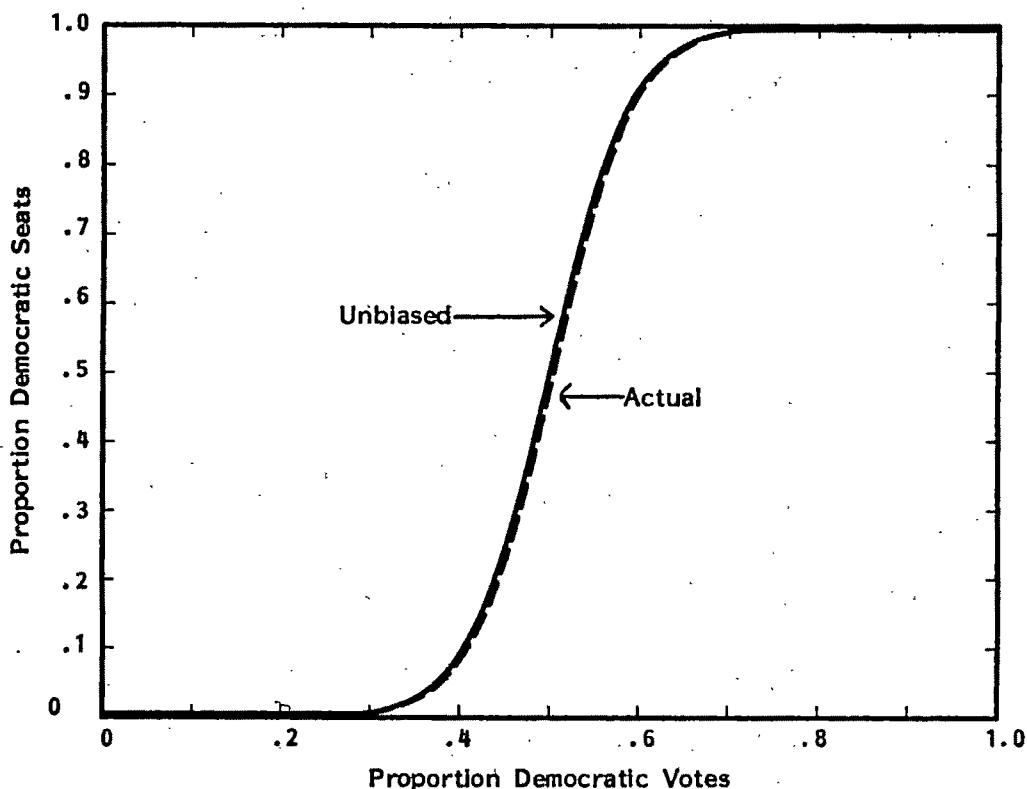
The effect of bias on proportional and majoritarian representation systems is seen in the examples of Indiana and Texas, graphed in Figures 6 and 7, respectively. Indiana is a generally Republican, but still competitive, two-party state. Our results indicate a very slight Republican bias (the unbiased baseline is almost indistinguishable from our empirical findings) and a steep, majoritarian, seats-votes relationship (the unbiased baseline is

a steep curve with two sharp bends). Indiana and other strong party states tend to demonstrate the slight bias and steeper slope portrayed in this figure.

Representation in Texas is quite near to proportional. The no-bias baseline in Texas is the nearly straight-line proportional relationship (see Fig. 7). However, this baseline deviates substantially from Texas electoral politics; the dominant Democratic tradition has created quite severe biases toward the Democrats, permitting them to win a majority of the seats with less than 30% of the votes. This is an extreme example of the biases that do exist in U.S. politics.

Representation and Partisan Bias

Figure 6. Bias and Representation in Indiana ($\beta = 5.70$, $\ln(\hat{\beta}) = -.08$)



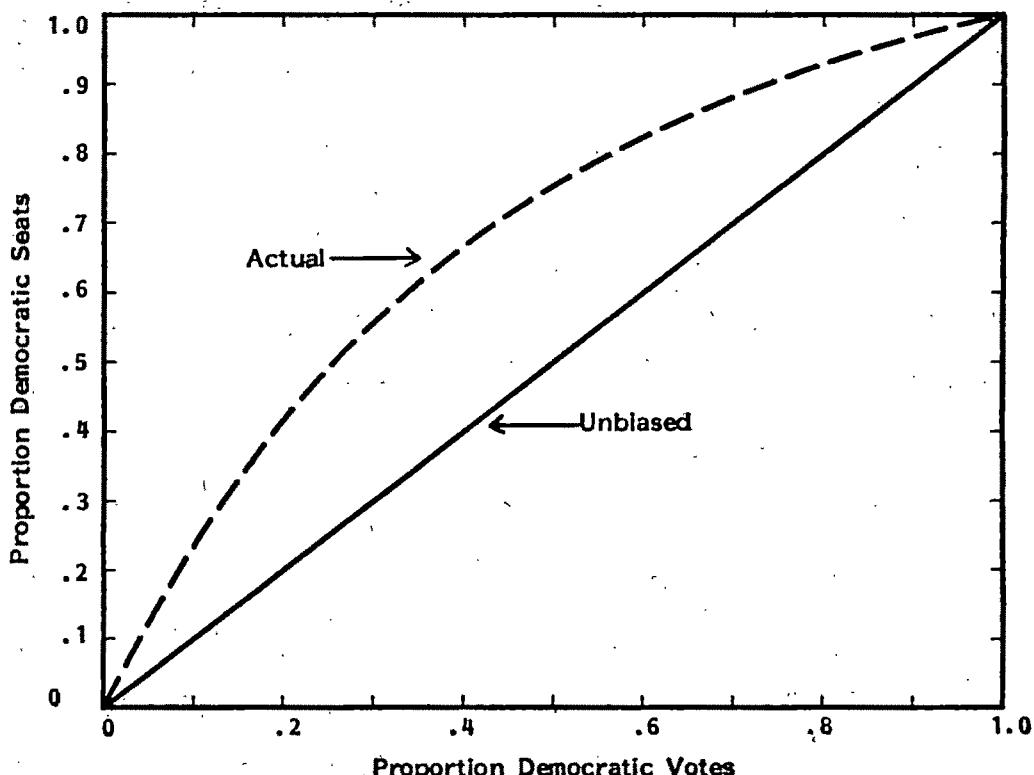
Explaining Representation and Bias in the U.S. House

In this section, we explore the differences among states in the bias and representation coefficients generated previously. This analysis helps to validate the estimates produced there. Leaving out the states with too little data for estimation or with coefficients equal to infinity (indicating winner-take-all representation), two series (bias and representation) of 44 observations each remain. The parameter estimates have different expected values (asymptotically equal to the population parameters) across states. The variation around these expected values is likely to be different for each coefficient, resulting in heteroscedastic disturbances. Fortu-

nately, we can estimate these variances by the approximate squared standard error resulting from the first-stage analysis. Furthermore, since the first-stage analysis was estimated with maximum likelihood, these coefficients will be normally distributed.

Taking all these factors into account, we use a weighted least-squares analysis, regressing the estimates of representation and bias on separate sets of explanatory variables.²⁰ Consider representation first. There are two main explanations for variation in the representation parameter. The best justified is Taagepera's (1973) index, $\ln(T)/\ln(D)$, where T is the total number of voters and D is the number of districts. To understand the logic behind this index, it is useful to focus on the

Figure 7. Bias and Representation in Texas ($\rho = 1.05$, $\ln(\hat{\beta}) = 1.12$)



extreme cases. When there is no bias and one district for each voter (i.e., $T = D$), $x\%$ of the votes for one party will automatically yield $x\%$ of the seats for that party. This proportional representation result is captured by Taagepera's index, since $\ln(T)/\ln(D)$, where $T = D$, is equal to 1. The winner-take-all extreme is also captured by this index: When there is only one district for all the voters, as in a presidential election, we have $\ln(T)/\ln(1) = \ln(T)/0 = \infty$. Thus, the extreme values of this index are theoretically appropriate. Between these two extremes, the index predicts a gradual and continuous increase in the representation parameter—from proportional ($\rho = 1$) to majoritarian ($1 < \rho < \infty$) to winner-take-all ($\rho = \infty$) forms of representation—as the number

of districts increases relative to the number of voters. The precise rate at which ρ increases is defined by the form of his index, $\ln(T)/\ln(D)$. Other forms that also meet the boundary conditions are possible, but Taagepera's (1973) seems plausible. In the weighted-least-squares regression analysis performed here, this hypothesis would be confirmed if the regression coefficient on this index were 1.0 and the coefficients on the other variables in the equation were 0.

Party competition has been suggested as an examination for the form of representation. In strongly competitive party systems, where the percentage voting for each party in each election district is near 50%, a small increase in votes for one party across districts will likely result in a

Representation and Partisan Bias

Table 2. Explaining Representation

Independent Variable	Estimated Coefficient	Standard Error	t-Statistic
Constant	-1.17	.67	—
Taagepera's index	.32	.07	4.62
Party competition	2.17	.70	3.09

Note: Number of observations (states) = 44; standard error of the weighted-least-squares regression = .82; mean of the dependent variable ($\hat{\rho}$) = 1.54.

large increase in seats for that party. Party competition also encompasses the effect of the power of incumbency and uncontested seats. From the widely used Ranney index of state party strength (Ranney 1976), we construct a measure of party competition that ranges from 0 (no competition) to 1 (pure competition).²¹

Table 2 presents the weighted least-squares regression of our estimated representation parameters on Taagepera's index and party competition. The estimates for the effect of both variables are relatively precise and, at conventional levels, significantly different from 0. Taagepera's index has an effect about one-third of what his theory would predict, but it has the correct sign, and helps to explain interstate variation in the form of representation. This finding has several interpretations: a different form of the index might account somewhat better for the range of representation types between proportional and winner-take-all, or there may be a tendency for U.S. states to be more proportional than would be expected solely on the basis of the relative sizes of their voter populations and numbers of legislative districts. Both the value of, and the need for further research on, Taagepera's index are emphasized by this result.

The effect of party competition is shown to be relatively strong: if a state were to move from the lowest to the highest level of partisan competition, the representation parameter would move more than two points toward the winner-take-all extreme (see Table 2). This is well

within the bounds of the original hypothesis.

The propensity of a state to be biased toward the Republicans or Democrats has generally been explained by relative party strength. Parties may be in decline in some ways, but the desire to gerrymander remains as strong as ever.²² We would therefore expect all parties in all states to attempt to gerrymander, but only the states with dominant party systems would be successful.

As a measure of party strength, we use the Ranney index, ranging, as usual, from 0 (Republican) to 1 (Democratic). We also use a measure of state ideological orientation ranging from -1 (liberal) to 1 (conservative), estimated from state level CBS News-New York Times polls by Wright, Erikson, and McIver (1985).

The weighted-least-squares-regression results of estimated bias on state party strength and state ideological orientation are reported in Table 3. Both coefficients are in the hypothesized directions, although the ideology coefficient does not meet conventional significance levels. Our results indicate that the strongest Democratic state has a bias coefficient about .9 points higher (in the direction of Democratic bias) than the strongest Republican state, and that ideologically liberal states tend to be somewhat more biased toward the Democrats.

The plausibility of these second-stage results lends criterion validity to our original model. Variations in our estimates of bias and representation in the U.S. states vary relatively closely with

Table 3. Explaining Partisan Bias

Independent Variable	Estimated Coefficient	Standard Error	t-Statistic
Constant	-.44	.20	—
Party strength	.90	.32	2.82
Ideology	-.67	.59	-1.15

Note: Number of observations (states) = 44; standard error of the weighted-least-squares regression = .64; mean of the dependent variable ($\hat{\beta}$) = -.05.

these plausible criterion variables. Sensitivity analyses indicate that these findings are quite robust to marginally different specifications.²³

Conclusion

In this paper, the venerable cube law of electoral policies was reformulated, generalized, and reinterpreted to provide a model for the relationship between votes and seats in representative democracies. Whereas Taagepera (1986) generalized this law to proportional-representation elections, we generalize it to include both the form of democratic representation and the extent of partisan bias. Previous analyses tended to confuse the form of representation with the degree of partisan bias and have unintentionally constrained the partisan-bias parameter, in effect assuming no bias.

One consequence of this is that unbiasedness has mistakenly been associated with proportional representation. We find that all representation types captured by a single parameter in our model can treat the parties symmetrically using our definition of unbiasedness. Recent court rulings have upheld this view.²⁴

Additionally, by constraining the β parameter, past research has yielded statistically inconsistent estimates of the representation parameter, ρ , ranging between 2 and 4 for most electoral systems. In contrast, we find that the plurality of states were much closer to proportional representation than had

been previously believed. Nevertheless, it is not true that all previous estimations of representation can simply be slightly adjusted toward 1.0. Although the empirical tendency is in this direction, the direction and size of the inconsistency is unknown for any particular example. Thus, some previous analyses may be overestimates and some may be underestimates. Statistical consistency requires that partisan bias be incorporated as an additional parameter. After all, if a party receives, for example, 55% of the votes and 75% of the seats, this may represent a severe partisan bias or a fair system with majoritarian representation. The only way to distinguish between the two situations is to have joint estimates of both β and ρ .

The remarkably flexible bilogit functional form helped to highlight these problems with past research in the context of a theoretically meaningful solution. The model developed herein also demonstrates that different forms of representation are associated with different forms of bias, and that partisan bias can occur in widely varying degrees. This paper has also contributed to the statistical estimation of this inherently nondeterministic relationship between seats and votes. Applying a discrete probability distribution to the discrete variable, s , the number of Democratic seats, is a considerably more statistically efficient procedure than had previously been used. In an application to congressional elections, we demonstrate that there exists a wide variety of representation systems and degrees of

Representation and Partisan Bias

partisan bias. These variations have important implications for national politics and policy (Browning 1986).

Finally, a second stage of the analysis provided some validity to the estimates from the first stage and some evidence that there are systematic explanations for the direction and size of the bias and representation parameters. The level of state party competition and Taagepera's index of state and electorate size helped to explain differences in the representation parameter estimates across states. The Ranney index of party strength and a measure of the state electorate's ideological orientations indicated when and where bias was more likely to occur.

Beyond these contributions, these results also have important implications for the recent court cases on reapportionment. First, the courts must explicitly distinguish between bias and representation type. The discussion of using seats and votes to create indicators of political discrimination in the federal-district-court decision in *Bandemer v. Davis* (603 F. Supp. 1479 [S.D. Ind. 1984]) reveals confusion on that court's part on this issue (see Browning and King n.d.). While it may be desirable for the courts to decide on an acceptable range for the type of representation in U.S. states, partisan bias is a separate issue. The courts would obviously prefer a system with no bias, but this too may not be possible. In such a case, the courts might establish an "acceptable" level of bias. Using this method numerous elections over at least a decade are necessary to make a confident determination about the degree of bias and type of representation. Perhaps in those states with a history of bias, the courts might more closely monitor the reapportionment process, or might even direct a court-ordered reapportionment plan. Regardless, this analysis can provide an understanding of the type of democratic representation and the existence of partisan bias over a historical series of elections for a particular state.

Appendix

We show how our emphasis on probabilistic relationships transforms Equation 5. We also resolve a problem existing in the political methodology literature so that β and ρ may be estimated efficiently and consistently. Estimates for each of the states appear in Table A-1.

Several authors have recently attempted to convert the deterministic cube law in Equation 1 (with $\rho = 3$) to a statistical relationship. The main issue here is how to incorporate a disturbance term (Linehan and Schrot 1978).²⁵ Schrot (1981) provides the most comprehensive analysis of possibilities but is ultimately unable to select any particular method: "In the absence of a theoretical justification for a specific error structure in the cube law, there is no *a priori* reason for choosing one . . . model over the others" (Schrot 1981, 35-36).²⁶

For a solution to this disturbance-term problem, we take a different approach. In reformulating the generalized cube law, we find that it is more politically interesting and more natural to model seats (s) than the odds of seats $S/(1 - S)$. The disturbance term should therefore be formulated in terms of the more fundamental and interpretable variable, s . Thus, we can rewrite Equation 5 to include an additive disturbance term:

$$E(s) = D \left\{ 1 + \exp \left[-\ln \beta - \ln \left(\frac{V}{1 - V} \right) \rho \right] \right\}^{-1} \quad (\text{A-1})$$

or

$$s = D \left\{ 1 + \exp \left[-\ln \beta - \rho \ln \left(\frac{V}{1 - V} \right) \right] \right\}^{-1} + \epsilon \quad (\text{A-2})$$

The expected value operator, $E(\cdot)$, means that the functional form on the right hand side of Equation A-1 (a bilogit function of votes) will correctly predict the number of seats, s , on average over

the long run. We assume in Equation A-2 that $E(\epsilon_i) = 0$. This means that, over the long run, the average error is zero.

We now only need to choose a probability distribution for s (or, equivalently, for ϵ). Recall that s is the number of seats allocated to the Democratic party. It is therefore a nonnegative integer ranging from 0 to D , the number of legislative districts. To incorporate as much information into the distribution as possible, we limit the range of possibilities to discrete probability distributions. The distribution should have the mean as a parameter but should not necessarily be symmetric, since the bounds at 0 and D make symmetry either impossible or implausible for means not equal to $D/2$. Instead, a formulation more faithful to the concept of partisan symmetry is required: the distributions with parameters $(D/2) + \lambda$ and $(D/2) - \lambda$ should be mirror images of one another.²⁷

The binomial distribution meets each of these requirements and is relatively easy to work with. One possible problem is that district outcomes within a state may not be independent, as is assumed by the binomial distribution. Experiments with alternative formulations that allow dependence among districts were performed and were found to add little to the analyses below. This result is consistent with recent research indicating that congressional elections are local, not national, events, fought primarily within individual election districts (Hinckley 1981). The likelihood equation that emerges from this distribution and Equation 5 is also quite similar to what would result if a Poisson or a variety of other distributions were chosen (see King n.d., 1987); indeed, estimates from this model would be consistent even if the distribution were

not binomial but were a member of the family of linear exponential distributions (Gourieroux, Monfort, and Trognon 1984). The results would therefore be quite similar if our assumption were incorrect.

Thus, for the analyses below, s was assumed to be distributed binomially; that is,

$$\Pr(s = k; \theta|D) = [D!/(D - k)!k!] \\ (\theta/D)^k[1 - (\theta/D)]^{D-k} \quad (\text{A-3})$$

where $\theta = E(s)$, from Equation A-1. D , the number of districts, is assumed to be known a priori. ϵ is defined as $\epsilon = s - \theta$. For a maximum likelihood solution, the log-likelihood equation, reduced to sufficient statistics, can be written by substituting Equation A-1 into Equation A-3, taking logs, simplifying, and summing over all observations:

$$\ln(L_t) = \sum_{i=1}^n \{s_i[\ln(D_i) \\ - \ln[1 + \exp(-\ln\beta \\ - \ln(\frac{V}{1-V}\rho))]]) \\ + (D_i - s_i)\ln[1 - [1 + \exp(-\ln\beta \\ - \ln(\frac{V}{1-V}\rho))]^{-1}]\} \quad (\text{A-4})$$

The Berndt et al. (1974) numerical-estimation algorithm, in combination with the positive definite secant update method, was used to maximize this likelihood function to derive the estimates described in the next section. Relatively quick convergence was achieved in nearly all cases.²⁸ The estimates of ρ and β , along with standard errors for each, appear in Table A-1. This table is further discussed and analyzed in the text.

Representation and Partisan Bias

Table A-1. Representation and Bias Coefficient Values

States	$\hat{\rho}$	Standard Error	$\ln(\beta)$	Standard Error
Alabama	1.95	1.14	-.70	.93
Arizona	1.55	1.78	-.46	.56
Arkansas	1.95	1.10	-.42	1.24
California	.93	.57	.16	.09
Colorado	2.08	2.55	.39	.51
Connecticut	6.11	1.83	.08	.33
Delaware	∞	—	.00	—
Florida	1.68	.97	.51	.44
Georgia	1.64	1.73	.46	1.98
Idaho	6.87	2.90	.14	.77
Illinois	1.34	1.60	-.09	.25
Indiana	5.70	2.39	-.08	.32
Iowa	7.07	2.13	-.31	.41
Kansas	2.43	1.68	-.82	.60
Kentucky	1.16	1.95	.51	.67
Louisiana	2.11	1.40	.04	1.81
Maine	11.87	7.65	-.92	1.50
Maryland	2.35	1.96	.02	.94
Massachusetts	1.96	1.04	-.14	.57
Michigan	3.23	1.96	-.47	.32
Minnesota	1.97	4.62	-.17	.51
Mississippi	1.38	.92	-.04	1.48
Missouri	2.62	2.62	.59	.80
Montana	2.11	4.17	.14	1.38
Nebraska	3.98	2.38	-.82	.93
Nevada	∞	—	.00	—
New Hampshire	6.31	5.48	-.13	1.37
New Jersey	2.83	2.37	.11	.25
New Mexico	5.15	2.34	.25	.44
New York	2.06	.98	.06	.14
North Carolina	∞	—	.00	—
North Dakota	7.43	7.88	-.25	2.20
Ohio	2.04	1.65	-.37	.28
Oklahoma	1.17	5.02	.95	1.58
Oregon	4.74	2.17	-.25	.55
Pennsylvania	1.63	1.25	-.03	.17
Rhode Island	11.23	19.11	-.33	3.59
South Carolina	2.20	1.66	-.12	.75
South Dakota	9.72	6.61	-.30	.94
Tennessee	1.25	1.62	.18	.66
Texas	1.05	.79	1.12	.68
Utah	10.60	4.43	.25	.67
Vermont	∞	—	.00	—
Virginia	2.09	1.54	-.21	.32
Washington	5.39	1.11	-.06	.25
West Virginia	5.67	5.06	.26	1.57
Wisconsin	3.52	1.88	-.32	.37
Wyoming	∞	—	.00	—

Notes

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1. The "swing ratio" is related to what we call *representation*. However, the swing ratio—the slope of an estimated linear relationship between seats and votes—is too restrictive to model the full range of nonlinear representational forms.

2. Because most nondemocratic regimes also have voting, the seats-votes relationship is important there as well. However, the mechanism for translation is obviously very different.

3. In *Davis v. Bandemer* (p. 2809), the Supreme Court recognized that previous cases "clearly foreclose any claim that the Constitution requires proportional representation or that legislatures in reapportioning must draw district lines as near as possible to allocating seats to the contending parties in proportion to what their anticipated statewide vote will be."

4. For most of his analyses, Tuft (1973) used a linear approximation to this form because at the time the logistic form was less commonly used and because the lack of extreme points in his data made the approximation relatively good. Since 1973, the logistic model has been considerably more popular; the figures in this paper will also aid in this interpretation. In addition, the data used in the bias section, as well as the data available from other electoral systems, have substantial numbers of extreme data points. Finally, as the figures will show, a linear approximation is not reasonable for many interesting and empirically common forms of bias and representation.

5. The bilogit form is formally undefined when $\rho = \infty$. As a result, it would be technically more appropriate to characterize the winner-take-all situation as $\rho \rightarrow \infty$.

6. Theil (1969, 521) describes a real electoral situation where the system was created to help the minority at the expense of the majority. Although this antimajoritarian representation system is unlikely to occur much in U.S. politics, it can be represented in the current scheme as $\rho < 1$.

7. In an interesting but wholly impractical proposal, Theil (1969, 524) suggests that citizens express a preference for ρ at the same time as they vote for candidates. Preferences for ρ would then be aggregated before translating votes into seats. Although it is not reasonable to "require the teaching of logarithms at an early stage" (1969, 524), this proposal does suggest the importance of the ρ coefficient in

standing for the type of representation in the electoral system.

8. If we used V_R and S_R in place of V and S , respectively, then the bias parameter would be $\beta_R = 1/\beta$. Thus, β is implicitly defined in terms of Democrats.

9. In systems where each party is guaranteed some minimum number of seats regardless of the outcome of the vote, it would be possible to include an additive parameter in Equation 4 to take this into account. However, for congressional elections, this is obviously not relevant.

10. This type of bias occurs, for example, in decisions of legislatures to seat members following closely contested elections. Since the majority party in the legislature can establish its own rules to count disputed ballots, the member seated may not be the candidate with the most votes. In our data, we assume that the candidate with the most votes wins the seat and ignore the bias that occurs if the legislature sat the "loser."

11. These values for β are empirically reasonable in the context of the analysis presented below.

12. Note that this demonstrates that a Gini index measure is inappropriate unless proportional representation is deemed the only fair representation system. If some form of majoritarian representation were considered acceptable, then comparing one of the biased curves in Figure 3 to be the fair proportional curve in Figure 2 will be misleading; indeed, by that standard, even the fair majoritarian curve would be considered biased. It might be useful to generalize the Gini index and derive a measure of bias based on the area between the unbiased ($\beta = 0$ and ρ unconstrained) and the actual bias (β and ρ unconstrained) curves, thus incorporating all the absolute biases. But this is not needed, since $\ln\beta$ contains all of this information.

13. Indeed, many modern-day physicists do not believe that even the physical world is deterministic (see the discussion in Zellner 1984).

14. Niemi and Pett (1986) review several possibilities and conclude that their "historical swing ratio" is better than their "biyearly form." The main reason is that many data points are better than two. Curiously, however, they conclude their article by favoring the "hypothetical (single-year) swing ratio." They give other reasons for preferring this method, but we do not conclude as they do that many data points are better than two, but that one datum is better than many. Our analysis will therefore use many data points collected over time.

15. The data were split into two nine-election-year samples for each state. The two sets each of β and ρ coefficients were each then correlated. Since positive infinity is a possible value for ρ , a simple measure of association was not possible. Omitting these values usually led to correlation coefficients of about .45. More revealing were scatterplots that indicated closer fits between the two time periods

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than any single coefficient could demonstrate. We also split the data to test whether the "one man, one vote" Supreme Court decision had an effect on ρ and β . As one would expect, we concluded that changes in state politics occur slowly. Incumbency, political parties, and geographical and constitutional factors prevent wholesale changes in the structure of representation and bias.

16. One might hypothesize that ρ is a linear function of a vector of explanatory variables, such as state size (X_1) and party competition (X_2): $\rho = \alpha_1 + \alpha_2 X_1 + \alpha_3 + X_2$. It might also be reasonable to hypothesize that β is a function of explanatory variables such as party strength (Z_1) and state ideological orientation (Z_2): $\beta = \gamma_0 + \gamma_1 Z_1 + \gamma_2 Z_2$. The right hand side of these two equations could then be substituted into Equation 5 to derive reduced-form estimates—resulting in a form analogous to interaction effects in regression analysis. This procedure is inapplicable here, however, because β and ρ are assumed to vary only across states. An alternative procedure is used below to express $\hat{\rho}$ and $\hat{\beta}$ as stochastic functions of these explanatory variables.

17. As is apparent from Equation 5, no estimates can be computed when a party received 0 votes for a year or when there is no variation in the number of seats across the years. For this reason, the coefficients of several of the states were not calculated on the full 18 years. The estimates from the following states are therefore not as reliable, since they have one or more years omitted (noted in parentheses): Georgia (1950, 1952, 1958), Louisiana (1950), Missouri (1954, 1956, 1958, 1962), South Carolina (1950, 1958, 1960), and Arkansas (1950, 1954, 1958). This and other causes of imprecision in the estimates are indicated in the standard errors in Table A-1.

18. It is likely that (1) less biased states (i.e., with smaller absolute values of $\ln\beta$) and (2) states where bias and representation vary independently will be closer to consistency than others, but there is no consistent way to know this or to estimate the bias parameter without the full joint estimation presented here.

19. The negative signs indicate bias toward the Republicans; the positive coefficients indicate Democratic bias. Most of the states listed in Table A-1 have estimated ρ and β coefficients within range of what one would expect. The exceptions are mostly those with large standard errors. These substantive points are more directly analyzed in the U.S. House.

20. The residuals from the two regressions correlated at only $-.117$ (t -value = $-.765$). A seemingly unrelated regression would therefore be of no help here.

21. If we let R stand for the raw Ranney index, the measure described as party competition in the text is calculated as $1 - 2|R - .5|$. This is the distance from being in between the parties, normalized

to a 0-1 scale.

22. For example, the architect of the recent and controversial California redistricting plan, late Congressman Phillip Burton (D-San Francisco), "did not deny that the gerrymander was alive and well in California. Burton publicly joked that his zigzagging district lines were 'our contribution to modern art'" (Lowell and Craigie 1985, 246).

23. In particular, no evidence of regional effects could be found: after controlling for the other variables in the equations, the South and other regions were not more likely to be characterized by different levels of bias or representation. There was also no interactive regional effect on the coefficients from the weighted-least-squares results.

24. The plurality in *Davis v. Bandemer* (n. 9) recognized that there was a range of fair seats-votes relationships that are not proportional. Justices White, Brennan, Marshall, and Blackmun's comment that their opinion "is not a preference for proportionality per se but a preference for a level of parity between votes and representation sufficient to ensure that significant minority voices are heard and that majorities are not consigned to minority status is hardly an illegitimate extrapolation from our general majoritarian ethic and the objective of fair and adequate representation recognized in Reynolds v. Sims."

25. It is useful to express relationships statistically even if there is no explicit sampling procedure. One only need conceptualize the dependent variable (Democratic seats, in this case) as a function of systematic (votes Democratic in the bilogit form) and random factors. The random factors are represented by a disturbance term with some known probability distribution.

26. Schrodт finds that three of the five models "have the undesirable property of varying depending on which party is in the numerator and denominator" (1981, 36). (Our model avoids this problem: ρ is invariant and only the sign of $\ln\beta$ changes by using the Republican, instead of Democratic, party for V .) Of the remaining two, he concludes after empirical analyses that one "appears to have little utility" (p. 41). The final model seems best, but it produces the largest standard errors in applications. It also assumes that the disturbance term is log-normal, which may be appropriate at times, but "is not the sort of probability distribution for an error term that one is likely to choose by default" (Linehan and Schrodт 1978).

27. Most probability distributions are eliminated by these axioms. The Poisson distribution is not bounded from above (King n.d.), and the truncated Poisson distribution does not meet the invariant requirement (Johnson and Kotz 1969). The beta-binomial and contagious binomial distributions are possibilities, but the latter makes implausible assumptions about sequential influence structures among congressional districts and the former is elim-

inated on empirical grounds.

28. We estimate $\ln\beta$ instead of β because it is more easily interpreted. Furthermore, because of the invariance property of maximum-likelihood estimation (DeGroot 1975, 291-92), the exponentiation of the estimate of $\ln\beta$ may be calculated in order to recover the maximum likelihood estimate of β .

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CONTROVERSIES

MACHIAVELLI'S PARADOX: TRAPPING OR TEACHING THE PRINCE

*I*n The Discourses Machiavelli extolled the virtues of republican government, yet in The Prince he advised the ruler on how to perpetuate autocratic rule. What accounts for this paradox? Mary Dietz argues that Machiavelli sought to deceive the prince, trapping him into actions that would destroy his rule. John Langton contends, in contrast, that Machiavelli was seeking to teach the prince how to govern so that the autocratic state could evolve into a republic.

*I*n his most substantial work, *The Discourses on the First Ten Books of Titus Livius*, Machiavelli defends the ideals of republican government by arguing on historical grounds that the most stable, solid, and humane states have been founded on social equality, political liberty, rule of law, popular elections, and mixed constitutions embodying a system of checks and balances. Yet in his best known book, *The Prince*, Machiavelli lays out an elaborate, "Machiavellian" program for acquiring and retaining autocratic power and thus seems to encourage a form of practical politics that vitiates the realization of his political values.

Students of political theory have proposed quite a few diverse solutions to this paradox. The prevailing view, it seems clear, is that Machiavelli is actually seeking in *The Prince* to teach an absolute ruler how to use his power to reform a corrupt and feeble state and thereby to lay the foundation for the emergence of a viable republic. In other words, according to the dominant interpretation, Machiavelli regards the absolutism he encourages in *The Prince* as the necessary precondition for the establishment of the kind of republicanism he endorses in *The*

Discourses.

Mary Dietz (1986) rejects this and other available attempts to reconcile the politics of *The Prince* and the values of *The Discourses* as unpersuasive. Instead, she advances the contention that *The Prince* is actually a "political act," "an act of deception," a piece of "duplicitous advice," designed to restore a republic in Florence by tricking a "gullible and vainglorious prince," Lorenzo de Medici, into implementing policies that would "jeopardize his power and bring his demise" (p. 781).

This is a novel and provocative thesis, but after analysis and reflection it strikes me as implausible and misleading. It is predicated on a selective, misguided reading of the relevant texts and an inadequate construal of both Machiavelli's fundamental values and his theoretical intentions. In exposing these flaws in Dietz's argument, I want to suggest that a particular "nationalistic" version of the dominant view of the relationship between *The Prince* and *The Discourses* offers a much more tenable interpretation than the one she devises.

Dietz (p. 782) essentially views *The Prince* as a well-disguised trap. The "text itself provides areas of 'solid ground,' or firm advice a new prince . . . can rely

upon to gain and maintain power." But hidden amid this solid counsel are "ditches and pitfalls" in the form of subversive directives, concealed by "promises of power, glory and popular support." My impression, in contrast, is that *The Prince* is too long, too historical, too involved, indeed altogether too judicious for this to be the case. Putting it another way, why is the solid ground around the putative pitfalls so well prepared—and so extensive?

Machiavelli examines many topics in *The Prince*, particularly in the area of foreign affairs, which have no direct bearing on the problem of maintaining power within a state. And he does so in a rigorous, bureaucratic "issues-and-options" style. Why did he not simply relate the solid counsel in a more straightforward, uncomplicated way? Indeed, if his goal was to bring Lorenzo down, why did Machiavelli not just write a short, snappy, meretricious memorandum on how to govern Florence instead of a densely packed handbook on *realpolitik* (which, by the way, a gullible and vain-glorious prince would probably not peruse)? The answer, I think it is clear, is that Machiavelli's purpose was instruction, not deception. Of course, it can be retorted here that the most effective trap always looks like something else, and this explains all the well-tended "firm ground" around the pitfalls in *The Prince*. But are there really consciously prepared pitfalls in the text? Or, to put the question in a more manageable form, Does *The Prince* contain any advice not consonant with what Machiavelli teaches in *The Discourses*?

Dietz contends that Machiavelli puts four main suggestions in *The Prince* that he hopes will induce Lorenzo to dig his own political grave: (1) that he should reside in the city of Florence itself; (2) that he should strive to gain the favor of the people; (3) that he should not build any fortresses; and (4) that he should create a

civilian militia or mass-based native army. Let us examine each prescription in turn to determine if it has a sinister intent.

Residing in a Conquered City (Florence)

As Dietz points out, according to Machiavelli, if a new prince wants to effectively govern a republic he has seized or conquered, he should either "destroy it" or "reside in it" (pp. 782-83). In *The Discourses* Machiavelli (1950, 183-84),¹ repeats the advice about destroying conquered cities or provinces in order to rule them securely, but he explains here that the destruction of a society in this context means the reorganization of its government, the redistribution of its population, and the restructuring of its stratification system. In other words, Machiavelli is apparently quite sincere when he advises Lorenzo that he has an option: to solidify his rule in Florence he can destroy the city or reside in it. What if Lorenzo had chosen the former option, which Machiavelli himself (1950, 184) describes as "the best means of holding a principality"? Why present an option here, thus giving Lorenzo a chance to follow a path leading away from the pitfall? Indeed, would any of Machiavelli's other alleged pitfalls been effective, if Lorenzo had elected to "destroy" Florence?

But what about the less radical option of residence within the city as a "means of securing possession"? Is this a trap? Dietz construes the suggestion to be a ploy to induce Lorenzo to abandon his country villa and take up residence in the city, where he could be more easily found and destroyed by a vengeful Florentine people still deeply imbued with republican sentiments (p. 783). Now, this argument turns on the assumption that the mass of Florentines had not forgotten "the name of liberty"—that they were, in fact, ardent republicans, just waiting for the

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opportunity to break the shackles of princely rule. Indeed, Dietz's whole thesis rests on the assumption that "the Florentines had become accustomed to a republic," that "in Florence, the idea of liberty was deeply rooted in political tradition" (p. 784). Yet, she herself quotes (p. 785) a passage from Machiavelli's *History of Florence* where he states that "liberty was unknown" in the city. Moreover, Machiavelli contends in *The Discourses* that republican values, traditions, and institutions were never strongly developed in Florence. The city had its "origin in servitude," and then

when afterwards the opportunity occurred for her to gain her liberty in a measure, she began by making a constitution that was a mixture of her old and bad institutions with new ones, and consequently could not be good. And thus she has gone on for the two hundred years of which we have any reliable account, without ever having a government that could really be called a republic. . . . And although Florence repeatedly gave ample authority, by public and free suffrage, to a few of her citizens to reform the government, yet these never organized it for the general good, but always with a view of benefiting their own party, which, instead of establishing order in the city, only tended to increase the disorders. (1950, 239-40)

The specific implication of this passage is that Machiavelli clearly does not see Florence as a hotbed of republicanism and certainly did not write *The Prince* under the illusion that most Florentines were chafing to regain their lost liberty. Indeed, all the evidence of *The Discourses* leads to the conclusion that Machiavelli regarded his countrymen as largely "corrupt" and "effeminate" (1950, 284-85, 369, 491). They were not a dangerous, rebellious mass in his estimation and the actual history of Florence after the fall of the republic in 1512 fully confirms this view.

The broader implication of Machiavelli's assessment of his native city is that it explodes Dietz's claim that the purpose of *The Prince* was to "restore" the previous Florentine republic (pp. 781, 794; see also Machiavelli 1950, 111). Machia-

velli did not think the old republic was worth restoring: its constitution was not good and its citizens, elites as well as common people, generally lacked civic virtue in the sense that their propensity was to put their private interests before the general good. He wanted the city reorganized, reformed, revitalized, and this, he emphasizes, requires the labor of an absolute ruler (1950, 110-11, 138-39, 166-67d, 170-71). But even that was not sufficient, because from Machiavelli's perspective, no Italian city-state, no matter what form of government it possessed, was really a viable geopolitical entity in a world dominated by large absolutist kingdoms. The only viable entity was the entire country of Italy, united under a strong (ultimately republican) government. As Machiavelli observed at one point in *The Discourses*,

A country can never be united and happy, except when it obeys wholly one government, whether a republic or a monarchy, as is the case in France and Spain; and the sole cause why Italy is not in the same condition, and is not governed by either one republic or one sovereign, is the Church. . . . The Church, then, not having been powerful enough to be able to master all Italy, nor having permitted any other power to do so, has been the cause why Italy has never been able to unite under one head, but has always remained under a number of princes and lords, which occasion her so many dissensions and so much weakness that she became a prey not only to the powerful barbarians, but of whoever chose to assail her. (1950, 151-52)

Gaining Popular Support

As Dietz relates, there is probably no more frequently repeated piece of advice in *The Prince* than that "the ruler should always strive to gain the favor of the people" (p. 783). Machiavelli not only reiterates this advice again and again but offers apparently prudent suggestions for implementing it (1950, 64-67, 85). How can practical advice about gaining popular support be a trap?

Dietz's conjecture is that Machiavelli's real view of the matter is just the opposite

of what he preaches in *The Prince*: in contrast to what he writes there, he actually believes that a dictator in Florence could best secure his position by gaining the support of the nobles, while distrusting and, if necessary, repressing the liberty-living people. In other words, by telling Lorenzo through *The Prince* to be hostile to the nobility and to cultivate the favor of the people, Machiavelli was, in Dietz's view, attempting to trick the Florentine dictator into alienating his natural allies and trusting his natural foes. Dietz supports this conjecture in large measure through a strained interpretation of a memorandum written by Machiavelli to Pope Leo X, entitled "On Reforming the State of Florence."

Yet in *The Discourses* Machiavelli (1950, 162) says that he regards "as unfortunate those princes" who try to control a mass of hostile subjects with "extraordinary measures," such as "cruelty." This only erodes their authority. The "best remedy" for popular hostility is not repression but "to try to secure the good will of the people." And this can be achieved by, on the one hand, massacring the nobles and, on the other hand, allowing the people "to live in security," that is, to conduct government according to the rule of law (1950, 162-63). Whom is Machiavelli trying to deceive here? Although he employs a different argument, he repeats essentially the same advice in *The Prince* (1950, 35-36). Are both books, then, duplicitous? I think not. However Machiavellian their teachings may be, both works are sincere efforts to educate and to exhort.

Building Fortresses

In chapter 20 of *The Prince*, Machiavelli proffers some very equivocal advice about the practice of building fortresses. Indeed, the advice is so wishy-washy that it is difficult to imagine that it would persuade anyone to do anything. Yet Dietz

detects a clever trap (pp. 787-88). She thinks this chapter is intended to convince Lorenzo not to build a fortress, because Machiavelli really believes a central citadel would make the dictator less vulnerable to a republican insurrection. But is this, in fact, really Machiavelli's true belief? Dietz herself acknowledges in a footnote that in *The Discourses* Machiavelli unequivocally denounces the practice of erecting fortresses (p. 797). She does not, however, discuss his three main grounds for this advice. First, Machiavelli claims, fortresses are "injurious" because they induce the foolish presumption on a prince's part that he can systematically abuse his subjects and yet control them with force. In the long run this generates tremendous hostility, and no citadel can permanently protect a ruler from an aroused populace. Second, fortresses are "useless" against the artillery of a modern army (1950, 362-68). Finally, and most crucially, fortresses have a debilitating effect on the military capacity of a society: they foster the conviction that a large, well-trained army is unnecessary; whereas, as Machiavelli observes (1950, 369), fortresses cannot preserve states without good armies and they are unnecessary for states with them.

In light of this analysis, I cannot see how Dietz manages to convince herself that Machiavelli's advice on fortresses is a trap. But perhaps he is being duplicitous in *The Discourses* as well when he avers, "A good and wise prince . . . will never build fortresses, so that they may place their reliance upon the good will of their subjects, and not upon the strength of the citadels" (1950, 364).

Creating a Citizen Militia

A new ruler, indeed any wise ruler, Machiavelli strenuously argues in *The Prince*, should always have his subjects armed and trained for combat, and he should never rely on mercenary or auxili-

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ary troops (1950, 44-46, 48, 51-52, 77-78). For Dietz, this piece of advice is perhaps Machiavelli's most treacherous pitfall: "The new prince who arms his subjects may . . . easily make himself a mark for overthrow by creating the very instrument of his own destruction, namely, a civilian militia" (p. 786). Now obviously, arming and training a substantial body of citizens may "facilitate plots, incite insurrection, and inspire rebels." But what leads Dietz to maintain that this is what Machiavelli actually hoped would follow from his advice? She seems to offer two arguments. On the one hand, she intimates that Machiavelli had a high estimation for the republican commitments and revolutionary potential of his countrymen (pp. 786-87); but this, as I have tried to demonstrate, does not appear to be the case. On the other hand, she implies that Machiavelli knew that few successful dictators or princes had actually armed their subjects, and that, in fact, Lorenzo's own grandfather, Lorenzo the Magnificent, had actually disarmed the citizens of Florence in order to secure his dictatorial position. Nothing more graphically reveals Dietz's careless reading of the relevant texts and her misconstrual of Machiavelli's values than this argument.

According to Dietz, "when Machiavelli discusses such virtuous new princes as Francesco Sforza . . . or Cesare Borgia . . . he makes no mention of their having armed their subjects, doubtless because they did not. His bold claim that 'history is full of such examples' [of new princes who armed their subjects] is followed by no examples at all" (p. 786). But in point of fact, Machiavelli cites many examples, including the case of Cesare Borgia himself! As he explicitly points out (1950, 51), Borgia found his mercenary troops "uncertain to handle, unfaithful and dangerous." He therefore "suppressed them and relied on his own men. . . . [Thereafter,] his reputation . . . constantly increased, and he was never so highly esteemed as

when every one saw that he was the sole master of his own forces." Perhaps even more damaging to Dietz's argument, in chapter 21 of *The Prince*, a mere three pages after the point where Machiavelli claims that "history is full of examples" of new sovereigns arming their subjects, he discusses the case of his contemporary, "Ferdinand, King of Aragon, the present King of Spain." As Machiavelli observes (1950, 81-82), "Ferdinand . . . may almost be termed a new prince because from a weak king he has become for fame and glory the first king in Christendom, and if you regard his actions you will find them all very great and some of them extraordinary. . . . He was able with the money from the Church and the people to maintain his armies, and by that long war [with the Moors] to lay the foundations for his military power, which afterwards made him famous." For Machiavelli, this is a particularly instructive example, because Ferdinand did for Spain exactly what Machiavelli exhorts some "redeemer" to do for Italy in the famous last chapter of *The Prince*.

Although it now seems almost superfluous to say it, *The Discourses* is actually full of examples of absolute rulers who created popularly based armies—Tullus, Pelopidas, Epaminondas, the "current King of England" (1950, 175-76). In encouraging Lorenzo to establish a civilian militia, Machiavelli was hardly attempting to deceive and ruin the Florentine dictator; instead he was articulating one of his deepest convictions, namely, that "princes and republics of modern times as have no national troops for defense or attack ought well to be ashamed of it; for . . . if there are no soldiers where there are men, it is not owing to any natural or local defect, but solely to the fault of the prince" (1950, 175).

Dietz tries to make her argument about the duplicitous character of Machiavelli's advice by focusing exclusively on ques-

tions of domestic politics and internal power. But both *The Prince* and *The Discourses* address as well the other dimension of *realpolitik*, international affairs. And on this subject if there is one message which Machiavelli wants to drive home, it is the utter folly of being without a strong, indigenous army in a world filled with violent and voracious states. Again and again in *The Discourses*, Machiavelli describes Florence as "feeble," and she flounders in this execrable condition precisely because, having few troops of her own, she relies heavily on cowardly mercenaries (1950, 212-14, 285, 317, 369, 385, 491). When Lorenzo the Magnificent disarmed the Florentine people, he committed, from Machiavelli's perspective, a heinous political crime. Creating and maintaining a civilian militia poses risks to a prince's regime, but being without such a force guarantees his country's degradation, subjugation, and inevitable obliteration.

Political Values, Theoretical Intentions

Dietz's thesis rests substantially on the assumption that Machiavelli's only deep values are "republicanism and liberty" (p. 794). But Machiavelli was also a passionate nationalist. Indeed, if the last chapter of *The Prince* is a sincere cry for *risorgimento* and not, as Dietz suggests, one more piece of "bait" to trap a vain-glorious Lorenzo de Medici (p. 796), then it seems clear that the national unification, security, and glory of Italy were among Machiavelli's most cherished values. The destruction of Lorenzo and the restoration of a small, feeble, corrupt, and badly organized Florentine republic could do nothing for the achievement of these nationalistic aspirations. The establishment of a viable Italian state could, however, set the stage for the reintroduction and evolution of republican institu-

tions. If this was Machiavelli's view and if he shared it with friends and associates, then it is no wonder that he was not given a governmental position in the (ultimately weak and short-lived) Florentine republic that emerged in 1527 after 15 years of Medici rule. The problem was not so much that Machiavelli had sought a position with Lorenzo but that he had become, in terms of his partisan commitments, an Italian nationalist rather than merely a Florentine republican.

In Machiavelli's estimation, the first step in the process of creating a national state in Italy had to be the emergence of some autocratic leader who possessed the knowledge—and the motivation—to accomplish in Italy what Ferdinand had achieved in Spain.

But this is only one aspect of what Machiavelli wants. In *The Discourses* (1950, 116-17, 129-30, 166), he discloses his conviction that the Roman republic stands as the historical model of what a state should be. What Machiavelli ultimately hopes to foster through his writings is the rebirth in Renaissance Italy of the Roman republic in some modernized form. Viewed from this perspective, *The Prince* and *The Discourses* are easily reconciled. After a ruler had carried out the "moral" and practical program outlined in *The Prince*—that is, after he had secured control over a particular city-state, developed a strong, popularly based army, driven "barbarian" forces from Italy, and extended his control over the country—he could turn to *The Discourses*, perhaps near the end of his reign and under Machiavelli's own personal guidance, to learn how to give his new political creation the republican institutions and culture which would make it stable, long-lived and humane, thus ensuring its greatness and the prince's (and Machiavelli's) ultimate glory (see Machiavelli 1950, 145).

Whether Machiavelli actually believed that Lorenzo de Medici was up to this

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stupendous task, there is no question that he hoped he might be and that he would hire Machiavelli as chief consultant for the project. It is not too farfetched to say that Machiavelli wanted to do for Lorenzo what Count Cavour did for King Victor Emmanuel II in the late 1850s. In sum, Machiavelli was not trying to deceive and ruin Lorenzo; he was trying to educate him about the opportunity for, the demands of, and the glory to be gained from, the unification and political regeneration of Italy. This, of course, is not a novel thesis, but it has the merit of being much more plausible than the argument concocted by Dietz.

In criticizing the kind of reconciliation of *The Prince* and *The Discourses* proposed here, Dietz suggests that Machiavelli was too politically astute to entertain the naive belief that the "heroic politics" of a full-fledged, Machiavellian ruler would "somehow 'give away' to mass politics, that the death of the prince [would] lead to the rise of the republic" (p. 780). But a moment's reflection on what we know about the broad trajectory of European political evolution will show that this view, far from being naive, is incredibly prescient. Absolutism served as the precondition for the establishment of the modern nation-state, which in turn permitted the rise of mass politics. Indeed, this is what eventually happened in Italy. Alas for Machiavelli, he was 350 years ahead of his time.

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Perhaps, as Garret Mattingly once remarked, the puzzle of Machiavelli's *Prince* "has taken up more time and energy than it deserves" (Plumb 1961, 190). Nevertheless, Machiavelli remains a theorist ripe for controversy, and *The Prince* continues to fascinate and invite competing interpretations. In response to

my interpretation of *The Prince* as an act of political deception whose advice is intended to trap and destroy Lorenzo de Medici, John Langton resurrects the staple view of the nineteenth century. *The Prince*, he states, is a cry for national self-determination, and Machiavelli himself is an Italian nationalist, rather than an ardent republican. His familiar interpretation rests upon two general claims: first, that *The Prince* is a sincere and straightforward "handbook on *realpolitik*," written in a "rigorous, bureaucratic, 'issues-and-options' style," and second, that Machiavelli intended the "heroic politics" of the (nationalist) prince would give way (under Machiavelli's guidance) to the republican politics of the *Discourses*.

Since I addressed the second argument at some length in my article and since nothing new has been added to it by Langton, I will not reiterate my criticism of the "from-heroic-to-mass-politics" view here. Suffice it to say that I still find unconvincing the notion that Machiavelli believed that a Medici prince, once secure, would selflessly endow his regime with republican *ordini*, thereby hastening his own political demise. Langton's first claim, however—that *The Prince* contains honest nationalist advice—challenges my interpretation more directly. Let me turn to our particular disagreements over Machiavelli's "advice," after resituating the general problem historically.

Florentine Republicanism

At the center of my interpretation of *The Prince* as an act of political deception stand two key arguments: a historical one, that Florentine republicanism was a living reality when Machiavelli wrote his treatise, and a biographical one, that Machiavelli remained a republican not just from 1498 to 1512 but throughout his life. The latter argument gives him the impetus to plot against the Medici autocrat; the former gives him reason to

believe that the political environment would be hospitable to an overthrow if only the conditions to hasten such an event were in place. Langton casts doubt on both of these arguments by contending that after 1512 Florence was hardly a "hotbed of republicanism" and perhaps not accurately viewed as a city deeply rooted in republican traditions at all. Furthermore, Machiavelli himself "did not think that the old republic was worth restoring"; instead, he thought Florence would be better served by an absolute ruler who would "reorganize, reform, and revitalize" it in order to unify Italy. Thus, on two counts—for the city and for Machiavelli himself—republicanism becomes a dead letter, and "nationalism" rises in its stead.

These are indeed bold claims. As Langton notes, by questioning the republican tradition in Florence and Machiavelli's political preferences, he challenges the very context upon which my interpretation depends. But is he correct?

As we know from the discoveries of a generation of scholars, from Hans Baron and J. R. Hale, to Quentin Skinner and J. G. A. Pocock, Florentine thinkers in the fifteenth century developed a political theory celebrating the republican ideals of liberty, civic equality, and an arms-bearing citizenry (Baron 1961; Hale 1977; Pocock 1975; Skinner 1978). At the very least then, Florence seems to have been a "hotbed" of republican ideas. Moreover, thanks to the labors of recent historians, we know that Florence was also a city of strong civic republican practices—of "substantial and lasting" opposition to the Medici stretching back to their first regime in 1434, and of constitutionalist traditions espousing equality before the law, elections by lot, and freedom of speech (Brucker 1969; Hale 1977; Najemy 1982; Rubinstein 1968; Schevill 1936).

Langton comments upon none of this previous scholarship (not even to reject it). What he does declare is that "the

actual history of Florence after the fall of the republic fully confirms" that the Florentines were disinclined to attempt to regain their liberty. But to what "actual history" does he refer? The newly restored republic of 1527 may indeed have been "weak and short lived," but it was nevertheless born of a *popular rebellion* against the Medici lords, fully in keeping with a tradition of republican fervor and citizen opposition between 1434 and 1458 and again in 1466, 1478, and 1494, the year of Piero de Medici's overthrow. Hale, Rubinstein, and Schevill ably trace the story of popular unrest in the city after the return of Lorenzo. And if the Florentine propensity to revolt requires any further confirmation, we also have Machiavelli's own words. In advising Pope Leo X on "reforming" Florence, Machiavelli emphasizes the volatility of the city after 1512, warning that

the whole general mass of Citizens . . . are never satisfied—and whoever believes otherwise is not wise—unless you restore, or promise to restore to them their Authority . . .

The general mass of Florentine Citizens will never be satisfied except the (Council) Chamber be reopened. . . . Therefore it is the better proceeding that You open it with secure methods and means, and that You take away from whoever was your enemy, the opportunity to reopen it against your will and with the destruction and ruin of your friends. (quoted in Panaini 1969, 635)

The picture Machiavelli paints for Pope Leo reveals a citizenry that cannot cast aside the memory of its ancient liberty. It does not portray, as Langton would have it, a populace immune to the promise of a republic and incapable of reclaiming its republican traditions. What Langton asserts, then, simply flies in the face of historical, textual, and political evidence.

Langton also emphasizes, and with reason, the critical attitude Machiavelli adopted toward his native city as well as his frequent attacks upon its factionalism, its political corruption, and its failure to achieve a stable government akin to the

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one he so admired in ancient Republican Rome and envied in Venice. Certainly no one, least of all Machiavelli, ever claimed that Florence was the pristine image of republicanism; the city was at best characterized by constant fluctuations between autocratic and republican rule. But do Machiavelli's realist appraisals mean, as Langton claims, that he had no interest in restoring the republic and instead welcomed the arrival of a state-building prince? I find no evidence of it.

In the first place, Machiavelli's criticism may be taken at face value—as evidence of his refusal to bow to idealistic visions and his awareness of what the Florentines were up against with regard to their political survival. Without question, as he acknowledges in his *History of Florence* and in reference to the Pazzi conspiracy, there were times in which "liberty was unknown" in Florence. The period of 1478—when the Medicean reaction against the republican revival of 1466 was in full swing—was certainly one such period. Likewise, Machiavelli was, without question, worried about the (in)ability of those cities "born in servitude" to effect successful republican regimes. But nowhere—including in the passage Langton takes as evidence of Machiavelli's disdain for republicanism in Florence—does he say that Florence would be better off governed by an absolute prince with nationalist aspirations. As Hale has observed, neither Machiavelli nor his contemporaries were asking themselves, "Should Florence be governed by a republican constitution or by an absolute prince?" Rather, Machiavelli's concern was with the question, "What qualities should our republic show the outside world, and how can our sick state have its vital tone restored?" (Hale 1961, 181). Thus, Machiavelli does not think, pace Langton, that once destroyed, a republic is best not recovered but rather that once destroyed (or overthrown), a republic faces its greatest difficulty and politics its most impor-

tant challenge—the restoration of its liberty and civic *virtù*.

As he writes of Rome in the *Discourses*, "in a great republic there are constantly evils occurring requiring remedies which must be efficacious in proportion to the importance of the occasion" (1950, 538). Much the same could be said of Florence, a far less glorious republic to be sure, but a republic nonetheless and in need of "efficacious remedies" to restore it to health. This is the issue that Machiavelli, as political theorist, political actor, and Florentine patriot, faces head on with historical creativity in the *Discourses*, with detailed advice about the restoration of republican *ordini* in "Reforming the State of Florence," and with craft and cunning in *The Prince*.

Pitfalls in *The Prince*

The force of Langton's criticism turns upon a general thesis about Machiavelli and Florentine republicanism that lacks biographical, historical, and textual support. I will return to this in closing. But now, what of his specific counters to the "pitfalls" I uncover in *The Prince*, especially regarding the Medici residence, the civilian militia, and the building of fortresses? I am afraid that none of Langton's counters succeed, much less do they require abandoning an interpretation of Machiavelli's pieces of advice as traps for the Medici prince.

On the issue of residence, Machiavelli advises Lorenzo either to destroy the city or reside in it. The first choice, far from leading away from the "pitfall," actually draws the prince toward it. The destruction of Florence, as Machiavelli knows, is an outrageous suggestion and a practical impossibility. It renders the truly dangerous second choice—residing in the city—as the only attractive alternative. Thus, Machiavelli would give Lorenzo the illusion of choice even as he narrows the

prince's range of possibilities. This is how Machiavelli usually conceives of the complex world of choices and deceptions; consider also Ligurio's ploys in *Mandragola*, where he often plays an outrageous choice off against one that seems reasonable to old Nicia but one which will actually compromise him.

As to the danger of residing in the city, we might recall that the history of republican upheavals in Florence was in part one of the people's taking and retaking the Palazzo Medici, which was vulnerable to mass action in a way the Medici villas in the Tuscan hills were not. Perhaps Machiavelli fondly remembered the ouster of Piero in 1494, when a vengeful Florentine populace drove the family from the palazzo, dragged Donatello's *Judith* from the family gardens, and set it up before the palace of the Signoria with a new inscription warning would-be tyrants and praising civic liberty. Such an event—of practical political as well as symbolic importance—could not have occurred had the Medici prince been fortified beyond the city walls. As Machiavelli understood, to oust a prince a people must have not only the spirit but also the opportunity to get at him.

The matter of Machiavelli's advice concerning "whom to arm" is even more important. I argue that in advising Lorenzo in chapter 20 to "keep his subjects armed" and make "partisans" of them, Machiavelli sets still another trap. He lays the groundwork for a new Florentine civilian militia that could contribute mightily to the destruction of the Medici regime. To convince Lorenzo that new princes regularly arm their subjects, Machiavelli appeals to history but he (uncharacteristically) offers no historical examples at all to illustrate the aptness of his advice. Thus, he presents the warrant of history to Lorenzo, but the warrant is in fact a sham, for the advisor knows that the practice he describes as routine for new princes is rare.

Langton, however, claims that Machiavelli offers at least two examples (in chaps. 5 and 21) of new princes who armed their subjects. In Cesare Borgia and Ferdinand of Spain, Machiavelli allegedly provides models whom Lorenzo can use for the course of action outlined in chapter 20. Leaving aside the obvious question—if Borgia and Ferdinand were such good examples, why did Machiavelli not mention them in Chapter 20?—let us turn instead to a more vital question: are Borgia and Ferdinand, in fact, examples of new princes who "always had their subjects armed?" On this score, I would suggest that it is Langton who advances the "careless reading." His misreading hinges on what should be the obvious difference between a "civilian militia" of the kind Machiavelli recommends to Lorenzo, and a "private army" or "national troops" which he credits Borgia and Ferdinand, respectively, with establishing. To put this otherwise, Machiavelli surely appreciates that in the Romagna (the greatest source of mercenaries) Borgia relies upon "his own men" and that in Spain Ferdinand taxed the people to build up his military might. As a result, neither was beholden to foreign soldiers. But nowhere does Machiavelli equate their actions with the creation of an *arms-bearing citizenry*. And it is the latter that he counsels Lorenzo to create in chapter 20, when in essence he advises the rearming of a formerly republican city. Thus, the examples of Borgia and Ferdinand are neither apt nor relevant in this context. In fact, the example of Borgia could be counterproductive, so Machiavelli does not mention him, just as he does not mention *il Magnifico*, Lorenzo's grandfather, who disarmed the Florentines and was the most successful of all the Medici lords.

These examples are important. Nevertheless, I think that by seizing upon them, Langton ultimately dodges the most important question of all. Regardless of whether Borgia or Ferdinand fit the bill, is

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Machiavelli's advice on arming one's subjects helpful counsel for a Medici in Florence? Langton himself concedes that "creating and maintaining a civilian militia poses risks to a prince's regime." Precisely so! But in admitting as much he simply begs the crucial question; he does not resolve it, nor does he persuasively challenge the contextual and historical evidence I present in order to reveal Machiavelli's deception.

To counter my interpretation of Machiavelli's advice on fortresses, Langton turns to the *Discourses*, and notes that there, too, Machiavelli is chary of fortress building and urges that the prince rely instead upon the good will of his subjects. He seems to imply that on matters where there is no contradiction between *The Prince* and Machiavelli's other writings (especially the *Discourses*) there must be no deception at work in *The Prince*. I see no reason to accept such an interpretation, unless one assumes (as I do not) that everything Machiavelli expresses elsewhere can be read as the "truths" that expose the "lies" of *The Prince*. If, however, this is Langton's presumption, he needs to be more consistent in his application of it, and acknowledge not only those passages that seem to point to a sameness in Machiavelli's advice, but also account for those passages where striking contradictions appear. He makes no attempt, for example, to explain the advice against fortress building in *The Prince* in terms of Machiavelli's letter to Guicciardini, where Machiavelli equates the successful Medicean conquest of Florence precisely with the building of a *fortezza*. Nor—to take another example of importance in my essay but unacknowledged by Langton—does he confront the counsel against liberality in *The Prince* with Machiavelli's treatment of it in the *History of Florence* as a valuable Medicean tactic to maintain power. Nor does Langton try to explain how Machiavelli's dictate to Lorenzo to "build upon the people" and be wary of

the nobility squares with his straightforward comment to Pope Leo that a prince in Florence "despoiled of Nobility cannot sustain the burden of the Principality," and thus must create a "middle group" between himself and the general public (Pansini 1969, 620). By simply asserting that the latter is a "strained interpretation," Langton again sidesteps the intriguing and difficult issues. He certainly offers no explanation for them.

Pitfalls aside, Langton's interpretation fails finally to confront the methodological premise behind my reading of *The Prince*—that genuinely historical study is the indispensable precondition for interpreting political texts of the past. Other than a few (unsubstantiated) assertions about Florentine republicanism, Langton offers no historical reading of Machiavelli's treatise and uses descriptions the author could not in principle have accepted as his. So, for example, we are given the nineteenth-century language of "nationalist aspirations" or the twentieth-century conception of a "viable geopolitical entity." Furthermore, Langton would have us accept an ahistorical description of *The Prince* and the *Discourses* as works of political literature. As noted, he describes the former as written in a "bureaucratic 'issues-and-options' style," and rhetorically he asks why, if my interpretation is correct, did Machiavelli not simply write a "short, snappy, merciful memorandum?" But these are anachronisms. The bureaucratic memorandum and the "white paper" were not literary options in the fifteenth century, and within the existing genre of the Mirror of Princes tracts *The Prince* is remarkably short and snappy.

In his conclusion, Langton shifts from an ahistorical to a suprahistorical interpretation of Machiavelli's intentions. He praises the Florentine republican for having the prescience to anticipate nothing less than "the political evolution" of Europe down through the nineteenth cen-

tury. Among its lesser faults, this credits Machiavelli with a vision of political creation as a linear progress, a view he did not hold nor could have held. To the contrary, his own clearly expressed vision of the cyclical movement of glory, decay, and regeneration, or *anacyclosis*, has more to do with the *revolution* of order and disorder than with the *evolution* of "mass" from "absolutist" politics (D'Amico 1984, 132). Even more troubling, however, is Langton's evident willingness to subscribe to an overly simplified view of European history—only to reconstitute Machiavelli's contribution both to it and to political thought more generally as a grand moment in the telos of the modern nation-state. Writing history backwards, Langton would have us understand Machiavelli as "350 years ahead of his time"! Commentators may well take issue with this or that interpretation of *The Prince*, including the "politics of deception" I find there. But I would like to suggest that we can make headway in our controversies over the meaning of Machiavelli's little treatise only if we return it to the period in which it was written and examine Machiavelli's intentions within his own time, not ahead of it.

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VALUE CHANGE IN INDUSTRIAL SOCIETIES

Ronald Inglehart has argued that, while most of the major political parties in Western countries tend to be aligned along a social class-based axis, support for new political movements and new political parties largely reflects the tension between materialist and postmaterialist goals and values. This has presented something of a dilemma to the traditional parties, and helps account for the decline of social-class voting. Scott Flanagan takes issue with Inglehart's interpretation in several particulars. Although their views converge in many respects, Flanagan urges conceptual reorientations and adumbrates a different interpretation of post-World War II political development in Europe and Japan.

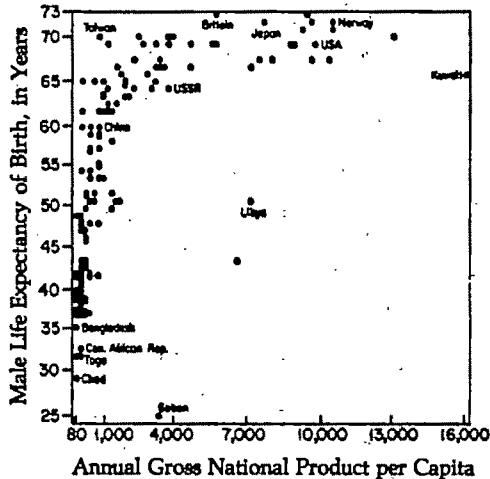
Diminishing Marginal Utility at the Societal Level

Ideology lags behind reality. Though Karl Marx died in 1883, his analysis of political conflict continued to fascinate, and sometimes mesmerize, social critics and social scientists for much of the following century. His thesis captured an important part of reality for the early phases of industrial society. But with the evolution of advanced industrial society new conflicts and new world views have emerged, making the economic conflicts Marx emphasized less central to political life.

This development reflects a principle that might be called the diminishing marginal utility of economic determinism: economic factors tend to play a decisive role under conditions of economic scarcity; but as scarcity diminishes, other factors shape society to an increasing degree. In this article we will examine evidence of this phenomenon, first from an aggregate cross-national perspective, then with time-series data at the individual level. The two types of evidence converge, pointing to a diminishing degree of both

economic determinism and class-based political conflict as advanced industrial society emerges. Figure 1 provides an illustration.

Figure 1. The Diminishing Marginal Utility of Economic Determinism:
1975 Male Life Expectancy at Birth,
by GNP per Capita



Source: Based on data from Charles L. Taylor and David A. Jodice, *World Handbook of Political and Social Indicators* (Cologne: Zentralarchiv für empirische Sozialforschung, 1982), 3:1948-77.

As Figure 1 demonstrates, human life expectancy is closely linked to a nation's level of economic development, especially at the low end of the economic continuum: it is virtually impossible for poor nations to attain a high average life span. A large number of countries cluster tightly together at the low end of the spectrum on both income and life expectancy, with such nations as Chad or Bangladesh showing per capita gross national products below one hundred dollars and a male life expectancy of 35 years or less in 1975. Just above them is a group of 20 nations having per capita GNPs of less than three hundred dollars and life expectancies of less than 45 years.

The curve rises steeply with relatively modest increases in wealth, until it reaches about two thousand dollars per capita. Thereafter, the curve levels off. Economic factors become less decisive, and life-style factors more so: longevity becomes less and less a question of adequate nutrition and sanitation facilities, and more and more a question of cholesterol intake, tobacco and alcohol consumption, exercise, levels of stress, and environmental pollution.

The leveling off of the curve does not simply reflect ceiling effects. In 1975 only a few nations had male life expectancies above 70 years; but in the ensuing decade, several nations raised them by several years. By 1982, female life expectancy in Switzerland had risen to 81 years, and this almost certainly does not represent the ultimate biological ceiling. Most developed nations still have considerable room for improvement—but it is no longer so closely tied to economic development. Thus, despite rising income, male life expectancy in the Soviet Union has declined in the last decade, apparently as a result of rising alcoholism rates.

As our hypothesis implies, a logarithmic transformation of GNP per capita shows a much better fit with life expectancy than does a linear model: the

former explains 61% of the variance in life expectancy, and the latter only 37%. Figure 1 shows untransformed GNP per capita in order to demonstrate the diminishing impact of economic gains directly. Another illustration of this phenomenon is the fact that among the poorer half of these nations, raw GNP per capita explains 44% of the variance in life expectancy; while among the richer half it accounts for only 14%. Similar patterns of diminishing returns from economic development are found with numerous other indicators of the quality of life. Caloric intake, literacy rates, the number of physicians per capita, and other objective indicators all rise steeply at the low end of the scale but level off among advanced industrial societies.

Equality of income distribution also increases sharply with economic development, up to a level of about 35 hundred dollars per capita; but above that threshold there is virtually no further rise. Figure 2 shows this curve (inverted by comparison with Figure 1, since high scores on the vertical axis represent high levels of inequality). In 70% of the nations with a GNP per capita below 35 hundred dollars, the top tenth of the population got more than one-third of the total income (in some cases as much as 57%). In none of the nations with a GNP per capita above 35 hundred dollars did the top tenth of the population get more than one-third of the total income; their share ranged from as low as 17% in Communist countries, to a high of 33% in Finland.

Does this cross-sectional pattern reflect a longitudinal trend? The point has been debated. The most reliable longitudinal data come from economically advanced countries which have shown little increase in income equality during the past 30 years. But if we interpret the pattern as reflecting a curve of diminishing returns rather than a linear trend, this finding is exactly what we would expect: it is only

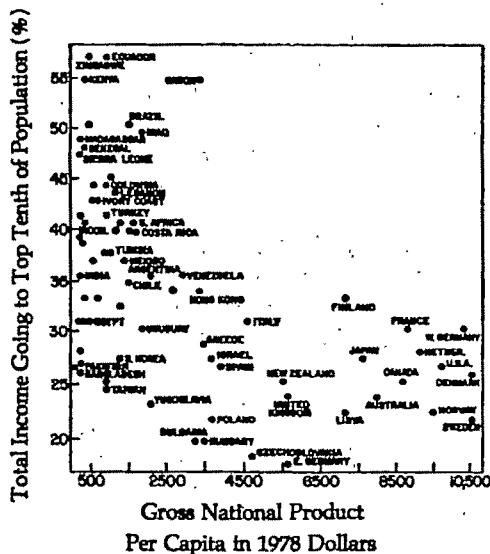
Values in Industrial Societies

in the earlier stages of economic development that we would find large amounts of change. The United States, for example, moved toward substantially greater income equality from 1890 to 1950, but has shown little change since then: absolute levels of income continued to rise, but relative shares changed only slightly. Conversely, Taiwan, South Korea, Singapore and Hong Kong have made a dramatic leap from poverty to prosperity only recently—and all have shown significant increases in income equality (Chen 1979).

Why do we find a curvilinear relationship between economic development and income equality? In the early phase, we believe, it reflects a process of social mobilization, engendered by economic development: industrialization leads to urbanization and mass literacy, which facilitate the organization of labor unions and mass political parties and the enfranchisement of the working class. Economic development does not necessarily have this effect, but it enhances the chances of transforming the masses from isolated and illiterate peasants into organized citizens with the power to bargain for a more equal share of the pie.

But why does the curve level off among mature industrial societies? There are two main reasons. First, as one approaches perfect equality, one necessarily reaches a point of diminishing returns. If one reached the point where the top tenth had only 10% of the income, any further transfer of income would be a move away from equality. None of these societies has actually reached this point, but some are getting close: in East Germany, the top tenth gets only 17% of the total income, according to official sources (these figures may exaggerate the degree of income equality in Eastern Europe: a Czech analysis cited by Connor [1979, 216-18] estimates that income equality in Eastern Europe is no greater than in some Western countries). In any event, there seems to be

Figure 2. Economic Development and Income Equality



Source: Based on data from Charles L. Taylor and David A. Jodice, *World Handbook of Political and Social Indicators* (Cologne: Zentralarchiv für empirische Sozialforschung, 1982), 3:1948-77.

a practical limit, for all societies use different rates of pay to motivate their labor force. China, during the Great Cultural Revolution, may have emphasized economic equality more heavily than any other nation in modern times, but some income differences were still retained. To motivate the people, the regime relied on a combination of intense moral exhortation and coercion. This system had severe drawbacks; the current Chinese regime has shifted toward less coercion and more reliance on economic incentives. The fact that Norway, Sweden, and Denmark have significantly greater income equality than the United States, West Germany or France, indicates that the latter countries could move farther toward equality without becoming ineffective economies, or coercive societies. But the Scandinavian countries seem to be approaching the limit

of what is possible in a democratic political system.

Why this is so, reflects a second basic principle: political support for increased income equality reaches a point of diminishing returns at a level well short of perfect equality. Przeworski and Wallerstein (1982) have demonstrated through mathematical modeling that under appropriate conditions workers and capitalists would reach a compromise in rational pursuit of their material interests: capitalists would consent to democratic institutions through which workers can effectively press claims for material gains. And workers would consent to profits in the expectation that they will be invested productively, improving their future material gains. Thus, given democratic institutions, some inequality of income may be acceptable to the lower-paid groups.

But as one moves closer to an equal income distribution, the political base of support for further redistribution becomes narrower. In a poor society where the top 10% gets 40%-60% of the total income, the vast majority would benefit from redistribution. In a society in which the top tenth gets only 20%-25% of the total income, far fewer people will benefit from further redistribution, and they will benefit proportionately less; a majority may even stand to lose by additional redistribution more than they would gain. This does not constitute a moral justification for not moving further toward equality; but it does constitute a serious practical problem. Under these conditions, the political base for further development of the welfare state is simply not there, at least not in so far as the citizens are motivated solely by economic self-interest. Ironically, further progress toward equality would come *not* from an emphasis on materialistic class conflict, but through an appeal to the public's sense of justice, social solidarity, and other nonmaterial motivations.

Is there any chance that such appeals

would work? We believe that there is. In the long run there seems to be a tendency for the pursuit of economic self-interest itself to reach a point of diminishing returns in advanced industrial societies, and gradually give way to postmaterialist motivation, including greater emphasis on social solidarity (Inglehart 1971, 1977). A large body of survey data, gathered during the past 15 years, suggests that economic development makes a sense of economic deprivation less widespread among mass publics, and consequently, a less important cause of political conflict.

This conclusion is not surprising. After the fact, it may even seem self-evident. It is not: it has been hotly debated and cannot be viewed as conclusively proven even now. A quarter of a century ago, the end-of-ideology school concluded that growing prosperity was giving rise to a *politics of consensus in an age of affluence*; the subsequent explosion of protest in the late 1960s led many to conclude that this school had been completely wrong. In fact, their analysis of what had been happening was partly correct; like Marx, they simply failed to anticipate new developments. While economic cleavages become less intense with rising levels of economic development, they gradually give way to other types of conflict.

Thus far we have seen indications that economic development leads to a diminishing impact of economic influences on such objective characteristics as life expectancy and economic equality. But do such changes actually reshape the political preferences of mass publics?

The evidence suggests that at high levels of economic development, public support for the classic economic policies of the Left tends to diminish. Table 1 sums up the responses of eleven Western publics to a set of questions dealing with three key issues underlying the classic Left-Right polarization. These questions were asked in Euro-Barometer surveys carried

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**Table 1. Support for the Classic Economic Policies of the Left
by Level of Economic Development, 1979-1983**

Nation (Ranked by per Capita Gross Domestic Product)	1982 GDP per Capita (European Currency Units)	Percentage in Favor of ^a			
		Reducing Income Inequality	More Government Management of the Economy	More Nationalization of Industry	Mean for 3 Issues
1. Greece	3,958	95	82	80	86
2. Ireland	5,408	90	72	64	75
3. Italy	6,287	88	68	36	64
4. Northern Ireland	6,852	76	65	57	66
5. Belgium	8,735	87	53	42	61
6. Great Britain	8,755	73	64	46	61
7. Luxembourg	9,407	82	60	31	58
8. Netherlands	9,830	78	64	32	58
9. France	10,237	93	63	44	67
10. Germany	10,927	80	52	41	58
11. Denmark	11,194	71	57	19	49

Source: Based on combined results from Euro-Barometer nos. 11, 16, and 19; respective Ns are 8,884 for 1979; 9,909 for 1981; and 9,790 for 1983. See ICPSR codebooks for details concerning fieldwork. Data on GDP per capita are from Eurostat, *Structural Data* (Luxembourg 1985).

^aText of questions: "We'd like to hear your views on some important political issues. Could you tell me whether you agree or disagree with each of the following proposals? How strongly do you feel [show card] (1) Greater effort should be made to reduce income inequality. [Those who said they *agree* or *agree strongly* are classified in this table as favorable.] (2) Government should play a greater role in the management of the economy. (3) Public ownership of industry should be expanded. In the 1983 survey, the polarity of Item 2 was reversed: while 70% of the 10 publics agreed that government should play a *greater* role in 1981, only 52% rejected the proposal that government should play a *smaller* role in 1983. For Item 2, these figures are based on the 1981 and 1983 results, only, giving equal weight to the two formulations. Missing data are excluded from percentage base.

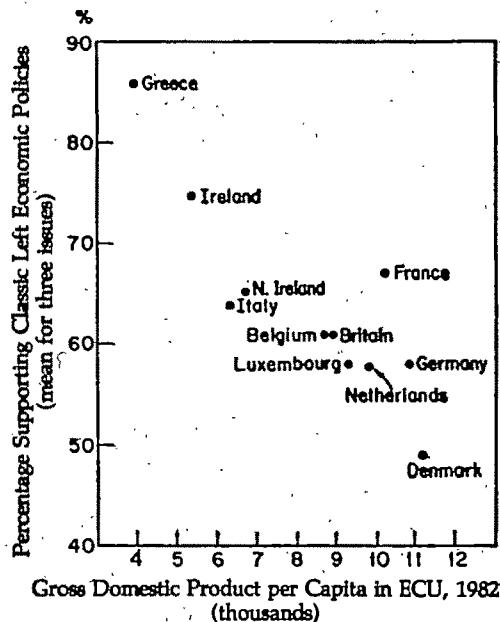
out in 1979, 1981, and 1983 in the 10 member nations of the European Community (Greece, not yet a member, was not surveyed in 1979). The questions deal with redistribution of income, government control of the economy, and nationalization of industry—the central elements of the traditional Left's prescription for society. They were worded as follows:

We'd like to hear your views on some important political issues. Could you tell me whether you agree or disagree with each of the following proposals? How strongly do you feel? (1) Greater effort should be made to reduce income inequality; (2) Government should play a greater role in the management of the economy [in 1983, this item was reversed, to refer to "a smaller role" and its polarity recoded accordingly]; (3) Public ownership of industry should be expanded.

Though a majority supports greater income equality in every country, while further nationalization of industry is rejected by majorities everywhere except in Greece and Ireland, the relative levels of support for these three policies among the publics of given nations show impressive consistency both across items and across time. Taken together, the results add up to a remarkably clear picture of which publics are most favorable to the classic Left economic policies—and the picture does not correspond to conventional stereotypes.

Everyone knows that Denmark is a leading welfare state, with advanced social legislation, progressive taxation, a high level of income equality, and well over half the GNP going to the public sec-

Figure 3. Support for the Classic Economic Policies of the Left by Level of Economic Development, 1979-83



Note: Based on responses to three items, asked in three surveys.

tor. Obviously, the Danish public must be relatively favorable to these traditional policies of the Left. Conversely, everyone knows that Ireland is a largely rural nation, with a modest public sector and no significant Communist or Socialist movements. Clearly, Ireland must be a bastion of conservatism on the classic Left-Right issues.

In fact, the conventional stereotypes are dead wrong on both counts. The stereotypes reflect patterns that were true in the past, but precisely because Denmark has now attained high levels of social security—and very high levels of taxation—the Danish public has little desire for further extension of these policies. Instead, support for the classic economic policies of the Left tends to reflect a

nation's level of economic development. As Figure 3 demonstrates, Greece is by far the poorest country among the 11 societies surveyed in 1979-83; and the Greek public has by far the highest level of support for nationalization of industry, more government management of the economy, and reducing income inequality. Ireland is the second poorest country, and overall Ireland ranks second in support for these policies. At the opposite end of the spectrum, Denmark is the richest country and has the lowest level of support for these policies. West Germany ranks next to Denmark in economic level and also in support for the classic Left policies.

The French show more support for these policies than their economic level would suggest, which may be linked with the fact that France also has an incongruously high level of economic inequality. But France constitutes the only significant anomaly. The other 10 societies show an almost perfect fit between level of economic development and support for the classic economic policies of the Left.

These findings suggest that the principle of diminishing marginal utility applies at the societal level, as well as the individual level. Greece is an economically underdeveloped country, with many living in extreme poverty and a small affluent elite. In such a context, the balance between rich and poor can be redressed only by strong government intervention. Denmark, on the other hand, is a rich country that has long since developed some of the most advanced social-welfare policies in the world and one of the world's highest rates of taxation. Almost 60% of the GNP is spent by the government, approaching the point where it becomes impossible to move much farther in this direction (even in the Soviet Union, the government's share probably isn't over 75%). In Denmark, further redistribution by the government seems less urgent than in Greece; and the costs of government intervention impinge on a larger share of the popula-

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tion. The incentives to press further with the traditional economic policies of the Left become relatively weak, and public resistance relatively strong.

In this sense, the policies that dominated the agenda of the Left throughout most of this century are running out of steam. Increased state intervention was desperately needed to alleviate starvation and social upheaval in the 1930s; was essential to the emergence of the welfare state in the postwar era; and still makes sense in some areas. But in others, it has passed a point of diminishing returns. The renewed respect for market forces that has emerged throughout most of the industrial world recently reflects this reality. It can be debated whether Gary Hart had come up with the right "new ideas" in 1984, but his recognition of the need for a new liberal agenda was well founded. His rival, Walter Mondale, doggedly but helplessly stuck to the New Deal formula a half century after the New Deal.

The neoconservative claim that the classic welfare state policies have failed is false, however. Quite the contrary: in countries like Denmark these policies have largely solved the problems they are capable of solving and have thereby reduced the demand for more of the same. Insofar as they *succeed*, they reach a point of diminishing returns and begin to cede top priority to problems that have *not* been solved.

Any attempt to turn back the clock to the savage laissez-faire policies of the early twentieth century would be self-defeating, ultimately leading to a resurgence of class conflict in all its former harshness. But the fundamentalists of the Left are equally self-defeating in their rigid adherence to a traditional program based on class conflict and state ownership and control of the means of production.

This does not mean that economic factors are no longer politically important. On the contrary, some of the most signifi-

cant recent research in political science has demonstrated strong linkages between fluctuations in the economies of Western nations, and support for the incumbent political party (Hibbs, Rivers, and Vasilatos 1982; Kramer 1971; Tufte 1978). But this research has also produced a surprising finding: while support for the incumbents *does* reflect the performance of the national economy, it *does not* seem motivated by individual economic self-interest. The electorates of advanced industrial societies do not seem to be voting their pocketbooks, but instead seem primarily motivated by "sociotropic" concerns: rather than asking "What have you done for me lately?" they ask "What have you done for the *nation* lately?" (Kinder and Kiewiet 1981). Recent research carried out in France, West Germany, Italy, and Great Britain confirms that there—as in the United States—the linkage between the economic cycle and support for the incumbents is overwhelmingly sociotropic (Lewis-Beck 1986).

In short, economics remains an important influence on electoral behavior, but it reflects sociotropic motivations rather than class conflict. The politics of advanced industrial societies no longer polarize primarily on the basis of working class versus middle class; and the old issues, centering on ownership of the means of production, no longer lie at the heart of political polarization.

Political Change at the Individual Level

The argument presented above is implicit in the materialist-postmaterialist value-change thesis; it is new only in its application to the societal level. In this section we examine political change at the individual level. Here we have a rare opportunity: the chance to test a set of predictions about social change that were

published years before the data by which they are tested came into existence.

At the individual level, our hypothesis concerning the diminishing role of economic factors, is supplemented by a second basic hypothesis: that early-instilled values tend to persist throughout a given individual's life. In context with the unprecedented economic development of the postwar era, these hypotheses imply a shift from materialist toward postmaterialist values. At the individual level, we should find sizable and persisting differences between the value priorities of young and old, reflecting their differing formative experiences; but at the societal level, this shift will manifest itself only gradually, as one generation replaces another. Moreover, because this shift involves basic goals, it implies a gradual change in the types of issues most central to political conflict and in the types of political movements and parties people support. Finally, it also implies a decline in social-class voting and increasing polarization over noneconomic values (Inglehart 1971, 1977).

The intergenerational value differences these hypotheses predict have now been explored extensively, in 26 different nations. Survey after survey reveals dramatic differences between the goals emphasized by old and young. Moreover, cohort analysis demonstrates that there is no tendency for given birth cohorts to become more materialist as they age, as they would if these differences reflected life-cycle effects. In 1985, given birth cohorts were fully as postmaterialist as they had been 15 years earlier. There were significant short-term fluctuations, reflecting period effects linked with inflation (Inglehart 1985a, 1985b). But by 1985, inflation had subsided almost to the 1970 level. With period effects held constant, there is no sign of the gradual conversion to materialism that would be present if a life-cycle interpretation were applicable. When short-term forces

returned to normal, a substantial net shift toward postmaterialism was manifest, most of it due to intergenerational population replacement (Abramson and Inglehart 1986).

The predicted intergenerational value shift seems to be confirmed by a massive amount of empirical evidence, but the predicted changes in prevailing types of political cleavages have barely been touched on. Let us examine the relevant evidence.

From Class-based to Value-based Political Polarization

The idea that politics is a struggle between rich and poor can be traced to Plato. But unquestionably the most influential modern version of this idea has been Marx's argument that throughout industrial society, social class conflict is the central fact of political life, and the key issue underlying the Left-Right polarization is conflict over ownership of the means of production. Marx's influence is reflected not only in a vast literature of social criticism but also in the existence of an entire family of political parties that were inspired by his writings and, in varying degrees, purport to be guided by his analysis. The rise of postmaterialism makes this analysis less adequate today. Let us consider why.

The postmaterialist outlook is linked with one's having spent one's formative years in conditions of economic and physical security. Hence it is more prevalent among the postwar generation than among older cohorts, and tends to be concentrated among the more prosperous strata of any given age group.

The political implications are significant and at first seem paradoxical. Postmaterialists give top priority to such goals as a sense of community and the nonmaterial quality of life, but they live in societies that have traditionally empha-

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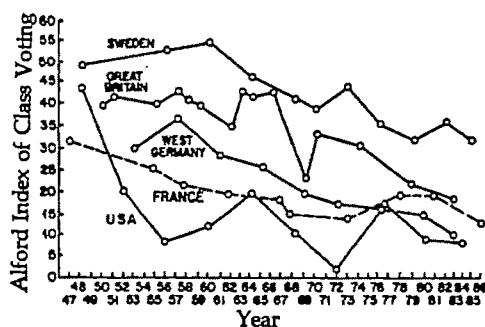
sized economic gains above all, even at the expense of these nonmaterial values. Hence, they tend to be relatively favorable to social change. Though recruited from the higher-income groups that have traditionally supported the parties of the Right, they tend to shift toward the parties of the Left.

Conversely, when postmaterialist issues (such as environmentalism, the women's movement, unilateral disarmament, opposition to nuclear power) become central, they may stimulate a reaction in which part of the working class sides with the Right, to reaffirm the traditional materialist emphasis on economic growth, military security, and domestic order.

The rise of postmaterialist issues, therefore, tends to neutralize political polarization based on social class. Though long-established party loyalties and institutional ties link the working class to the Left and the middle class to the Right, the social basis of new support for the parties and policies of the Left tends to come from middle-class sources. But at the same time, the Left parties become vulnerable to a potential split between their postmaterialist Left, which seeks fundamental social change, and their materialist constituency, which tends to take a traditional stance on the new issues raised by postmaterialists.

In 1972, this phenomenon temporarily split the Democratic party in the United States, when an insurgent movement won the presidential nomination for George McGovern. Though he won the postmaterialist vote overwhelmingly, much of the normally Democratic working-class electorate voted Republican, and social-class voting fell almost to zero, as Figure 4 demonstrates. This was an extraordinary election in which nearly half of all Democratic-party identifiers voted for the Republican candidate. In subsequent elections, many Democrats returned to their normal political loyalties, partially restor-

Figure 4. The Trend in Social-Class Voting in Five Western Democracies, 1947-86

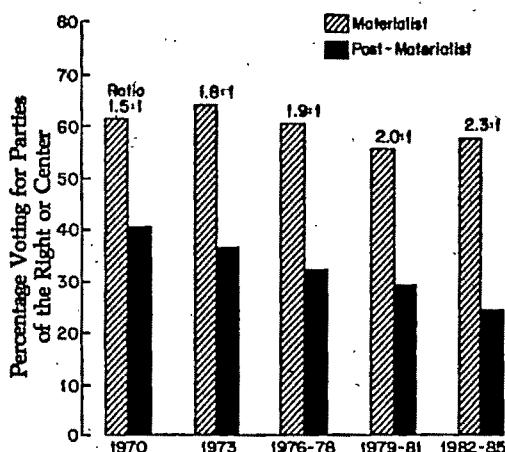


Source: Adapted from Lipset 1981, 505; updated by present author with results from recent elections and French elections. British data from Books and Reynolds (1975), Finer (1980); 1983 results calculated from Euro-Barometer no. 19 data; Swedish data, Stephens (1981), Zetterberg (1986); German data, Baker, Dalton and Hildebrandt (1981), Dalton (1984); American data based on whites only, Abramson et al. (1985); French data, MacRae (1967), later data from surveys conducted by Converse and Dupeux (1962), Converse and Pierce (1986) and Euro-Barometer surveys.

ing class voting. Nevertheless, it now hovers at some of the lowest levels to be found anywhere in the Western world.

In 1981, value cleavages contributed to a more enduring division of the British Left, split between a Labour party that had been captured by a neo-Marxist and neutralist left wing, and a new Social Democratic party that won over much of the party's mass constituency. Throughout the past decade, the materialist-postmaterialist cleavage threatened to split the German Social Democratic party, torn between a postmaterialist "Young Socialist" wing, and the labor-oriented main body. Though the postmaterialist Left was unable to take over the Social Democratic party as long as it still held power in Bonn, they did succeed in launching a Green (Ecologist) party that in 1983 won enough votes to enter the

Figure 5. Percentage Voting for Political Parties of the Right or Center, by Value Type in Britain, France, Italy, West Germany, Belgium and the Netherlands, 1970-85



West German national parliament for the first time.

A longstanding truism of political sociology is that working-class voters tend to support the parties of the Left, and middle-class voters those of the Right, throughout Western society. This was an accurate description of reality a generation ago, but the tendency has been getting steadily weaker. As Figure 4 illustrates, social-class voting has declined markedly during the past few decades. If 75% of the working class voted for the Left, and only 25% of the middle class did so, one would obtain an Alford class-voting index of 50 (the difference between the two figures). This is about where the Swedish electorate was located in 1948, but by 1985 the index had fallen to 31. Norway, Sweden, and Denmark have traditionally manifested the world's highest levels of social-class voting, but all have shown declining levels of social-class voting during the past three decades (Borre 1984, 352). In the United States, Great Britain, France, and West Germany

in the late 1940s and early 1950s, working-class voters were more apt to support the Left than were middle-class voters by margins ranging from 30 to 45 percentage points. In the most recent national elections in these countries, this range had shrunk to 8-18 points. In the most recent national elections (from 1983 to 1986) class voting fell to or below the lowest levels ever recorded to date, in Britain, France, Sweden, and West Germany. Though long-established political-party loyalties tend to maintain the traditional pattern, it is being eroded by (1) new support for the Left coming increasingly from middle-class postmaterialists; and (2) by working-class shifts to the Right, in defense of traditional values (Inglehart 1977; Lipset 1981).

It is important to note that the class-conflict model of politics is not a mere straw man: a few decades ago it provided a fairly accurate description of reality. But that reality has changed gradually but pervasively to the point where today, class voting in most democracies is less than half as strong as it was a generation ago. This change seems linked with intergenerational population replacement: throughout Western Europe, social-class voting indexes are about half as large among the postwar birth cohorts as among older groups.

We have argued that Western politics are coming to polarize according to social class less and less, and according to values more and more. We have just seen evidence of the former. Now let us examine evidence of rising polarization according to materialist-postmaterialist values. Figure 5 sums up voting intentions by value type from almost 95 thousand interviews carried out in Britain, France, Germany, Italy, the Netherlands, and Belgium from 1970 to 1985. A vast number of nation-specific events took place in these six nations during 15 turbulent years, which we will not attempt to discuss here. The overall pattern is clear,

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however: from 1970 to 1985, there was a trend toward increasing polarization on the basis of materialist-postmaterialist values.

In 1970, 61% of the materialists intended to vote for parties of the Right and Center, as compared with 40% of the postmaterialists. Materialists were likelier than postmaterialists to vote for the Right by a ratio of almost exactly 1.5 to 1. This already was a sizable difference, but it has grown steadily larger since 1970. In 1973, the ratio had increased to 1.8 to 1. In 1976-78, it grew to 1.9 to 1. By 1979-81 it was slightly more than 2 to 1. And in 1982-85, the ratio had risen to 2.3 to 1. This changing ratio was mainly due to a loss of postmaterialist votes by the parties of the Right. In 1970, 40% of the postmaterialists supported parties of the Right and Center; in 1982-85, only 25% did so; 75% were voting for the Left.

But which Left? In order to fully understand the significance of what has been happening, we must differentiate between various forces *within* a changing and divided Left. There has been only a modest increase in the proportion of postmaterialists voting for the two major long-established parties of the Left, the Socialists and Communists. In 1970, they drew 48% of the postmaterialist vote; in 1982-85, they got 53%. The major gains have been made by New Politics—above all, Ecologist—parties. In 1970 these parties won 13% of the postmaterialist vote. In 1982-85, they obtained 22%. The New Politics gains reflect two countervailing trends: (1) stagnation or decline of the Marxist New Left parties of the 1960s and early seventies; and (2) spectacular growth of Ecology parties, having a distinct and still evolving ideology concerning the quality of the physical and social environment. They have grown from almost nothing in the mid-1970s, to being the largest component of the New Politics parties. In the last few years, Ecology parties have won representation in the

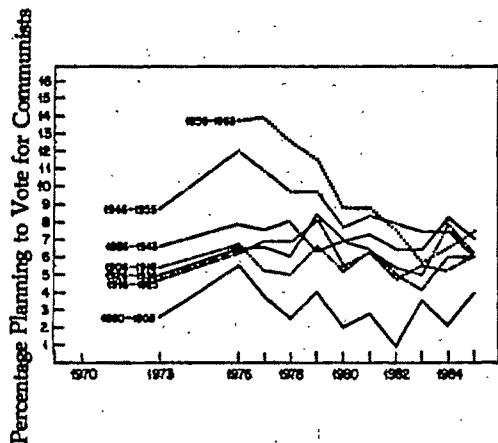
national parliaments of Belgium, Luxembourg, and West Germany, and in the delegations to the European Parliament elected in 1984 from Belgium, Germany, Italy, and the Netherlands. Their future potential may be even more than meets the eye, as we will see below.

The rapid growth of the Ecologists and the decline of the New Left parties of the 1960s reflect an important characteristic of both sets of parties: they have not yet developed strong voter loyalties or party organizations. Whether they ever will is an open question. If they don't, in the long run their electorates will probably be absorbed by larger parties that modify their ideological stance sufficiently to present an attractive alternative, just as the Socialist party has absorbed much of the New Left electorate in the Netherlands, partly capturing it, and partly being captured by it. We will not attempt to forecast the fate of given parties in given countries; it is influenced by the party's leadership, the strategies they adopt, and by nation-specific events, as well as by the values of the electorate. Figure 5 makes one thing clear, however: since 1970 there has been a growing tendency for electoral behavior to polarize on the basis of the materialist-postmaterialist value cleavage.

Postmaterialists have become increasingly likely to vote for the Left but this trend has become increasingly selective, with the Postmaterialist vote going to parties that have distinctive programs tailored to postmaterialist concerns. One striking consequence is that the Communists have lost their relative appeal to postmaterialists. In the early and mid-1970s, postmaterialists were about 2.5 times as likely to vote for the Communists as were materialists. By the mid-1980s, there was little difference between the two groups.

When postmaterialism emerged as a significant political force in the 1960s, its proponents tended to express themselves

Figure 6. Percentage Voting for Communist Party in Nine European Community Nations, by Age Cohort, 1973-85



Sources: Based on representative national samples of publics of Britain, France, West Germany, Italy, Belgium, Netherlands, Luxembourg, Denmark, and Ireland, weighted according to population of given nation. Includes data from Euro-Barometer survey nos. 5 through 23, plus 1973 European Community survey.

in Marxist slogans, which were then the standard rhetoric of protest in Western Europe. To a large extent the term *Left* meant the Marxist parties, and it was natural for the postmaterialists to assume that they were Marxists. But in fact there were profound and fundamental disparities between the goals of the postmaterialists and those of the Marxist Left, as the postmaterialists gradually discovered. These disparities became apparent in France earlier than in other countries—partly because the crisis of May-June 1968 brought to light the basic contradiction between the bureaucratic and authoritarian materialism of the French Communist party, and the postmaterialist desire for a less hierarchical, more human society, in which the quality of life was more important than economic growth. Even there ideological reorientation took

many years. But today, a Left has evolved in France that is increasingly non-Marxist or even anti-Marxist, and increasingly independent of the Soviet Union. The French Communist party, on the other hand, has remained one of the most authoritarian and Moscow-oriented parties in the West, with disastrous electoral consequences. After winning 20%-25% of the vote in most French elections from 1945 to 1978, the Communists suffered a sharp decline in the 1980s, falling to 16% of the vote in 1981, then to 11% in 1984, and dropping below 10% in the 1986 election. Though the degree varies from country to country, by the 1980s communism had lost its disproportionate appeal to the growing postmaterialist constituency in Western Europe.

With this has come the decline of the Communists' relative appeal to youth. For decades it had been axiomatic that the young were disproportionately likely to vote Communist: 'If a man isn't a Communist when he's 25, he has no heart; if he's still a Communist when he's 45, he has no head.' This linkage between youth and the Communist party, however, was not a biological constant; it was based on the belief that the goals of a specific younger generation converged with those of the Communist party.

That belief persisted until recently. As Figure 6 demonstrates, the two postwar cohorts among the West European publics were markedly more likely to support the Communist party than were their elders in each of the Euro-barometer surveys carried out during the 1970s. But the differences were growing weaker in 1978 and 1979. In the early 1980s, Communist support among the two postwar cohorts continued to decline, falling to, or even below the level of older cohorts. The young and the postmaterialists had shifted to other parties.

Where did they go? For the most part, not to the parties of the Right, despite a good deal of recent talk about the grow-

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Table 2. Support for Traditional Left and New Politics Parties by Age Cohort in Britain, France, West Germany, Italy, Denmark, Ireland and Benelux, in 1984-85 (%)

Birth Years	Communists	Socialists and Social Democrats	Ecologists and Other New Politics Parties	Right and Center	Number of Cases
Postwar					
1966-70	5	31	19	45	1,026
1956-65	8	36	17	39	3,782
1946-55	8	40	9	43	3,692
Prewar					
1936-45	7	38	6	50	3,143
1926-35	8	36	4	52	2,810
1916-25	6	40	3	51	2,886
1906-15	7	35	2	56	1,732
Before 1906	3	30	1	66	421

Source: Combined data from Euro-Barometer nos. 21, 22, and 23 (April and November 1984 and April 1985) weighted according to population of each nation (total number reporting a voting intention, 19,492).

ing conservatism of youth. The old Left clearly is in decline, which presents an opportunity for enterprising and adaptive parties of the Right and Center. But given the option, younger voters gravitate toward New Politics parties, above all the Ecologists, rather than toward the Right.

As Table 2 demonstrates, support for the Communists no longer bears much relationship to age. The Socialists and Social Democrats have also lost their special appeal for the young in most countries. Nevertheless, support for the parties of the Right and Center is substantially weaker among the three postwar cohorts than among all of the older groups. There is no overall trend to the Right. Instead, the slack is taken up by a pronounced rise in support for the Ecologists and other New Politics parties. Consequently, the relative strength of the Communists and the New Politics parties has shifted dramatically among the young. Among the cohorts born before World War II, Communist support outweighs New Politics support by as much as 2:1 or 3:1. In the cohort born in 1946-55, the New Politics parties are slightly stronger than the Communists. And among the two young-

est cohorts, support for the New Politics parties outweighs support for the Communists by margins of 2:1 and nearly 4:1.

Neither the young nor the postmaterialists automatically vote for any party that claims to represent the Left. They are influenced by past loyalties, like other voters. But when they abandon these loyalties, they do so in order to support the party that seems most likely to attain their goals, which are not necessarily those of the old Left, and emphatically not those of rigidly authoritarian parties such as the French Communist party. The old Left parties are losing ground among the young: they win the support of only 44% of the 1945-65 cohort, and only 36% of the 1966-70 cohort. But the Right need not win them by default. When an option is available that addresses the postmaterialists' concerns, they tend to take it. The evidence indicates that the Left can win the young *provided* it develops programs that appeal to the postmaterialists, as well as the old Left constituency—clearly not an easy task, but not an impossible one.

Though Western communist parties are in decline, it is extremely unlikely that

they will disappear. They still have millions of hard-core loyalists. Moreover, they have made important contributions to politics in their societies: by advocating greater economic equality they stimulated mainstream parties throughout the West to adopt welfare state policies that were motivated, in part, by the need to meet the challenge from the Left, but that helped their societies adapt to the contemporary world.

Today, Marxism itself needs to readapt—and for the sake of survival, is likely to do so. The Italian Communist Party already has reshaped itself extensively. Conservative Marxist parties like the French Communist Party will eventually be forced to do so, regardless of what the current leadership prefers. The need for change is so clear that it is taking place even in societies ruled by communist parties, with first Yugoslavia and Hungary and more recently, the People's Republic of China moving to relax the grip of central controls and allow greater freedom of expression. As the 1980s draw to a close, the Soviet Union itself is experimenting with ways to liberalize its economy and society. Even Marxist movements must respond to the forces of history.

Conclusion

The rise of a new axis of politics, based on polarization between postmaterialist values and traditional cultural values, and the decline of class-based polarization, has left Western political systems in a schizophrenic situation. Most of the major political parties have been aligned along the class-based axis of polarization for decades, and established party loyalties and group ties still hold much of the electorate to this alignment. But the most heated issues today tend to be New Politics issues, on which support for change comes mainly from a postmaterialist middle-class base. This creates a stress that can be resolved in two ways: by a

repositioning of the established parties; or by the emergence of new ones. Both have been taking place.

The Marxist interpretation of society seems in decline throughout the industrialized world. Its emphasis on economic factors as the driving force of history provides a good first approximation of reality in the early stages of industrialization, but is of diminishing value as scarcity diminishes and new problems emerge. Similarly, the *policies* that are needed to counter the ruthless exploitation of capitalism in its laissez-faire stage, reach a point of diminishing returns in advanced welfare states. Where government spending is already 40%–60% of GNP, there is less potential to move further in this direction; and the concentration of power in big government itself becomes an increasingly serious problem. The old assumption of the Left that more government was automatically better has lost its credibility. But to elevate government nonintervention into a theological principle is equally untenable. Some of the emerging problems of advanced industrial society will require more government intervention, not less. Within the next few decades, we will need to shift from petroleum to other energy sources or face dislocations far more severe than the OPEC-triggered recessions of the mid-1970s and early 1980s. Developing solar energy and nuclear fusion to meet this need requires a massive research and development effort, decades ahead of time, that market forces are not making and are not likely to make. Governmental inertness in such areas may be disastrous.

The meaning of *Left* and *Right* has been transformed. The key Marxist issue—nationalization of industry—remains a central preoccupation only to Marxist fundamentalists like the embattled hard-liners of the British Labour party. Properly handled, nationalization does little harm; but it is not the panacea it once appeared to be. And in so far as it diverts attention

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from increasingly pressing problems concerning the nature of modern war and the quality of the physical and social environment, it can be downright counterproductive—for it provides no solution to these problems. Nationalized factories pollute just as much as private ones. Indeed, in so far as nationalization merges the political regulators and the military-industrial complex into one cozy elite, it may even make things worse. Thus, according to a recent UNESCO study, East Germany is the most severely polluted nation in Europe, with air and water pollutant levels two to three times as high as those in West Germany. And it is no coincidence that the only nuclear-power-plant accident that has cost human lives, occurred in the Soviet Union, where public pressures for safety measures are minimized and, until recently, plants were built without containment structures, using a type of graphite reactor that Western countries stopped building decades ago because it was too hazardous. Though their environmental problems are even more severe and their arms expenditures at least as high as those in the West, the political systems of the East European countries rule out the emergence of independent and vigorous environmentalist movements or peace movements comparable to those in the West. It is relatively easy for the ruling elite to simply ignore such issues: officially, the problems underlying them exist only in capitalist countries.

War is as old as human society, but modern technology has given it new implications: today, it could terminate the human race. This fact has shaped a post-war generation for whom war has a different meaning from what it had in previous history. If one still accepts the Marxist conventional wisdom that war is caused by capitalist greed for profits, then one has a ready solution: abolish capitalism. But the fact that most of the bloodiest battles of the past decade have been

fought *between* nominally Communist countries—with Cambodia against Vietnam, Vietnam against China, China against the Soviet Union, and the Soviet Union against Afghanistan—has led to growing skepticism that the answer is this simple. The problem is rooted in the mentality of a military-industrial complex deeply rooted on *both* sides of an obsolescent ideological boundary.

The old ideological paradigm no longer corresponds to reality. Neither the Marxist fundamentalists nor the laissez-faire fundamentalists have adequate answers for the problems of advanced industrial society. The goals of individuals and the challenges facing society are different from those of a generation ago.

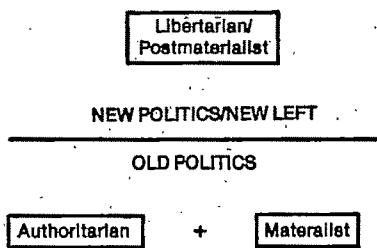
RONALD INGLEHART

University of Michigan

This controversy harks back to a debate I started with Ronald Inglehart in 1979 (Flanagan 1979), which has found its clearest expression to date in an exchange of articles in 1982 (Flanagan 1982a, 1982b; Inglehart 1982). There are two reasons for revisiting this debate now. First, Inglehart's most recent contribution to the values debate, presented above and in a companion piece,¹ represents a significant shift in his position, which I believe brings his argument much closer to my theoretical view. Second, new data is now available to present a definitive test, at least in the Japanese context, of the issues that divide us. Thus, I believe we are finally in a position to take a giant step towards a resolution of the debate.

The crux of the debate revolves around my argument that there are two distinct kinds of value change taking place in the advanced industrial democracies and that Inglehart has obscured this distinction by collapsing indicators of both into a single scale. My argument has been that these

Figure 7. Inglehart's View of Cleavage Structures in Advanced Industrial Democracies

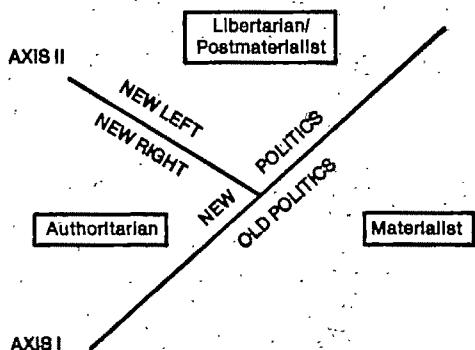


two kinds of change are not only conceptually distinct but are explained by different causal phenomenon and exhibit different patterns of relationships with key demographic and political variables.

Libertarian-Postmaterialist Values

The distinctions between our respective positions can be confusing because we use overlapping but not identical kinds of indicators to measure our respective scales, and similar labels to identify different conceptual phenomenon. To help clarify the differences, Figures 7 and 8 depict our contrasting conceptualization of value change, with the types of indi-

Figure 8. Flanagan's View of Cleavage Structures in Advanced Industrial Democracies



cator items used to measure the scales presented in boxes. Each line represents the axis of a value cleavage, with labels on either side of the cleavage indicating the political impact of the particular type of value orientation defined by the cleavage.

First it should be noted that our three scales, my two and Inglehart's one, are measured by essentially only three kinds of items. What I call *libertarian* and Inglehart and others label *postmaterialist* items are essentially identical. Looking across the full collection of items that have been designated by these labels, we find they include an emphasis on personal and political freedom, participation (more say in government, in one's community, and on the job), equality, tolerance of minorities and those holding different opinions, openness to new ideas and new life styles, environmental protection and concern over quality-of-life issues, self-indulgence, and self-actualization (Calista 1984; Hildebrandt and Dalton 1978; Inglehart 1977; Lafferty and Knutsen 1985). There may be some minor differences over which of the above elements are more or less central to these two concepts, but to the extent that they are all found to cluster, I expect none of the above authors would have trouble accepting any of these elements as part of the concept of *postmaterialism*. I include them all in my notion of *libertarianism*, so we have two different labels representing the same cluster of items. While I believe that Inglehart's term *postmaterialism* is a misnomer as a characterization of either the causes or nature of this value cluster, I will label it here *libertarian-postmaterialist* to clarify that regardless of how we choose to identify these items, we are measuring essentially the same set of values.

The differences emerge at the other end of our scales. I have used the term *materialism* in a more limited sense than Inglehart to identify the emphasis attached to economic concerns, both for oneself and

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one's society. In my sense of the term, then, *materialists* are those who place a high priority on a stable economy, economic growth, fighting rising prices and, at the more personal level (the private domain), on securing a high-paying job, adequate housing, and a comfortable life. Inglehart includes in his *materialism* concept, however, a second set of non-economic issues, namely support for a strong defense, law and order, and fighting crime. I label this second set of non-economic issues as one component of an *authoritarian* value orientation. This *authoritarian* orientation designates a broader cluster of values, which, along with concerns for security and order, includes respect for authority, discipline and dutifulness, patriotism and intolerance for minorities, conformity to customs, and support for traditional religious and moral values. To avoid confusion, I will use the term *materialism* in this discussion to refer to my definition of the term rather than his.

As Figure 7 shows, Inglehart in his original argument presented us with one dimension of value change. Those holding his combination of what I label *materialist* and *authoritarian* value orientations defined the Old Politics for him, while those with the libertarian-postmaterialist orientation the *New Politics* or *New Left*, terms which he used virtually synonymously. He made no mention of a New Right.

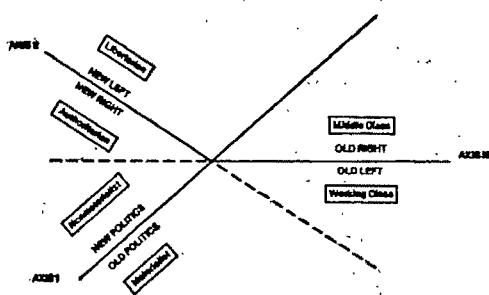
This conceptualization encounters difficulty when it attempts to explain realignment. On the one hand we can readily grasp why the younger generation of highly educated, middle-class respondents, whose families have traditionally supported the Right for economic reasons, may now be induced to vote Left as a result of their socialization into libertarian-postmaterialist values. Inglehart's argument breaks down in trying to explain why working-class voters should abandon their historic support of the par-

ties of the Left. Here his argument has rested on a variation of the *embourgeoisement* thesis that he still endorses (Inglehart and Rabier 1986, 458). In his original formulation he wrote that working-class respondents who "have attained a certain level of prosperity relatively recently" will "continue to place a comparatively high value on defending and extending their recent gains" (Inglehart 1971, 992). Because the value priorities of these working-class respondents remained primarily acquisitive or materialist, their growing share of the good life would lead them to become more economically conservative and, hence, potential recruits for the conservative parties.

However, while the working-class Tory phenomenon is well documented, it is capable of explaining the behavior of only a deviant portion of the working class (Goldthorpe et al. 1968; Hamilton 1967). Conservative parties do not defend the economic interests of the working class, and certainly we cannot expect a major realignment of working-class support from Left to Right based on the economic appeals of conservative parties. Moreover, realignment via *embourgeoisement* applies only to the most affluent portion of the working class. However, since affluence in Inglehart's formulation is the primary cause of postmaterialism, these are the very workers most likely to be developing postmaterialist values, which should only reinforce their traditional support for the Left.

In contrast to Inglehart's view, my conceptualization defines two distinct dimensions along which values are changing. The first value cleavage, running along the lower-left-to-upper-right diagonal, taps the priority a respondent attaches to economic issues as opposed to non-economic, value issues. This cleavage divides materialists from nonmaterialists and the Old Politics from the New Politics. Since materialists attach primary importance to economic concerns, they

Figure 9. Full Cleavage Structure for Advanced Industrial Democracies



tend to relate to politics in economic terms, which corresponds to the traditional class politics of the Old Right and the Old Left party alignments. Conversely, the nonmaterialists emphasize the importance of value concerns over economics. This emphasis on value issues defines the New Politics and includes, for example, both prochoice and right-to-life alternatives and both strong defense and antinuclear positions. Nonmaterialists, then, are those holding either authoritarian or libertarian value preferences, and those who place a higher priority on the kinds of issues defined by these value preferences than on economic issues.

The New Politics is divided by the second value cleavage, which distinguishes the New Left from the New Right. Those falling on the libertarian side of this second value cleavage support for the New Left issue agenda, including liberalizing abortion, women's lib, gay rights, and other new morality issues; protecting the environment, antinuclear weapons, and other quality-of-life issues; and support for protest activities, more direct forms of participation, and minority rights. On the other side of this value cleavage, the authoritarians endorse the New Right issue agenda, which includes right-to-life, anti-women's lib, creationism, antipornography, and support for traditional moral and religious values; a strong defense, patriotism, law and order,

opposition to immigration and minority rights, and respect for the traditional symbols and offices of authority.

Figures 7 and 8 are presented to demonstrate the contrasting ways that Inglehart and I have chosen to combine three types of items into our respective scales. Figure 9 depicts the full cleavage structure and its implications for realignment. As in Figure 8, Axis 1 again divides the Old Politics from the New Politics and materialists from nonmaterialists on the basis of the relative salience accorded to economic, as opposed to value, issues. The relevant cleavage on the New Politics side of Axis 1 is Axis 2, which divides the New Left from the New Right and libertarians from authoritarians. The relevant cleavage on the Old Politics side of Axis 1 is Axis 3, which divides the Old Right from the Old Left and the middle class from the working class. As the dotted extension of the New Left-New Right value cleavage into the Old Politics domain in Figure 9 suggests, the Axis 1 value priority distinction and the Axis 2 value preference cleavage are essentially independent of each other. Thus we should expect to find libertarian-materialists as well as authoritarian-materialists. The line is dotted in the Old Politics domain because for the materialist, who places greatest priority on economic concerns, the libertarian-authoritarian distinction will have little effect on his or her voting behavior. Similarly, the Axis 3 class cleavage extends as a dotted line into the New Politics domain, but for nonmaterialists these class distinctions will not be paramount in their voting decisions.

It is important, therefore, to reach an independent determination of a respondent's position on both value dimensions. An authoritarian-libertarian value preferences scale will tell us whether the respondent is likely to support the New Right or New Left issue agenda. A materialist-nonmaterialist value priorities scale will tell us whether these New Poli-

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tics kinds of value concerns or the Old Politics economic issues will be foremost in the voter's mind when he or she makes a choice. This latter distinction is particularly important for predicting the behavior of cross-pressured voters, who, for example, may fall on the Left side on the Old Politics cleavage because of their working-class occupations and union memberships but on the Right side of the New Politics cleavage because of their authoritarian values.

The conceptualizations of value change in Figures 8 and 9 can explain the realignment of working-class voters to the Right more effectively than can Inglehart's conceptualization (Figure 7). Across Axis 2 in Figure 9 there has been a long-term shift from authoritarian to libertarian values associated with the changing circumstances under which younger generations are being socialized and the growth of higher education. As the number of libertarians in the advanced industrial democracies reached a critical mass, they began pushing for New Left issues and achieving some successes. The increasing articulation of libertarian values and the protest movements organized to press for the adoption of the New Left agenda mobilized a backlash among authoritarians, who felt that their basic values and way of life were being threatened. This increasing polarization on the New Politics issues in turn heightened their salience in relation to the Old Politics class issues, which were already on the decline due to growing affluence and the success of the welfare state. Thus the shift across Axis 2 from a heavily asymmetric balance in favor of the traditional authoritarian values to a more symmetric balance between authoritarian and libertarian values heightened the salience of New Politics relative to Old Politics issues, thereby inducing movement across Axis I as well. Since education is related to the class and values cleavages in a cross-cutting pattern, associating high

education with both the Old Right and the New Left, the combined trends on both dimensions promote the middle-class-to-the-Left and working-class-to-the-Right realignment pattern Inglehart and others have been describing.

One thing that is new in Inglehart's most recent contributions is that he is now beginning to adopt much of the terminology of the Figure 9 conceptualization explicitly. However, he is doing this without in any way revising or altering his scale, which collapses the two dimensions into one and hence treats authoritarian values and the New Right issue agenda as essentially synonymous with materialism. This approach of grafting a new terminology onto his old conceptualization simply adds to the confusion.

Examples of this subtle shift in terminology are abundant in his recent discussions of the value change-realignment phenomenon. In this discussion, Inglehart first reiterates the argument that he and others have made explaining why the New Politics values cleavage is an important phenomenon to study even though there is as yet little evidence that it has induced a realignment of party systems in the advanced industrial democracies. That argument points out that the expression of the New Politics value cleavage in patterns of party support is inhibited by frozen party loyalties and institutionalized interest-group-to-party linkages (Dalton, Flanagan, and Beck 1984; Flanagan and Dalton 1984). Nevertheless the rising salience of the New Politics is increasingly placing these party systems under stress. Perhaps Inglehart's most important contribution in this regard is to demonstrate that the meaning of *Left* and *Right* is changing among the elites and mass electorates of these societies from economic-issue orientations to non-economic-value-issue orientations (Inglehart 1984). And most recently, he demonstrates that the amount of class-party realignment that can be shown to have

already taken place is mostly benefiting New Left and New Right parties rather than the traditional Old Left and Old Right parties and that beyond that, there exists considerable potential support in these electorates for New Left and New Right kinds of parties (Inglehart and Rabier 1987; and the opening to this Controversy).

In this discussion of the shift from the Old Politics to the New Politics and its implications for realignment, he seems to buy into many of the distinctions presented in the conceptualizations of Figures 8 and 9. He says that the New Politics issue polarization "primarily reflects the new noneconomic issues" and that "the issues that define Left and Right for Western publics today are not class conflict [issues], so much as a polarization between the goals emphasized by post-materialists, and the *traditional social and religious values* emphasized by materialists" (Inglehart and Rabier 1986, 470, 471; emphasis added). While still using the term *materialists*, the cluster of values and issues preferences that emerge from his own factor analyses reported in his 1984 and coauthored 1986 pieces as being associated with self-placement on the Right end of a Left-Right scale are that the military-defense effort should be stronger, the existing social order should be defended, terrorism should be punished more severely, nuclear power plants are essential, the unemployed don't want to work, there are too many immigrant workers, one should sacrifice oneself for one's country, abortion is wrong, religion is important, and God exists. To apply the label *Materialist* to these New Right issues is clearly a misnomer. Moreover, the New Left issues he identifies (pro-environment, abortion, peace, homosexual rights, etc.) clearly did not "stimulate a materialist reaction" in the sense of an increased concern for economic issues. Quite to the contrary, the New Left issues have helped to crowd the economic issues

off the agenda and have rather provoked the emergence of the above New Right set of moral and religious issues.

Here we begin to see the problems inherent in the theoretical arguments and conceptual labels that Inglehart has attached to his value scale. The emergence in his own studies of a New Right cluster of noneconomic value issues around the New Politics cleavage that fit my definition of authoritarian values implies a delinking of the New Right with the concept of *materialism*. The point is that the New Right is as much *nonmaterialist* as the New Left. The program of the National Front, the French example of a New Right party, stresses law and order, restrictions on immigration, opposition to abortion and anticomunism. It is because these issues have little or nothing to do with traditional class issues and material, economic concerns that the New Right parties are able to draw their support disproportionately from the working class, as Inglehart so clearly demonstrates.

A New Theoretical Basis for Materialism

In my view, the most important fresh contribution found in Inglehart's analysis presented above is the new theoretical basis he provides for understanding the shift from materialist to nonmaterialist issue priorities. He labels this argument "the diminishing marginal utility of economic determinism." The diminishing-marginal-utility approach is a much sounder theoretical foundation for explaining the shift from economic to noneconomic value priorities than Maslow's need hierarchy. Maslow presents us with a theory of psychological development and motivation arguing that an individual reared in an environment in which lower-level needs are satisfied will develop into a mature personality, one

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who as an adult is better able to cope with the deprivation of lower-level needs and hence is insulated from need regression (Maslow 1970). In contrast, Inglehart's items typically tap issue priorities in the domain of public policy. He is, then, dealing with a vastly different phenomenon, and thus the inappropriateness of the Maslowian argument leaves him with no rationale for explaining why respondents would not increasingly place a higher priority on the issue of rising prices when inflation becomes an increasingly serious problem in their country. Indeed the evidence suggests that they will (Flanagan 1982b).

What the *diminishing marginal utility* concept suggests, however, is something quite different than the Maslowian early-childhood-socialization notion. As Inglehart's Table 1 and Figure 3 demonstrate, the higher a nation's GNP per capita, the weaker both the felt need for further reducing income inequality and the other classic economic policies of the Left designed to redistribute the wealth more equally. As Inglehart argues, after a certain level of equality has been achieved, further movement in the direction of perfect equality has a diminishing marginal utility, as there is less and less left to redistribute, and fewer gain and more lose in the process.

Figure 1 in his companion piece (Inglehart and Rabier 1987), most clearly presents the lesson that is to be learned from the diminishing-marginal-utility thesis. As median income goes up in a nation, the ratio of what the respondent views would be the absolutely necessary income to what the actual household income is drops. In other words, more people find themselves with a comfortable surplus. Their basic economic needs are being met, so economic issues become less intensely held and are assigned a relatively lower priority than they were when household income was at or below the level of absolutely necessary income. All other

things being equal, the proportion of nonmaterialists will rise as average household income within a nation rises above a level perceived as necessary to provide the basic necessities of life. As the margin of surplus income increases, the citizen's expenditure of energy in support of economic issues will yield a diminishing marginal utility, and the nonmaterialist is born.

Being a nonmaterialist at time one, however, does not ensure that a respondent will still assign a relatively low priority to economic concerns at time two. As has been shown elsewhere, the relative priority that a respondent attaches to economic issues in relation to other concerns varies across the life cycle as the respondent's economic responsibilities and burdens change (Flanagan 1982a; Milkis and Baldino 1978). A respondent's priorities will also shift in response to short-term changes in perceptions of how well-off he or she is. Aggregate stability in the proportion of materialists, therefore, is a function of national affluence and will remain rather stable so long as economic conditions in the country do not change. Nevertheless, aggregate stability can mask considerable instability at the individual level.

Inglehart claims that his diminishing-marginal-utility (DMU) argument is not new but was implicit in his original value-change thesis. Rather, the logic of the DMU argument as a causal explanation of value change is completely different from his original Maslowian approach. It should be recalled that it was his reference to Maslow's hierarchy of needs that enabled Inglehart to claim that the materialism-postmaterialism value change was the product of early childhood socialization. The Maslowian analogy gave birth to the idea that socialization under conditions of affluence and security create a distinct prioritization of needs that immunizes the individual from regression to lower-level concerns as an

adult, even if the conditions of life change. Apparently Inglehart has found that there is a diminishing marginal utility in citing Maslow for his purposes, and indeed we find no references whatsoever to Maslow or the need hierarchy in either of his two current companion pieces. I believe history will show that his Maslowian argument was a clever analogy that did not fit.

In contrast, childhood socialization logically plays no role in the DMU argument. An estimation of the diminishing marginal utility of added increments of income is a rational-choice assessment based on the individual's current level of need and sense of relative deprivation. This assessment, then, is very much a *context-dependent* phenomenon, very similar to the life-context arguments that Milkis and Baldino (1978) and I have made. What is new in Inglehart's notion is that he has changed the relevant context from the individual level, a level at which we would expect to find considerable change across the life cycle, to the societal level. In doing so, he has convincingly made the case that we should find important thresholds in national income and household income, which, once crossed, yield aggregate shifts in the relative priority attached to economic concerns. These thresholds are probably not absolute but rather are likely to shift somewhat in line with changing societal standard-of-living expectations. Still, the notion that at some point a sufficient surplus should lower the priority attached to economic issues and to further increasing one's income, not in every case but in the aggregate, is compelling.

The *diminishing marginal utility* notion provides some legitimacy for using the term *postmaterialist*. I have preferred the term *nonmaterialist* because of my focus on the fluctuation that occurs on the individual level across the life cycle. However, the notion that advanced industrial societies as a whole are moving towards a

lower priority ranking for materialistic concerns suggests more of a unidirectional shift, perhaps justifying the *post-* prefix. Still this is clearly not the kind of irreversible phenomenon as suggested by the Maslowian arguments. Indeed we would expect that serious economic problems, such as runaway inflation or depression, would alter the current assessments of many respondents as to the relative priority that should be attached to economic concerns.

Thus, we are dealing with two distinct kinds of value change explained by very different kinds of causal phenomenon. On the one hand we are witnessing an eclipse of the salience of economic issues. This trend is explained by the diminishing-marginal-utility argument. As the affluence of, and equality within, nations increases, the percentage of the population that enjoys a cushion of surplus income above what is needed for the basic necessities of life grows. For these people, noneconomic issues begin to gain in salience relative to material, economic concerns. There may be some variation in the priority accorded to economic issues at the individual level over time, with changes in fortune and life context, and also short-term setbacks at the aggregate level associated with economic downturns. Over the long haul, however, we should be seeing a net growth in the proportions of *postmaterialists*—as here defined in terms of the relative priority attached to economic issues (Old Politics) as opposed to noneconomic, value issues (New Politics).

The second and distinct trend is the one that defines the change in value preferences. Within the so-called New Politics can be detected a long-term, gradual movement from the New Right to the New Left poles, as a function of age (intergenerational change) and education. Space constraints will not permit a full elaboration here of the causal mechanisms, elaborated elsewhere, behind this

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pattern of value change (see Flanagan 1979, 1982a, 1984). In brief, the argument is that an intergenerational pattern of value change along the authoritarian-libertarian (A/L) dimension has resulted from four major changes in the basic conditions of life under which successive generations have been socialized—a growing equality in incomes and life styles, the accelerating pace of change, the advance and diffusion of scientific knowledge, and the rise of the "no-risk" society (Aharoni 1981). These changes are increasingly liberating mankind respectively from the constraining conditions of subservience to authority figures, conformity, ignorance, and insecurity, and enabling the individual to pursue more fully the goal of self-actualization. Given the nature of many of these changes in basic life conditions, which are driving the change along the A/L value dimension, it is not difficult to understand why the increasing levels of higher education found in the advanced industrial societies are also playing an important role in diffusing libertarian values.

New Evidence of Dimensional Independence

At this point one might raise the question why—if I am in fact right—it has been so difficult to demonstrate the problems with Inglehart's scale empirically and why he has been able to pile up evidence from an exhaustive number of surveys conducted in 10–15 nations over the past 15 years. For example, Inglehart has consistently claimed that there is no evidence of a life-cycle effect on the proportion of materialist found in given age cohorts over time and that there was no substantial reduction in the proportion of his postmaterialists across a large number of countries despite inflation, recession, and other economic woes in the seventies.

Moreover, he seems to have the evidence to make good on those claims.

Such a finding is central to his argument because it supports his notion that the materialist-postmaterialist change is a function of early learning that insulates the individual against later change. However the problem here is that while Inglehart labels his scale *materialist-postmaterialist*, 75% of the items used to operationalize the scale rather tap the authoritarian-libertarian dimension. This latter dimension, which measures how respondents divide on the New Politics value cleavage, is not affected by changes in the economy. Changes in one's income, all other things being equal, will not affect one's position on abortion, pornography, or nuclear weapons or one's respect for authority. The relative aggregate stability of Inglehart's scale over time in the face of changes in the economic context, therefore, is derived in large part from the fact that his scale, in essence, is tapping more of the New Left-New Right (libertarian-authoritarian) cleavage than the New Politics-Old Politics (materialist-non-materialist) division.

What further confounds our ability to disaggregate the two dimensions combined within his scale is the format of his value-priority questions, which artificially forces an association between his materialist and authoritarian items. Whether using his original 4-item format or his expanded 12-item version, respondents are typically presented with groups of four alternatives and asked to select their first and second priorities. In each case two items are—to use my terminology—libertarian (e.g., free speech and participation); one item is authoritarian (e.g., defense); and one is materialist (e.g., rising prices). This presents the respondent with a *dilemma of constrained choice*. Libertarians who are also non-materialists will naturally select the two libertarian items. Libertarians who are materialists, placing greater priority on

their economic than on their value concerns, will pick the one materialist item and then, having no other materialist item to pick, will select one of the libertarian alternatives, dropping them into Inglehart's mixed category. Authoritarian-materialists, following the same logic, will pick the materialist item first and then select the authoritarian option.

The real problem, however, is confronted by the authoritarian-nonmaterialists. As a nonmaterialist the respondent will be mostly concerned with the non-economic, value issues. But he or she finds only one authoritarian option. As an authoritarian, the respondent is very unlikely to select either of the libertarian options and thus, by default, is most likely to pick up the economic option as a second choice. The result is that the authoritarian-nonmaterialists are classified as *materialists*, which is an incorrect coding for a materialist-nonmaterialist scale but a correct coding if we view Inglehart's scale as a stand-in for the authoritarian-libertarian scale.

This logic explains two important characteristics of Inglehart's scale. First it helps us understand why we always find a heavily skewed distribution with many materialists and typically only 5%-15% postmaterialists, while the authoritarian-libertarian type of scale tends to divide samples much more evenly. Although both types of nonmaterialists are properly sorted, the authoritarian-materialists are classified as *materialists* while the libertarian-materialists are classified as mixed. Secondly, and more importantly, whatever real association there may be between authoritarian and materialist orientations is greatly exaggerated by the constrained-choice feature, which forces authoritarian-nonmaterialists to select the materialist option.

The answer to this problem is to adopt an item format that allows an independent and unconstrained assessment of the priority of all three types of items identi-

fied in Figure 7—libertarian, authoritarian, and materialist. This is a little tricky, due to the inherent differences between value priorities and value preferences. The materialism items are designed to tap the relative importance of economic and value concerns, both of which are presumably positively valued by the respondent. In contrast, authoritarian and libertarian values stand in opposition to each other, with those who stand at one pole typically viewing the opposite set of preferences in a negative light.

The solution to these problems is to follow the approach developed for the 1976 German Youth Survey conducted by the United States Information Agency (see Dalton 1981). Respondents are handed a card with a number of social and personal goals listed and in each case are asked to indicate whether they feel that their society places too much or too little emphasis on that goal. For the value-priority items, this allows for a rough ranking of the items' importance; for the value-preference items it indicates on what side of the New Politics value cleavage the respondent is found. Since all three types of items are asked in the same way, this procedure will enable us to determine how they cluster in an unconstrained situation. If Inglehart is correct, the materialist items on economic concerns should cluster with the security subset of authoritarian items, and the rest of the authoritarian items should lie distinctly outside of that cluster. If I am correct, all the authoritarian items should cluster together while the materialist items form a distinct separate cluster. A third option somewhere in the middle would be to find that all the authoritarian and materialist items cluster together.

The data to test these conflicting hypotheses comes from a 16-item value question I designed using the above unconstrained format administered in a 1984 nationwide Japanese survey of university-educated respondents.² Due to the rich-

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ness of the value themes tapped by the survey, it was possible to add several more items, bringing the total to 21, with 9 libertarian, 8 authoritarian and 4 materialist items. As shown in Table 3, the *libertarian* items cover the themes of participation, support for minorities, open-mindedness, personal freedom and freedom of speech, improving the quality of life, self-fulfillment, and self-indulgence.

The *authoritarian* items include the themes of patriotism, a strong defense, respect for authority and strong leaders, preserving traditional morals and values, conformity, and intolerance of dissenters. The *materialist* priorities were measured by the four items identifying those who place high emphasis on maintaining high economic growth, securing a high-paying job, and working hard and saving for the

Table 3. Factor Loadings for 21 Libertarian, Authoritarian, and Materialist Items on the First Unrotated Factor and for the Three-Factor Rotated Solution, Japan 1984

Items by Type	1st Unrotated Factor	Factor Loadings For		
		Three-Factor Rotated Solution	1st	2d
Libertarian items				
Personal freedom	.611	.523	-.293	.267
Freedom of speech	.530	.413	-.292	.279
Giving people more say in government decisions	.475	.548	-.117	-.098
Increasing benefits for disadvantaged	.475	.672	.032	-.047
Improving environment and quality of life	.436	.633	.073	.113
Active citizen participation in local politics	.363	.318	-.185	.040
Being open-minded to new ideas	.309	.421	.015	.040
Seeking personal fulfillment	.309	1.286	-.081	.395
Living for today and enjoying oneself	.281	1.188	-.143	.431
Materialist items				
Securing a high-paying job	.104	.131	.100	.664
Working hard and saving for the future	.002	.299	.390	.329
Maintaining high economic growth	-.114	-.116	.132	.541
Feel our society is not too materialistic	-.115	-.192	.037	.482
Authoritarian items				
Preserving traditional morals and values	-.197	.150	.468	-.009
Following custom and neighbors' expectations	-.325	.066	.592	.145
A few strong leaders better than parties	-.343	-.131	.380	.044
Respect for authority	-.351	-.106	.419	.029
Providing for strong defense forces	-.491	-.349	.399	.295
No room in Japan for dissenters	-.509	-.103	.633	-.145
Patriotism and loyalty most important	-.631	-.268	.638	-.106
Should increase defense spending	-.637	-.468	.456	.106

Note: The three-factor solution was derived from a varimax rotation.

Table 4. The Association among the Libertarian, Authoritarian, and Materialist Item Scales and between Those Scales and Several Key Demographic and Attitudinal Variables

Scales and Variables	Libertarian	Authoritarian	Combined Authoritarian- Libertarian	Materialist
Authoritarian scale	-.32			
Materialism scale	.13	.23	(-.07)	
Age	-.28	.30	-.37	(-.02)
Right-Left party identification	.28	-.39	.41	(-.04)
Right-Left self-placement	.30	-.41	.44	(-.05)

Note: All correlations are significant at the .001 level except those in parentheses, which are not significant even at the .01 level.

future and those that do not feel that their society overemphasizes material things.

As Table 3 shows, the libertarian items all load positively on the first unrotated factor and the authoritarian items all load negatively, while the four materialist items all fall in the middle, closer to a zero loading. Even more dramatically, the table demonstrates that with the exception of two libertarian items and one materialist item that have somewhat ambiguous loadings, the rotated three-factor solution neatly divides the three types of items, with the libertarian items loading on the first factor, the authoritarian items on the second factor, and the materialist items on the third. In discussing the exceptions below, it is important to note that all of the items included in each of the three reported value domains drawn in Table 3 load highly on a first unrotated factor when the analysis is limited to only those items within a given value type. The inclusion of these three items (marked ?) within their identified value domain is only brought into question because of the presence of other factors on which they also load.

Each type of item was then combined into a scale and the resulting three scales were then correlated with each other and with several key demographic and attitu-

dinal items as shown in Table 4. As expected, a strong inverse correlation was found between the libertarian and authoritarian scales (-.32). Moreover, the libertarian scale is inversely associated with age (-.28) and positively correlated with the Left ends of the Right-Left self-placement and party identification scales (.30 and .28 respectively). Conversely, the authoritarian scale is associated with old age (.30) and the political Right (-.41 and -.39). The strength of these correlations is somewhat further increased when all the libertarian and authoritarian items are combined into a single scale. In marked contrast, the materialism scale yields no significant correlations with either age or political preference. Unfortunately, the relationships with the other key demographic variable, education, could not be tested because the sample was limited to the university educated. However, the relationships reported in Table 4 are precisely those reported in my earlier study (Flanagan 1982a), only now the materialism scale is derived from the kinds of materialism items Inglehart employs (rather than on a post hoc basis from a most-important-problem question), so there can be no question of comparability. The earlier study included education and class, and we can infer that the

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relationships would be the same here if the sample had not been restricted on those dimensions.

The most important finding in Table 3 is that the security items Inglehart uses to measure materialism cluster with the authoritarian items while the economic items stand alone. The most important finding in Table 4 is that the pattern of relationships supports my argument that the materialism-nonmaterialism and authoritarian-libertarian dimensions of change are explained by different causal phenomenon and yield contrasting patterns or relationships with key demographic and political variables.

The most interesting unexpected finding in Table 4 is that materialism is positively correlated not only with the authoritarian scale (.23) but also with the libertarian scale (.13). Since I have viewed materialism as essentially an utilitarian orientation, I expected some significant correlation between the authoritarian and materialist items based on the following logic: The relative importance a respondent will place on the economic issues of the day—*inflation, depression, unemployment, further economic growth, etc.*—is a function of both the respondent's margin-of-income surplus and the extent to which the respondent perceives that his or her economic interests are being threatened by those issues. Thus new middle-class professionals in the advanced industrial democracies are likely to be nonmaterialists, both because of the substantial cushion their incomes provide them with beyond the basic necessities of life and because the skills they possess are highly valued in postindustrial economies, providing them with great job security. Conversely, the blue-collar worker is more likely to be a materialist, both because of his or her smaller income surplus and because this respondent's skills are increasingly becoming obsolete, thus heightening concern over livelihood.

The finding of positive correlations

with both of these opposite value preferences suggests that those lying towards both of the extreme ends of the authoritarian-libertarian value dimension might have other kinds of inducements beyond utilitarian evaluations. It may be that something in the values themselves that are held by the extreme form, or pure type, of authoritarian or libertarian predispose each of them towards materialist priorities. Clues as to what these motivations might be are derived from the three items in Table 3 with ambiguous loadings. The materialism item *working hard and saving for the future* actually loads slightly more heavily on the authoritarian dimensions than on the materialism dimension. Elsewhere I have argued that the values of frugality, discipline, and hard work are part of the traditional authoritarian orientation (Flanagan 1979).

One might just mark this down as a bad item, since it combines the authoritarian orientation towards diligence and the traditional "Confucian" and "protestant" work ethics with the materialist concern with economic well-being. If we look more deeply, however, we may find a *psychological materialism*; that is, the authoritarian's preoccupation with discipline and hard work, in contrast with the self-indulgent libertarian orientations, which place a high priority on leisure activities, may be related to the same heightened need for power and sense of weakness that drive one to identify with established authority figures and symbols. Thus the same insecurities that prompt one to support a strong military and law and order may also motivate hard work, frugality, and a heightened effort to maximize one's economic security to placate anxieties about one's own weaknesses and lack of power. Psychological studies have provided evidence suggesting that such authoritarian needs for power and feelings of weakness and insecurity are the product of child-rearing practices that stress discipline over affec-

tive ties, a behavioral distinction that has been shown to correlate with class and education (Adorno et al. 1950; Winter 1973). Thus, a number of lower-class, low-education authoritarians may also be psychological materialists.

Just as revealing is the loading of the two libertarian items, *seeking personal fulfillment* and *living for today and enjoying oneself*, on the materialism factor. This suggests a Yuppie type of *terminal materialism*. Some libertarians who are preoccupied with self-fulfillment and self-indulgence may place an excessive valuation on maintaining a strong economy, acquiring high-paying jobs and surrounding themselves with all the material trappings of affluence. While for some libertarians, materialism may only be a secondary instrumental priority, something to be valued because it makes other life goals possible; for others, materialism may become an end in itself—a terminal value—because of the status, self-esteem, sense of achievement, comfort, self-indulgence and other gratifications that it provides. Some libertarians, then, may be susceptible to a "terminal materialism" in which, notwithstanding their relative affluence, increasing their wealth, possessions, and consumption of the good life comes to be valued more highly than other kinds of libertarian values. Thus, for them, economic issues may take precedence over their support for the New Left issue agenda.

The diminishing-marginal-utility thesis is a powerful one for explaining why there is a higher proportion of nonmaterialists in the United States than in Colombia. However, after reaching and surpassing some level of societal and personal affluence, further gains in affluence will have little effect on altering one's value priorities, and we are left with having to explain why some upper-middle-class respondents are still materialists. Indeed there is some evidence of a countertrend in the United States running against the logic of

the diminishing-marginal-utility thesis. The American Council on Education's (1973-86) annual nationwide surveys of roughly two hundred thousand entering college freshmen, which began 20 years ago, have documented a growing emphasis on materialism. In 1966 only 45% of the respondents nationwide selected *money: being well off* as an essential or very important life goal. By 1976 this had increased to 52% and by 1986 to 74%. Undoubtedly for some of these entering freshmen, money is viewed as a strictly secondary, instrumental priority. On the other hand, it is likely to be a vital priority for others and the notion of a Yuppie type of *terminal materialism* may be useful here in explaining the failure of materialism to wither away among some of the affluent.

As shown in Figures 10-11, when the three items with ambiguous loadings are dropped and the remaining 18 items are factored again, the factor plots yield three tightly clustered and distinct value domains. The plot of the first and second factors in Figure 10 depicts a sharp differentiation of the libertarian and authoritarian items along both dimensions while the materialism items fall near the origins of both axes. The plot of the second and third factors show the libertarian and authoritarian items splitting on the second factor but lying close to the origins on the third, while the materialist items load heavily on the third factor but lie close to the origins on the second.

When the value scales are recomputed based on the smaller set of 18 items, the correlation between the libertarian and materialism scales drops from .13 to an insignificant but still positive .04. The correlation between the authoritarian and materialism scales also drops from .23 to .18, and if the two authoritarian items loading below .40 are removed, the correlation declines further to .14. The finding of a higher correlation between authoritarianism and materialism in the

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Figure 10. Two-Dimensional Plot of the Item Factor Loadings for the 18-Item Three-Factor Varimax Rotated Solution (horizontal axis = factor 1; vertical axis = factor 2)

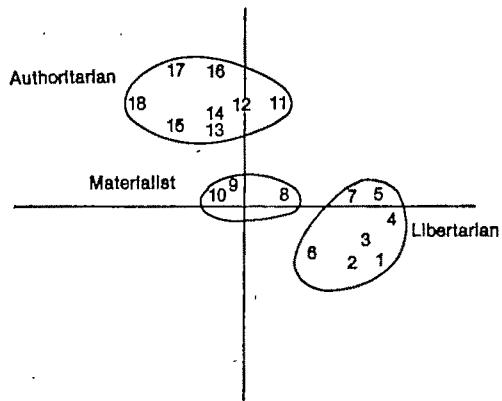
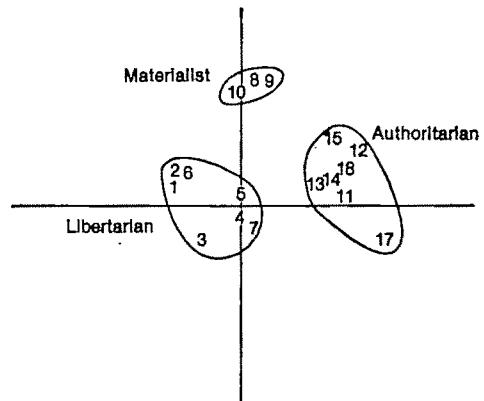


Figure 11. Two-Dimensional Plot of the Item Factor Loadings for the 18-Item Three-Factor Varimax Rotated Solution (horizontal axis = factor 2; vertical axis = factor 3)



Note: Items 1-7 are respectively the first seven libertarian items listed in Table 3, Items 8-10 are the three materialist items (omitting *working hard*), and Items 11-18 are in order of the Table 3 listing of the eight authoritarian items.

case of a class-constrained sample of university-educated respondents, where the utilitarian argument discussed above is likely to have less relevance, suggests that even among the highly educated there are still more authoritarians who are psychological materialists than there are libertarians who are terminal materialists.

Where Do We Go from Here?

Inglehart's theory of postmaterialist value change and his related research clearly constitutes one of the most important theoretical contributions in the comparative field over the last two decades. My argument here and elsewhere has been that Inglehart was correct in identifying an important value cleavage with strong relationships to vote choice and important implications for realignment. Where we differ is in my claim that he has incorrectly conceptualized and measured the value cleavage that divides the New Left

and New Right. To date, Inglehart has resisted tampering with his scale because it "works." As I have shown, however, it continues to work for the wrong reasons —namely that it is rather tapping the authoritarian-libertarian dimension while at the same time causing a forced constraint between the authoritarian and materialist items, making it very difficult to disentangle the two distinct value dimensions using his procedures.

The conceptualization of the politically salient value cleavage that divides the advanced industrial democracies has been dominated by the notion of *materialism* for too long. As the wide variety of conceptual subdimensions associated with the authoritarian-libertarian scale suggests, we now need to cast our nets more broadly to determine what clusters with what, and in this regard much work remains to be done. While the results reported above appear to provide definitive proof for my arguments, some cautions are in order. A sample that cut across all levels of educa-

tion might have yielded somewhat higher correlations between the authoritarian and materialism scales, due to the utilitarian argument. Also, in other countries cultural differences might somewhat affect what does and does not cluster within the authoritarian and libertarian factor spaces. Notwithstanding these caveats, the above evidence makes a strong case for now moving beyond Inglehart's original scale and developing two distinct scales. The two-scale approach is superior because it enables us to distinguish between the materialist-non-materialist value-priority dimension, which simply identifies whether the Old Politics economic issues or New Politics value issues are of higher salience to the respondent, and the authoritarian-libertarian dimension, which identifies whether the respondent is likely to support the New Left or New Right issue agendas.

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Notes

1. My response to Inglehart's new contribution grew out of my review of a conference paper he presented at the IPSA conference in Paris in July 1985, which in published form was divided into two articles, including the above offering and a second one published elsewhere (Inglehart and Rabier 1986). My comments, therefore, will occasionally refer to this second companion piece as well.

2. This Japan's Successor Generation survey was conducted under the sponsorship of the United States Information Agency in October and November of 1984, yielding 803 completed questionnaires of university-educated Japanese between the ages of 20 and 91. I am greatly indebted to the principal investigator of the study, James S. Marshall of the East Asia and Pacific Branch of the USIA's Office of Research, for including my values battery in the survey and making the data available to me (see Marshall 1985).

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RESEARCH NOTE

SOPHISTICATED SINCERITY: VOTING OVER ENDOGENOUS AGENDAS

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The empirical findings on whether or not legislators vote strategically are mixed. This is at least partly due to the fact that to establish any hypothesis on strategic voting, legislators' preferences need to be known, and these are typically private data. I show that under complete information, if decision making is by the amendment procedure and if the agenda is set endogenously, then sophisticated (strategic) voting over the resulting agenda is observationally equivalent to sincere voting. The voting strategies, however, are sophisticated. This fact has direct implications for empirical work on sophisticated voting.

Over the past decade, considerable theoretical attention has been devoted to voting in committees. The focus has been on two related questions. First, given an arbitrary agenda, what is the structure and consequence of strategic (sophisticated) voting and how does this differ from sincere (naive) voting? Second, given strategic voting and an arbitrary status quo, what final outcomes can a monopoly agenda setter reach by an appropriate choice of agenda? For decision making under the amendment procedure¹ with complete information, the answers to these questions are now known (see, in particular, Farquharson 1969, McKelvey and Niemi 1978, and Moulin 1979 on the first problem; and Banks 1985, Miller 1980, and Shepsle and Weingast 1984 on the second).

In the wake of the theoretical advances, empirical work on legislators' voting behavior has become increasingly directed toward trying to discover whether legislators do in fact vote strategically (Bjurulf and Niemi 1978;

Denzau, Riker, and Shepsle 1985; Enelow 1981; Enelow and Koehler 1980; Krehbiel and Rivers 1985). For if they do, then any legislator's voting behavior cannot be used unequivocally as an instrument to reveal his or her (legislative) preferences. The findings are mixed: this is at least partly because, as Krehbiel and Rivers remark, "to assess the voting strategies of [legislators] one needs to know [their] preferences" (1985, 3), and preferences are private information. Bjurulf and Niemi and Enelow (and Koehler) find evidence of strategic voting; Denzau, Riker, and Shepsle examine a case in which some legislators appear to vote strategically and others do not; and Krehbiel and Rivers are unable to reject the hypothesis of sincere voting.

To account for legislators not voting strategically to promote their *prima facie* interests, Denzau, Riker, and Shepsle extend the domain of individual preferences to "home-style" constituency concerns. With this wider framework, they show that there are situations in which it

is rational not to vote strategically: the cost of doing so in terms of the probability of reelection is too high. Because preferences and motives are private information, constituents are unable to disentangle the reasons why a legislator voted in any particular way. Since they can never be sure whether their representative was voting strategically or simply against their interests, the legislator faces a risk, when deciding to vote strategically, that constituents will believe the legislator is voting against their interests. The evidence that "home-style" considerations are important in voting is strong (Kingdon 1973). But the Denzau, Riker, and Shepsle explanation is problematic in that it makes it even more difficult to test any hypothesis of strategic voting. If a legislator is observed to vote sincerely when there exists an opportunity to be strategic, it is *prima facie* impossible to reject the hypothesis of sophisticated behavior (relative to the broader domain of preferences): the unobserved "home-style" costs may be too high.

Krehbiel and Rivers (1985) estimate legislators' preferences and find that sophisticated and sincere voting coincide for the issue they study. Consequently, "failure to reject sincere voting does not automatically imply rejection of sophisticated voting" (p. 26). They conjecture that, since the issue was one-dimensional and a median most-preferred outcome existed, a process of amendments and counteramendments resulted in this median being proposed. Although the existence of a median on the agenda does not, in and of itself, guarantee that sincere and sophisticated voting will coincide, the suggestion is that the locus of strategic behavior may be more at the agenda-setting stage of the process than at the subsequent voting stage.

I show that this suggestion generalizes. If decision making is by the amendment procedure, if the agenda is set endogenously, and if information is complete,

then sophisticated voting over the resulting agenda is observationally equivalent to sincere voting. The voting strategies that support the observed voting behavior, however, are sophisticated in the classical sense. The intuition behind the result is straightforward. Given an arbitrary agenda, a legislator knows that rational committee members will vote strategically. So the legislator takes these voting strategies into account when selecting a proposal or amendment to put on the agenda: proposing is "sophisticated." And given the structure of sophisticated voting on binary agendas, this results in choosing proposals and amendments that defeat previously proposed alternatives under sincere voting. The class of agenda-setting procedures explicitly covered by the result is quite broad. Effectively, any mechanism satisfying the following three criteria fall within it: (1) legislators are given at most one opportunity to offer an alternative; (2) proposals are made openly within the committee; and (3) the agenda is set sequentially, that is, the s^{th} proposal on the agenda can be offered only after the first $(s - 1)$ proposals have been fixed and before the $(s + 1)$ -to-final proposals are volunteered.

In the next section, the model and the result, which is technically quite trivial, are developed formally. A final section offers some concluding remarks.

Model and Result

The model is fairly standard. A committee N consists of an odd number, $n \geq 3$, of individuals, $N = \{1, 2, \dots, n\}$. Each individual $i \in N$ has preferences defined on the k -dimensional issue space, \mathbb{R}^k : i 's preferences are assumed representable by a continuous and strictly quasi-concave utility function, $u_i: \mathbb{R}^k \rightarrow \mathbb{R}$. The committee makes a choice from the alternative space, $X (\mathbb{R}^k \supseteq X)$, using simple majority voting over an agenda based on

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the amendment procedure. An agenda of length t is a t -tuple $y = (y_1, y_2, \dots, y_t) \in X^t$. The agenda is based on the amendment procedure if the sequence of votes over the agenda is given by first putting y_t against y_{t-1} ; putting the winner of this contest against y_{t-2} ; and so on until y_1 is paired against the winning alternative in the $(t - 2)^{\text{th}}$ contest: the winner of this last contest is the final outcome. Let $\Sigma(y)$ be the voting tree defined by this process. Hereafter, leave the dependency of $\Sigma(\cdot)$ on y implicit, and write $\Sigma(y) \equiv \Sigma$.

The agenda is determined endogenously. The mechanism assumed is as follows.² If there is a historically given status quo, labeled $b(0)$, then this occupies the position of y_1 in any agenda. Thereafter, alternatives are offered by eligible committee members in a sequence, π . Although all committee members have voting rights, it is not assumed that all committee members are necessarily eligible to make proposals: agenda setting may be confined to a subcommittee. Let $M, N \supseteq M$, be the set of legislators eligible to offer proposals and amendments. Let $|M| = m$.

The first legislator under π is chosen by some (possibly degenerate) lottery over M . Let $p(j = i_s)$ be the probability in this lottery that legislator $j \in M$ is the first-ranked individual in π . Assume $p(j = i_s) \in [0, 1]$, $\forall j \in M$, and $\sum_M p(\cdot) = 1$. Once the first-ranked individual is chosen, he or she determines the second alternative on the agenda, y_2 : call this proposal $b(1)$. Once $b(1)$ is revealed, the second individual under π is selected by a second lottery from the remaining $m - 1$ eligible legislators, and this individual then determines y_3 , denoted $b(2)$. In general, given the sub-sequence $\pi_{s-1} = (i_1, i_2, \dots, i_{s-1})$, where i_r is the r^{th} -ranked individual in π , and given proposals $\{b(0), \dots, b(s-1)\}$, the s^{th} -ranked legislator is chosen by lottery from legislators in $M \setminus \{i_1, \dots, i_{s-1}\}$, $s = 2, \dots, m$. For any $j \in M \setminus \{i_1, \dots, i_{s-1}\}$, the probability that j is selected to

make proposal $b(s)$ is given by $p(j = i_s | \pi_{s-1}) \in [0, 1]$: by assumption, $\sum_M p(\cdot | \cdot) = 1$ (so that $p(j = i_s | \pi_{s-1}) > 0$ for at least some $j \in M \setminus \{i_1, \dots, i_{s-1}\}$). So while the lottery at any stage s can depend on the subsequence π_{s-1} , it is presumed independent of the proposals $\{b(0), \dots, b(s-1)\}$. By convention, write $p(j = i_s) \equiv p(j = i_s | \pi_0)$, $\forall j \in M$. By definition, $\pi = (i_1, \dots, i_m)$ and $\forall j \in M$, $\exists s \in \{1, \dots, m\}$ such that $j = i_s$.

All the probabilities are common knowledge and can be used at each stage of the process to compute the likelihood of any given sequence, π , occurring. Let $\mu(\pi | \pi_s)$ be the probability that the de facto sequence of proposers is π , given the realized subsequence π_s , $s = 0, \dots, m - 1$. For example, if $M = \{1, 2, 3, 4, 5\}$ and $\pi_2 = (4, 2)$, then

$$\begin{aligned} \mu[(4, 2, 1, 3, 5) | \pi_2] &= p[1 = i_3 | (4, 2)] \\ &\cdot p[3 = i_4 | (4, 2, 1)] \\ &\cdot p[5 = i_5 | (4, 2, 1, 3)] \\ &= p[1 = i_3 | (4, 2)] \\ &\cdot p[3 = i_4 | (4, 2, 1)]. \end{aligned}$$

Evidently, $\mu(\pi | \pi_{m-1}) \in \{0, 1\}$ for all π .

At any stage $s > 1$ in the agenda-setting process, if the s^{th} legislator (i_s) does not wish to offer any alternative at all or any not already proposed, then $b(s) = \emptyset$ (where, by an abuse of notation, $\emptyset \in X$): under the amendment procedure, this captures the parliamentary rule preventing reconsideration of previously eliminated alternatives. The final agenda, then, is at most of length $m + 1 \leq n + 1$ and is given by $b = [b(0), b(1), \dots, b(m)]$. Once b is determined, voting proceeds according to the amendment procedure to arrive at the committee decision.

To save on notation, given any subsequence π_s , $s \leq m$, relabel individuals in N so that $i_r = r$, $\forall r = 1, \dots, s$. For every $s = 1, \dots, m$, let $B_s = \{b(0), \dots, b(s-1)\}$. A (pure) proposal strategy for indi-

vidual $i \in M$ is then a function: $b_i: X^i \rightarrow (X \setminus B_i) \cup \emptyset \equiv \beta_i$. For legislators $j \in N \setminus M$, define the (vacuous) proposal strategy, b_j , by setting $\beta_j \equiv \emptyset$.

Let $|\Sigma|$ be the number of possible pairwise contests identified by branches of the voting tree, Σ . A (*pure*) voting strategy for individual $i \in N$ is a function: $v_i: \Sigma \rightarrow \{0, 1\}^{|\Sigma|}$. In any pairwise contest, a value 1 denotes a vote for the alternative proposed later in the agenda (i.e., with the larger index under π), and a value of 0 denotes a vote for the alternative proposed earlier.

Since a legislator will cast at most m votes during the voting process, i 's voting strategy can be decomposed, namely, $v_i = \{v_{it} \mid t = 1, \dots, m\}$, where t indexes the voting stage ($t = m$ being the final vote). Legislator i votes *sincerely* at stage t , $t = 1, \dots, m$, if and only if for any $j > m - t$,

$$u_i[b(j)] > (<) u_i[b(m - t)] \\ \rightarrow v_{it}([b(j), b(m - t)]) = 1 (0).$$

If ever a legislator is indifferent and sincere at stage t , assume he or she votes surely for the alternative with the higher index under π (with endogenous agenda setting, this turns out to be the unique equilibrium strategy [Banks and Gasmi 1986]). Legislator i is said to vote *sincerely* if and only if i votes sincerely at every stage $t = 1, \dots, m$.

A (*pure*) strategy for legislator i is thus an ordered pair, $\sigma_i = (b_i, v_i)$, $i \in N$. Let $\sigma = (\sigma_i)_N$ and $\sigma_{-i} = (\sigma_1, \dots, \sigma_{i-1}, \sigma_{i+1}, \dots, \sigma_n)$. Given any list of strategies, σ , let $y(\sigma)$ be the final outcome of the decision-making process. The noncooperative solution concept used for the extensive-form committee game induced by the structure described above can now be defined. An *equilibrium* is a list of strategies $\sigma^* = (\sigma_i^*)_N$ such that, $\forall i \in N$:

1. $b(i)^* = b_i^*(\cdot)$ maximizes $E u_i[y(b, v_i^*)]$, $\sigma_{-i}^*]]$ over β_i , where the expectation

operator E is with respect to the sequence π .

2. $v_i^* = \{v_{it}^*\}$ is such that $\forall t = 1, \dots, m$, conditional on i being pivotal at each stage t , v_{it}^* maximizes $u_i[y((b_i^*, v, \sigma_{-i}^*))]$ over $\{0, 1\}^{|\Sigma|}$.

Because there is complete information and decision making is by the amendment procedure, in equilibrium legislators will adopt sophisticated voting strategies in the sense of Farquharson (1969), and McKelvey and Niemi (1978). So, taking this as given, it is enough to consider the agenda-setting stage of the game.

For any policy $x \in X$, define the *win set* of x , $W(x) = \{y \in X \mid \{i: u_i(x)\} > n/2\}$, and let $W^c(x)$ be its closure. If $W(x) = \emptyset$ for any alternative x , x is a *Condorcet winner*. By the assumptions on individual preferences, for any alternative x , $W(x)$ is open. Therefore, if ever an individual is constrained to choose an alternative in $W(x)$ and $W(x)$ is nonempty, i 's maximization problem is not well defined. However, by the observation made earlier regarding equilibrium voting strategies for pairs of alternatives over which an individual is indifferent, the statement, "Maximize over $W(x)$ " can always be replaced (without loss of generality) by the statement, "Maximize over $W^c(x)$," which is well defined. Hereafter, this is left implicit.

THEOREM. Suppose there is no Condorcet winner in X . Then there exists a unique equilibrium σ^* to the game, and the observed voting behavior of all legislators in equilibrium is sincere.

Proof. To save on notation (and without loss of generality), set $M = N$. For any legislator j , define, $Q(j) = \{\cap W[b(s)]\} \cup \emptyset$, where the intersection is taken over all $b(s) \in B_j$. Given π_{n-1} and $B_n = \{b(0), \dots, b(n-1)\}$, legislator i_n offers the final proposal. Without loss of generality, set $i_n = n$. Hence,

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$$b_n^*(B_n) \in \{b(n) \in \beta_n \mid b(n) \in \operatorname{argmax}_{Q(n)} u_n(b)\}$$

is, given sophisticated voting, a best response for legislator n . Given π_{n-2} , the individual offering the penultimate proposal knows surely the identity of the final proposer, i_n ($= n$, by assumption). Moreover, n 's proposal strategy is uniquely defined. Again without loss of generality, label N so that $i_{n-1} = n - 1$. Then legislator $n - 1$ can do no better than to adopt

$$b_{n-1}^*(B_{n-1}) \in \{b(n-1) \in \beta_{n-1} \mid b(n-1) \in \operatorname{argmax}_{Q(n-1)} u_{n-1}[b_n^*(\{b\} \cup B_{n-1})]\}.$$

Given π_{n-3} , there are two possible de facto sequences π that can occur, depending on the outcome of the final lottery over $\{n, n - 1\}$ to determine who proposes the penultimate alternative on the agenda. But whichever sequence occurs, the proposal strategies of the remaining proposers are well defined. Therefore, individual i_{n-2} (identified by assumption with $n - 2$) has a best response to adopt,

$$b_{n-2}^*(B_{n-2}) \in \{b(n-2) \in \beta_{n-2} \mid b(n-2) \in \operatorname{argmax}_{Q(n-2)} Eu_{n-2}(\cdot)\}$$

where

$$Eu_{n-2}(\cdot) = \sum_{\pi} \mu(\pi \mid \pi_{n-3}) \cdot u_{n-2}[b_{m(\pi)}^*(b_{(m-1)(\pi)}^*(\{b\} \cup B_{n-2}))]$$

and $m(\pi)$ is the final proposer under π , and so on. Proceeding iteratively in the obvious manner, define analogous proposal strategies for all i_{n-3}, \dots, i_1 . By construction, this set of proposal strategies is well defined, constitutes an equilibrium, and is the unique such set. However, by sophisticated voting, for some

$Q(j)$, j 's best payoff can be invariant across several feasible proposals, or none. Therefore, there may exist several equilibrium agendas induced by σ^* .

In the equilibrium, voting strategies are sophisticated. Consider voting behavior along the equilibrium path for any agenda b^* , derived from the specified proposal strategies. At the final nonvacuous voting stage of this path—that is, $b(s)$ versus $b(0)$, $n \geq s \geq 1$ and $b(s) \neq \emptyset$ —all legislators surely vote sincerely. Because there is no Condorcet winner, $W[b(0)] \neq \emptyset$, and such a stage exists. By construction, $b(s) \in W[b(0)]$, so $b(s) = y(\sigma^*)$ is the final outcome. Consider the penultimate voting stage in which $b(1)$ is put against an alternative $b(j) \neq \emptyset$, $n \geq j > 2$. Then, $y(\sigma^*) \in \{b(j), b(1)\}$. If there is no such alternative, we are done. So suppose there is a nonvacuous alternative. If $y(\sigma^*) = b(1)$, then, by the proposal strategies, $W[b(1)] = \emptyset$ implying $b(1)$ is a Condorcet winner: contradiction. Hence, $y(\sigma^*) = b(j)$. By construction, $b(j) \in Q(j)$. Clearly, any individual $i \in N$ for whom $u_i[b(j)] > \max\{u_i[b(1)], u_i[b(0)]\}$ will vote sincerely at this stage. Similarly, since $b(1)$ and $b(j)$ are in $W[b(0)]$, any individual i for whom $\min\{u_i[b(1)], u_i[b(0)]\} > u_i[b(j)]$ has sincere voting as a weakly dominant strategy here. Suppose $i \in N$ is such that $u_i[b(1)] > u_i[b(j)] > u_i[b(0)]$. Then because $b(1) \in W[b(0)]$ by construction, i has a weakly dominant strategy to vote sincerely. Finally, suppose $i \in N$ is such that $u_i[b(0)] > u_i[b(j)] > u_i[b(1)]$. By construction, $b(j) \in W[b(1)] \cap W[b(0)]$ and $b(1) \in W[b(0)]$. So it is again a weakly dominant strategy for i to vote sincerely at this stage. Hence sincere voting over $\{b(j), b(1)\}$ is a weakly dominant strategy for all individuals. By sincere voting at the final stage, definition of $b(j)$ implies all legislators vote sincerely at this penultimate stage. Now apply an obvious inductive argument to complete the proof. QED

COROLLARY. Suppose there exists a Con-

Condorcet winner, x^* , in X . Then there exists an equilibrium σ^* to the game, which is unique up to the proposal of x^* . The observed voting behavior of all legislators in equilibrium is sincere, except possibly at voting stages up to and including the first vote involving x^* .

Proof. For any legislator $j \in M$ such that $Q(j) \neq \emptyset$, the proposal strategy specified in the proof of the theorem is the best response. Suppose there exists $i \in M$ and $b(j) \in B_i$ such that $W[b(j)] = \emptyset$. Then $Q(i) = \emptyset$. Set $b(j) \equiv x^*$. By a theorem of McKelvey and Niemi (1978), x^* will be the final outcome under sophisticated voting whatever alternative i elects to propose. Hence, any proposal strategy is a best response for i . In particular, if $b(i) \neq \emptyset$ and $b(i) \in \cap \{W[b(t)] \mid b(t) \in B_i \text{ and } W[b(t)] \neq \emptyset\}$, then sophisticated voting along the equilibrium path need not be sincere. Moreover, by the argument for the theorem, a necessary condition for voting not to be sincere at some stage for some legislator is that there be at least one such proposal $b(i)$. The corollary now follows from the theorem. QED

The corollary identifies the logically possible exception to the claim that, with endogenous agenda setting, sophisticated voting is always observationally equivalent to sincere voting. Having said this, three points should be emphasized. First, the conclusion of the theorem goes through if a Condorcet winner x^* is not proposed, even though it exists. Second, if x^* is placed on the agenda, then making no further proposals is equilibrium behavior, as is offering alternatives that can defeat (under sincere voting) all those previously proposed other than x^* . In both these cases, voting along the entire equilibrium path will be observationally equivalent to sincere voting. Thus, even when there exists a Condorcet winner x^* , there always exist equilibria to the game, in which sincere and sophisticated voting

coincide along the equilibrium path; and these equilibria are unique up to the proposal of x^* . Third, in the spatial context at least, the existence of a Condorcet winner is generically confined to one-dimensional issues.

Finally, note that if there exists a Condorcet winner x^* in X and if all legislators are eligible to propose alternatives ($M = N$), then x^* is surely the outcome to the agenda game above. To see this, simply note that by Plott's (1967) theorem and n -odd, there exists an individual $j \in N$ with ideal point x^* . Therefore x^* will be placed on the agenda.

Conclusion

If committee decision making is by the amendment procedure and the agenda is determined endogenously (either by a subcommittee or by the committee of the whole), then the theorem offers an explanation for why legislators are observed to vote sincerely. Furthermore, along the equilibrium path, no individual will vote against his or her own proposal should this be put against an alternative offered earlier in the agenda-setting sequence.³ This explanation does not rest on widening the domain of legislator preferences.⁴ Nevertheless, it is worth reiterating that the observed behavior is supported by sophisticated voting strategies. In empirical work attempting to test for strategic voting, therefore, it is important to determine whether or not agenda setting is endogenous. If it is, then, *ceteris paribus*, the theorem predicts that a hypothesis of sincere voting will not be rejected.

The key to the result is that sophisticated voting over an agenda induces legislators to confine their proposals to alternatives that can beat—under sincere voting—the proposals offered earlier in the agenda-setting process. Since the formal structure of sophisticated voting is invariant across the class of binary agen-

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das (McKelvey and Niemi 1978), this suggests that the theorem holds not only for the amendment procedure, but also for any binary agenda and (binary) agenda-setting mechanism.

Notes

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1. Formal definitions of the amendment procedure and related concepts are given in the next section.

2. For related models of endogenous agenda setting, see, for example, Banks and Gasmi 1986, McKelvey 1986, Miller 1985, and Miller, Grofman, and Feld 1986.

3. Unfortunately, the observation that legislators behave as if constrained to vote for their own proposals against those offered previously has not been formally documented. It is nevertheless a commonplace among students of Congress. In the case where a Condorcet winner, x^* , has been placed on the agenda, a legislator can ensure against having to vote for alternatives previously placed on the agenda against his or her own proposal—if this is made after the appearance of x^* on the agenda—only by proposing nothing or by offering an alternative that beats all previously volunteered proposals other than x^* . (Both constitute equilibrium strategies here.) Thus, if legislators, for whatever reasons, do feel obliged to vote for their own proposals against those offered previously, the logically possible exception (identified in the corollary to the theorem) to the claim that sophisticated and sincere voting observationally coincide with endogenous agenda setting vanishes.

4. It has been rightly observed that, just as the model here excludes "home-style" reasons for voting behavior, it excludes similar ("position-taking") reasons for proposal behavior. These may well prove important, giving rise to a distinct model of strategic agenda setting. Having said this, there is in fact some room for position taking in the model here, as the corollary makes clear. If there is an underlying Condorcet winner (x^*) and if this is put on the agenda before all eligible committee members have made a proposal, then subsequent proposers can "position take," since whatever they offer will lose to x^* under sophisticated voting.

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The following articles, controversies, and research notes have been tentatively scheduled for publication in the March 1988 issue:

Robert A. Bernstein, Gerald C. Wright, Jr., and Michael Berkman. Policy Moderation and Reelection Strategy. A Controversy.

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Norman Schofield, Bernard Grofman, and Scott L. Feld. The Core and the Stability of Group Choice in Spatial Voting Games.

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REVIEW ESSAYS

Women and Politics in Western Europe.
Edited by Sylvia Bashevkin (Totowa, NJ:
Frank Cass, 1986. 101p. \$29.50).

Women of Europe: Women MEPs and Equality Policy. By Elizabeth Vallance and Elizabeth Davies (New York: Cambridge University Press, 1986. x, 180p. \$37.50).

Women, State, and Party in Eastern Europe.
Edited by Sharon L. Wolchik and Alfred G.
Meyer (Durham, NC: Duke University
Press, 1985. xiv, 453p. \$42.50, cloth;
\$16.95, paper).

The study of women and the politics of individual European nations has been less developed than the study of women in U.S. politics for a variety of reasons. Truly comparative studies of women and politics have been even rarer. Yet more difficult to find are analyses of women in international politics and organizations. These three books, therefore, are bound to make a contribution to our knowledge despite some unevenness in their quality.

Women and Politics in Western Europe is a reprint of a special issue of *Western European Politics*. It begins with an essay on the women's movement and the state by Jane Jenson that claims to be general in its focus but actually focuses on France. Of the remaining articles, one deals with the connection between the Italian women's movement and Communist party by Karen Beckwith; one is on women, the women's movement, and the French Socialist government (Wayne Northcutt and Jeffra Flaitz); and three are more comparative in nature, including Lawrence Mayer and Roland Smith's piece on electoral behavior and religiosity in Italy, West Germany, and the Netherlands, Ingunn Norderval's article on politics in Scandinavia, and Pippa Norris's study of legislative participation in Western Europe.

Women of Europe focuses on women and the European Community (EC) and European Parliament. Based on documentary and interview analysis, it considers both the role of women as participants in EC politics and European legislation as it affects women. The book provides necessary background material on

the structure and processes of the EC and its relationship to member nations.

Women, State, and Party in Eastern Europe is an anthology based on papers presented to a 1981 conference on women in Eastern Europe that includes articles by both U.S. and Eastern European scholars. It is divided into five sections. Part 1, "Conditioning Factors," includes one article on Marxist and feminist theory (Meyer) and one on development and transformation (Wolchik). Part 2, on the pre-Communist period, includes two articles on Czech women (Karen Freeze and Bruce M. Garver), one on Ukrainian feminism in Poland (Martha Bohachevsky-Chomiak), and one on peasant women in Croatia (Mary E. Reed). Part 3, "Women and Politics," includes case studies of Romania (Mary Ellen Fischer) and Albania (John Kolstø) and comparative studies of Romania and Poland (Daniel B. Nelson) and Czechoslovakia and Poland (Barbara Jancar). Part 4, "Women and Work," includes work on Hungary (Rozsa Kulcar, Ivan Volgyes), Yugoslavia (Silva Meznaric, Susan L. Woodward), and Czechoslovakia (Alena Heitlinger) and two comparative studies (Robert J. McIntyre and Bogdan Mieczkowski). Finally, part 5, "Women's Voices," looks at Poland (Renata Sieminska), Romania (Gail Kligman) and East Germany (Dorothy Rosenberg).

The research presented in *Women, State, and Party* employs a rich variety of sources and approaches, including theoretical treatments; historical and documentary evidence; census materials; surveys; and literature, song, and ritual explorations. The major sections of the book are introduced by synthesizing essays by Wolchik.

These three books, separately and together, raise a series of questions that are or should be central in the study of women and politics. All three include policy change as a major theme and provide interesting evidence regarding the relative roles of political organization mobilized on the basis of feminist politics and women's interests and that mobilized on the basis of other interests. It has long been noted that some of the most important changes in women's policy takes place in the absence of a

strong feminist movement. This theme is taken up most directly in the two anthologies. Both raise the particularly interesting question of the uses "women's policy" serves for state and party. A related question, also a theme in the anthologies, is the organizational relationship between women's or feminist groups and party organizations. One wishes, however, that more attention had been paid in both volumes to distinguishing carefully between women's groups and feminist groups.

Both *Women of Europe* and *Women, State, and Party in Eastern Europe* focus our attention on one of the questions that is most unfortunately neglected elsewhere: the role of international politics in gender relations and policy. The agencies of the EC have gained importance in affecting the condition of women in member nations; we must be grateful to Vallance and Davies for taking up this topic. Many of the articles in *Women, State, and Party* underscore the need to recognize an international politics of gender in Eastern Europe, not just because of the relationship between the Soviet Union and Eastern European states, but because of the historical relationships among Eastern European communities.

The link between religion and gender politics is also a subtheme in both Bashevkin's anthology and Wolchik and Meyer's. Whether the gender relevance of politics and religion is viewed through the lens of the affinity between religion and some political parties of the West, as treated as Bashevkin's book, or through struggles between religion and state, as is often the case in Eastern Europe, it is a problem that needs more work. These two books serve as a useful corrective to feminist scholars whose understanding of gender, religion, and politics has been exclusively embedded in the peculiar relationship of religion to politics in the United States.

Each book, of course, has its own merits and problems. *Women and Politics in Western Europe* is uneven in quality. Its particular high points are Beckwith's rich article on the women's movement and party in Italy and the comparative syntheses of Scandinavia by Norderval and Western Europe by Norris. The most serious criticism is owed to the publisher: Frank Cass and Company should note that readers grow weary of paying 30 dollars for hundred-page books, especially when they are

reprints of special issues of journals already published.

Women of Europe is a pathbreaking book because of its topic, but some of the research methods are disappointing. The book is based in part on interviews, but there is no discussion of how the respondents were selected or how the interviews were analyzed. One is often left with no sense that the choice was systematic or representative of anything in particular. There is considerable use of anecdotal reporting, which leaves the scholarly reader with no confidence in what observations to accept and what not to accept. There is a considerable literature on gender and politics available that should have been used but was barely touched. The study is largely descriptive rather than analytical or theoretical. Finally, Cambridge University Press must share the bouquet of onions for the price of the book.

Women, State, and Party in Eastern Europe is a very good book. The range of methods and topics makes one forgive a certain unevenness in quality. This book handles the tie between theory and ideology on the one hand and practice on the other in a way the other two books do not. It usefully joins historical and contemporary research. The existence between two covers of some articles based on rich and systematic Eastern European demographic and survey data (see especially the articles on production and reproduction) and one on oral poetry and ritual in Romania that is both provocative and lovely to read is impressive. It is also good to see people based in the United States and Eastern Europe cooperating in scholarship that deserves the name.

These three books, then, each deserve to be read on their own terms. But even more, they should be read together.

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Book Reviews: Review Essays

Why We Lost the ERA. By Jane J. Mansbridge (Chicago: University of Chicago Press, 1986. xii, 327p. \$30.00, cloth; \$9.95, paper).

Why ERA Failed: Politics, Women's Rights, and the Amending Process of the Constitution. By Mary Frances Berry (Bloomington: Indiana University Press, 1986. 147p. \$12.95).

Rights of Passage: The Past and Future of the ERA. Edited by Joan Hoff-Wilson (Bloomington: Indiana University Press, 1986. xx, 140p. \$22.50, cloth; \$6.95, paper).

Serious attempts to amend the U.S. Constitution are rare. Even rarer are attempts to amend the Constitution that prove unsuccessful despite the mobilization of thousands of people and the investment of hundreds of thousands of dollars. On this basis alone, the proposed Equal Rights Amendment to the Constitution merits serious scholarly attention. However, the struggle over the ERA is important not only because of the insights it can offer about the amending process but also because of the central role it has played in one of the most significant social movements in U.S. history. Indeed, the ERA became the major focal point for the U.S. women's movement during the late 1970s and early 1980s.

Individually and collectively, these three books represent an important first step toward understanding the history and struggle surrounding the ERA. While all of the authors agree that in some ways the unsuccessful attempt to ratify the ERA was worth the effort, the three books take different approaches and offer distinctive explanations for the ultimate failure of the proposed amendment.

Jane Mansbridge argues that the ERA failed because a majority of citizens did not want major changes in gender roles. Both sides in the ERA dispute exaggerated the probable effects of the amendment, which Mansbridge concludes would have had little short-term impact on the status of women. As a result, many state legislators came to fear that the ERA might lead to major substantive changes, and they were reluctant to give the Supreme Court new language that might lead to more progressive decisions in the area of gender relations.

Mansbridge claims that the polarization of views evident in the ERA campaign and the exaggeration of the probable effects of the pro-

posed amendment stemmed largely from the voluntary nature of movement participation. The need to mobilize volunteers and the reliance on their efforts fostered ideological purity, decisions by accretion, and lack of accountability, which ultimately worked against the proponents' goal of ratification. The positions taken by ERA proponents on the issues of women in combat and the relationship of abortion to the ERA illustrate these tendencies. Mansbridge suggests that feminists might have won more support from legislators if they had argued for a "deferential" interpretation on the question of whether the ERA would require women to be sent into combat. However, the argument that the war-powers clauses of the Constitution would lead the Supreme Court to defer to military judgment in military matters was never really seriously considered within the pro-ERA ranks. Rather, proponents automatically opted for an "egalitarian" interpretation, consistent with their commitment to the general principle that the law should be gender blind, and argued that women should be sent into combat on the same basis as men.

On the abortion issue, the relatively autonomous actions of feminist lawyers, who were not accountable to national leadership, were partially responsible for linking abortion to the ERA. In several states feminist attorneys brought suits arguing that restrictions on public funding of abortions were unconstitutional under due-process and equal-protection clauses of state constitutions and under state ERAs. The arguments put forward by feminist lawyers in state courts made it more difficult for feminists in unratified states to argue convincingly that a federal ERA would not require federal funding for abortion. Although Mansbridge claims that tendencies toward ideological purity, decisions by accretion, and lack of accountability can be countered by institutional arrangements that foster sustained internal dialogue within organizations, it is difficult to imagine a social movement that would not show these tendencies.

The major problem with Mansbridge's intriguing and provocative book is that it appears on the surface to be a more comprehensive and nationwide study of the ERA ratification effort than it really is. One has to search for a description of the study's methodology, which is found in a footnote to the

preface. Although the author conducted almost a hundred interviews, the majority of these were with activists and legislators involved in the Illinois campaign. Of the interviews conducted with individuals outside Illinois, a significant number were with lawyers, and indeed one of the great strengths of this book is the insight it provides into the thinking and actions of attorneys involved in the ERA struggle. Despite Mansbridge's claim that the patterns and processes she observed in Illinois, the only nonsouthern state apart from Mormon Utah to fail to ratify the amendment, were "probably quite typical" (p. 177), one cannot help wondering if and how the book might have been different had her interviews focused primarily on the ERA struggle in Utah or Florida or Virginia. Despite an occasional quote from National Organization of Women President Eleanor Smeal, the perspective of national leaders in the ERA movement also seems largely absent from the book. Thus, *Why We Lost the ERA* is most appropriately read as an in-depth study of the Illinois ratification effort framed in the larger context of the national struggle over the ERA, and as such it makes an important contribution to our understanding of the politics of the ERA and of social movements generally.

Mary Frances Berry adopts an approach to examining the ERA quite different from that of Mansbridge. Berry's book places the ERA in historical perspective by comparing the circumstances surrounding the attempt to ratify the ERA with the circumstances surrounding past attempts, both successful and unsuccessful, to amend the Constitution. Specifically, she compares the ERA campaign with attempts to add to the Constitution amendments on the status of blacks, an income tax, prohibition, woman suffrage, and child labor.

Berry maintains that the defeat of the ERA was predictable when examined in historical perspective. The framers of the Constitution intentionally made it difficult to amend the document. Successful amendments have required a consensus that constitutional change is necessary not only at the national level but also within enough states to achieve ratification. Successful attempts to amend the Constitution have taken place during eras of reform, not reaction, and they have required a widespread perception that a social problem

could not be remedied without constitutional change.

Proponents of the ERA were successful in building the national consensus necessary to obtain congressional passage of the amendment in 1972. However, proponents did too little too late in trying to build a state-by-state consensus. Moreover, in the decade following congressional passage of the ERA, the Supreme Court broadened its protection of women under the Fourteenth Amendment, making it harder for proponents to demonstrate the necessity of the amendment. Support for the ERA actually eroded during the final stages of the ratification campaign.

Berry seems more optimistic than Mansbridge about the future of the ERA. Unlike Mansbridge, she holds out the possibility that the ERA might someday be added to the Constitution. To this end, she concludes her book with one paragraph of general advice for ERA proponents based on her analysis of how and why past amendments succeeded, which leaves the reader longing for a more detailed and expansive examination of the implications of her argument for the future of the ERA. Nevertheless, Berry's scholarly caution does not seriously detract from the important and compelling argument of her book.

Like Berry's book, the goal of the volume edited by Joan Hoff-Wilson, clearly intended as a text for classroom use, is to place the defeat of ERA in historical perspective. An impressive array of historians, political scientists, and other scholars engaged in relevant research examine the origins of the ERA, the reasons for its defeat, and its legal and political impact. The section on origins includes informative essays on Alice Paul, who drafted the ERA and had it introduced into Congress in 1923, and Florence Kelley, a politically active social reformer who opposed the ERA. The defeat of the ERA is examined from many perspectives: as a cultural conflict over the meaning of womanhood, as a non-zero-sum game, as the result of proponents' reluctance to use civil disobedience and nonviolent direct action, and as the product of supporters' failure to devise effective persuasive appeals.

Unlike many edited volumes, the editor's introductions to this book and to its various sections not only link together the essays of the various contributors but also make an im-

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tant contribution in their own right. Hoff-Wilson discusses the difference between feminism based on "radical individualism," as represented by ERA, and "relational" feminism, which seeks to preserve differences between women and men. She notes that many contemporary feminist scholars are now questioning whether equality based on individualistic and male notions of rights should be the major goal of contemporary feminism. Somehow, contemporary feminism must find a way to bridge the gap between these two distinct strains of feminism.

The books by Mansbridge and Berry are "must" reading for scholars interested in social

movements, women, and politics, or the amending process. All three books are also potentially useful as classroom texts. Mansbridge's book would be particularly appropriate for courses on social movements, Berry's book should be considered as a supplementary text for courses on the legislative process, and the Hoff-Wilson reader would seem best suited for courses on twentieth-century U.S. political history. All three books deserve consideration for use in courses on women and politics.

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POLITICAL THEORY

Strong Democracy: Participatory Politics for a New Age. By Benjamin R. Barber (Berkeley: University of California Press, 1984. xvi, 320p. \$9.95, paper).

In *Strong Democracy* Benjamin Barber argues powerfully for a government in which "all of the people govern themselves in at least some public matters at least some of the time" (p. xiv). With verve, style, passion, and insight, Barber explains how this ideal is possible, why we have never practiced it, and what conceptual and practical innovations might make it work.

Rejecting the "liberal" idea of a natural, pre-political state whose inhabitants are endowed with liberty, equality, and rights, Barber insists, correctly, that we acquire these goods through the process of governing ourselves in common. Rejecting as well the static, aggregative liberal conception of citizenship, Barber insists that citizens become capable of common purpose through the process of common governance rather than through the simple coincidence of preexisting interests. Such a politics involves activity, energy, and work. It involves institutions that help people create "public ends where there were none before" (p. 152), and individual interests that will "change shape and direction when subjected to these participatory processes" (p. 152). It involves a

concept of political knowledge that is provisional, evolutionary, and mutable, "produced by an ongoing process of democratic talk, deliberation, judgment, and action" (p. 170). It involves a political judgment that is neither "subjective" nor "objective," but rather proceeds from the "kind of 'we' thinking that compels individuals to reformulate . . . 'I want x' . . . as 'x' would be good for the community to which I belong"—an operation in social algebra for which not every 'x' will be suitable" (p. 171).

"At the heart of strong democracy is talk," Barber writes (p. 173). And the section on talk is the most persuasive in the book. Strong democratic talk, he tells us, "entails listening no less than speaking; . . . is affective as well as cognitive; and . . . its intentionalism draws it out of the domain of pure reflection into the world of action" (p. 174). Barber contends, correctly I believe, that representative democracy (which he sometimes calls "thin" democracy) diminishes this kind of talk, and, in doing so, dramatically reduces the meaning and effect of citizenship.

Barber concludes with a collection of imaginative suggestions for adding strong democracy to the primary representative institutions of large-scale modern societies. He argues for "common work and common doing"—collectively creating pocket parks, urban farms,

storefront community-education centers, neighborhood skill teams, crime-watch units, and universal-citizen service. He proposes neighborhood assemblies, for populations of from 5 to 25 thousand citizens, which could deliberate, vent grievances, act as ombudsmen, and possibly form part of an initiative and referendum process. He suggests representative town meetings, office holding by lot, and handling decriminalized small offenses through new forms of lay justice. He proposes a national initiative and referendum process, which would include a mandatory tie-in with neighborhood assemblies and interactive-television town meetings for the purpose of civic education, a multichoice referendum format (in which citizens would have a choice among *support strongly, support but with [a specified] objection, oppose strongly, oppose the proposal, but not the principle behind it*, and so forth), and a two-stage voting process providing for two readings. He supports experimental electronic balloting, vouchers in schooling and housing (with some reservations), universal-citizen service, neighborhood-action programs, workplace democracy, and a program for redesigning public space. If we tried all these things at once, as Barber recommends, citizens would undoubtedly "fly to the assemblies"—at least until the novelty wore off. Even after ennui set in, the intellectual and emotional residue would be incalculable. Why could not a state like Oregon or a town or an innovative university adopt some of his agenda on a trial basis, building in, of course, the prudence and the ways to "institutionalize regret" (p. 308) that Barber suggests?

Barber's repudiation of static liberalism and his creative suggestions for an ever self-renewing "strong" democracy are the best parts of this book. The worst side emerges when, in order to establish a conceptual foundation for strong democracy, Barber distorts opposing views in a way that is woven so tightly into the analysis that it undoes much of the good I have described. For all Barber's preaching about "empathy" and "listening" in his sections on democratic talk, he does not himself seem to have learned to listen. He hears what he wants to hear, and when the reality is otherwise, he distorts to fit. In this respect, *Strong Democracy* sometimes reads like a series of polemics against some of Barber's favorite bugaboos, including repre-

sentative government and anarchism as in his past works, he argues by straw monsters—caricatures of ideal adherents would never recognize.

Twelve years ago Barber gave a graph of uniformed officers subduing the simple caption, "Representatocracy" (*The Death of Democracy* [Princeton: Princeton University Press, 1975]). Nothing quite that blunt this time; he writes only that "the representative steals from individuals the ultimacy for their values, beliefs, and so forth" (p. 145), is "incompatible with freedom" (p. 146), "precludes the evolution of a public in which the idea of justice can take root" (p. 146), and produces "passive" citizens (p. 219). That's representative democracy. Barber does not qualify the words *steals*, *incompatible*, *precludes*. He does not write an argument that every citizen wants and needs representation for some purposes at some time, no hint, let alone discussion, of a public talk that representatives enter into that their election inspires. This makes it hard to learn much about representative democracy from Barber's analysis.

Anarchism, another old enemy, gets similar treatment. Barber pulls on it of a complex (even inchoate and contradictory) set of ideas and concepts. "The anarchist perspective is the perspective of the radically isolated self for who is only what it can see with its own eyes. The anarchist reads the world as One [Self], where the existence of others is scarcely perceived and never felt" (p. 219) so on. Not a word on the communist tradition.

Although Barber deplores the adversary tradition in political discourse, his own pen has been tempered in this style thus undercuts his own purpose. His critical overstatement and denigration may be required to infuse public discourse with a new vision as important as this, but this style is not compatible with the others required by "strong democracy."

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Book Reviews: Political Theory

Reciprocity. By Lawrence C. Becker (New York: Routledge & Kegan, 1986. 436p. \$32.50).

Although the death of political philosophy was widely predicted a generation ago, the renewed vigor of the field is epitomized by Lawrence Becker's *Reciprocity*. While many political theorists may disagree with the mode of approach, few would deny that it reflects the spreading interest in philosophical analysis to determine the foundations of political virtue.

Becker writes in the neo-Kantian tradition of "*a priori* moral theory," based on what he describes as "the general conception of morality." In this view, "moral judgments are judgments about what rational agents ought to do or be, period" (p. 17)—and such judgments are determined by discovering "determinable rules of participation, teleology, deontology, valuation, procedure, transformation and recognition" that satisfy the "conditions for logical validity and soundness" determined by contemporary philosophers (p. 36).

While the abstract quality of this fundamental approach may strike many political scientists as difficult, Becker seeks to provide concreteness through an unusual series of devices. Part 3 is a scholium, with lengthy bibliographic essays on a wide range of scholarly fields. Interspersed in his text are many novelistic epigraphs "meant to bring vividly to mind the intricate, subtle, and particular lives within which, and for which, moral philosophy is done"; while "nothing in the argument formally depends on them," these epigraphs are also a means of providing concreteness (p. xi).

Becker's main thesis is that the abstract logical criteria for a "general theory of morality" lead to a "virtue theory" based on reciprocity. His definition of reciprocity, however, differs from that in most social psychology and game theory (pp. 354–68). For Becker, reciprocity must be the "disposition" to reciprocate for benefits received, whether they have been requested or not; such reciprocation need not be narrowly "in kind," but it does rest on an asymmetry between good and evil. According to Becker, while "good received should be returned with good," maxims concerning evil are quite different: "Evil received should not be returned with evil" and "Evil received should be resisted" (p. 74).

In part 2, Becker spells out the implications of the resulting view of morality for families, for future generations, and for the law. Throughout, he addresses philosophic literature from the ancients to such contemporaries as Nozick and Rawls. In short, Becker's *Reciprocity* is a serious attempt to confront contemporary ethical issues from the neo-Kantian perspective, focusing on the need to return to the kind of theory of dispositions or virtues traditionally associated with Aristotle and more recently emphasized by Alasdair MacIntyre.

For me, such an approach encounters the fundamental difficulty inherent in most theories supposedly based on *a priori* logic. According to Becker, one of the first requisites of "well-defined" rules of any activity is that the participants need to be specified. Since "a rule is a moral rule if it describes what rational agents ought to do or be, all things considered," "participants are therefore also specified: moral action guidance is addressed to rational agents, from rational agents" (p. 30).

Since this "participation rule does not limit the class of rational agents to human beings, nor does it necessarily include them all," Becker himself admits that "the boundaries of the concept of agency are somewhat vague (with respect to the status of young children, for example, and some animals)" (p. 31). Further reflection leads to the realization that this admission calls into question Becker's entire theoretical apparatus.

On the one hand, it is manifest—both from the philosophical tradition and from observation—that many animals satisfy the criteria of purposive action and self-consciousness that are used by Becker to define "rational agency." To cite the most obvious example, Jane Goodall and Frans de Waal have given us incontrovertible evidence of such putatively "human" traits in chimpanzees.

Unless the use of verbal speech is formally required, an *a priori* theory of rational agency cannot be limited to humans. If so, the web of reciprocity should, on Becker's own grounds, extend to other animals—with consequences that he does not seem to draw concerning the relationship of humans to individual animals and the ecological systems of which they are a part. If not, the theory cannot claim to be a *priori*, since it is based on the concrete characteristics of *human* behavior, combining as it

does feelings and emotions as well as verbal speech and social behaviors derived from millions of years of hominid evolution.

At the other extreme, of course, one could argue that "rational agency" only applies to those who reason "in the silence of the passions." Here, however, Becker must confront the opposite difficulty, perhaps best stated by Rousseau. In the *Second Discourse*, after describing the golden rule as "cette maxime sublime de justice raisonnée," Rousseau adds, "Quoi qu'il puisse, appartenir à Socrate, et aux Esprits de sa trempe, d'acquerir de la vertu par raison, il y a longtemps que le Genre-humain ne seroit plus, si sa conservation n'eût dépendu que des raisonnemens de ceux qui le composent" (*Oeuvres Complètes*, vol. 3, Bibliothèque de la Pleiade no. 169 [Paris: Editions Gallimard, 1964], pp. 156-57).

In short, either Becker's theory applies to many animals as well as to human infants or it is only applicable to "Socrates and minds of his calibre." Such an approach therefore proves either too much or too little, leading to the suspicion that purely a priori approaches to "virtue theory" are inferior to the observation and reflection characteristic of Aristotle. No longer does it seem reasonable to ignore the role of emotion and feeling in the phenomena known as morality; no longer can an account of human obligations to the family, to future generations, or to the law be based on abstract logic. It is time to admit that, like behaviorism, philosophic a priori-ism has been rendered obsolete by contemporary research in the life sciences. As Robert Axelrod has already shown, evolutionary biology is a better guide to human reciprocity than attempts to deduce obligations from the kind of "reasoned theorizing" (p. 31) used by many contemporary philosophers.

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Research in Political Sociology, Vol. 2. Edited by Richard G. and Margaret M. Braungart (Greenwich, CT: JAI, 1986. xviii, 332p. \$52.50).

This book is the second volume in an annual series of original contributions revolving around a dominant theme. The previous year's

issue dealt with the development of nation-states, whereas this 1986 issue considers a germane topic: effectiveness and legitimacy. Twelve articles of relatively equal length are grouped together under four headings: *political structure and change*, *cultural politics*, *youth politics*, and *social and political conflict*. Within these generous boundaries, each contribution remains autonomous and distinct in terms of the approach and topic chosen.

The series opens with what I would call a theoretical hors d'oeuvre. I cannot say whether it simply stands as an abstract endeavor meant to ennoble (so to speak) what is to follow or whether it is the outline of more elaborate future developments. In any case, Jonathan H. Turner and Charles H. Powers submit no less than twelve propositions under four captions. Borrowing from such classics as Marx, Durkheim, and Weber together with episodic imports from Simmel, Spencer, and Pareto, the authors manage to outline converging or complementary principles of political organization. Right from the start, Turner and Powers are aware that such an attempt may elicit more than reticent comments. Marx, Durkheim, and Weber do not always mix well. But despite incompatibilities deemed by them only apparent, the authors rely on common denominators in the name of a to-be-developed "natural science of society." The principles are not necessarily meant to be original and some of them verge on the obvious. The intention is mainly to bridge a gap between the classics and contemporaries.

The articles coming after this theoretical opening belong to different genres of writing; some may be considered full-fledged research articles, others tend towards the essay type, and some stand in between.

Robert J. Antonio expounds on the limits of Max Weber's model of authoritarian bureaucracy, which he considers too monolithic and fatalistic. In contrast, Antonio tries to demonstrate from the ancient Roman Empire—which had been Weber's point of reference—that instead of being destined for a state of permanence, any such regime is likely, by its own action, to generate countervailing pressures from the ruled and to have to submit to changes in the long run. The phenomenon of clientelism is put in evidence, underlining a feature common to both ancient and modern modes of authoritarian bureaucracy. The

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whole argument counts on the existence of an inherent dialectic in the relationship between rulers and ruled.

In the line of systematic research, Ronald Inglehart pursues his postmaterialist saga and furnishes up-to-date confirmations of a trend that should, I imagine, one day lead to this postmaterialist minority becoming the majority devoted to the promotion of the values of self-expression, a better environment, and social solidarity. Inglehart's mode of argumentation is sufficiently flexible to take into consideration the ups and downs of the postmaterialist trend, and therefore more likely to convince skeptical readers. The argument, as we know, revolves around the hypothesis that people reach levels of material satisfaction that drive them to strive for more sublime values thereafter. Just as Marx bound society to the imperative of material needs, this approach resorts to a psychological reading of human social development. In direct affiliation with Maslow, Inglehart expounds a psychological historicism that sets goals close to the ultimate values of the Western world. Scarcity and abundance are more or less given an absolute character.

From data collected in France in the late 1970s, David Swartz takes a stand against the "managerial-revolution" school and opts for a "class-organization" perspective. The study suggests that different classes in France control different types of organizations but that organizational constraints also require members of the managerial elite to conform to specific educational credentials. Social closure is therefore exercised through the recognition of a double standard of selection: economic and scholastic capital (using Bourdieu's terminology). In order to modify the frequent impression of an overwhelming technocratic elite topping both private and public spheres, Swartz establishes a dividing line between a public technocracy—which comes from professional family backgrounds and the highly selective channel of the *grande écoles*—and a still impressive part of the private sphere reserved for an elite based on family wealth.

On cultural matters Donald Granbert and Sören Holmberg, using a social-psychology approach, gauge the degree of subjective ideological congruence of individuals with the party they prefer. Comparing Swedish with U.S. citizens, the study reports a stronger

ideological correspondence between the Swedes and the political party of their choice than is the case for their U.S. counterparts. Such a disparity in ideological attachment would be largely due to the way the political competition takes place—the presidential contest, for instance, lending itself to personalized rather than ideological campaigns.

As to youth politics, Joyce M. Mushaben compares the "no future" generation of the 1980s in (West) Germany with the youth movements of that same country between the two world wars. The paper focuses on their respective contributions to German political culture while denying that the present condition is necessarily conducive to a Nazi form of regime. In opposition to Inglehart, Mushaben emphasizes the feeling of insecurity deriving from economic uncertainty, unemployment, environmental deterioration, nuclear policies, and the like. Claiming that today's German youth are excluded from the benefits of postmaterialism, the author wonders if the present policies of integration into society are adequate. Indeed, the essay is normative, at least in its conclusions. In contrast, there is the study by Vladimir Shlapentokh of attitudes and behavior of Soviet youth in the 1970s and 1980s. Seemingly well documented (I am not a specialist in the field), the paper comes close to the type of good survey a reader might read in *The Economist*, including, of course, the same underlying message.

The book concludes with monographs on social and political conflicts. After a disproportionately long historical background to the "process of class struggle" in Hawaii, J. A. Geschwender and R. F. Levine sum up in a few pages how, after World War II, the Democratic party was prevented by capital's "red-baiting" campaign from becoming a prolabour party. Next are Carol L. Schmid's considerations on Quebec. Having posited that to exist as a submerged nation, a group has to conform to a number of conditions regarding its identity and political will, the author arrives at the foregone conclusion that Quebec only partially fulfills these requisites. The book closes on E. Walsh and S. Cable's case study of movements engendered by the Three Mile Island accident. The authors discuss the complexity involved in any analysis of social-movement litigation.

The idea of an annual review of this kind on

research in political sociology is excellent, even though the treatment of themes still opens the way to a dispersion of topics.

ANDRÉ J. BELANGER

University of Montreal

Politics, Self, and Society: A Theme and Variations. By Heinz Eulau (Cambridge: Harvard University Press, 1986. x, 567p. \$39.95).

This is a splendid book. It brings together 20 of Heinz Eulau's principal papers—the earliest from 1958, 10 from the sixties, 8 more from the seventies, and 1 published here for the first time. As such, it is a most valuable reference. Harvard University Press has done the profession a service by assembling these scattered papers.

The theme of the subtitle is that "self and society are inextricably intertwined in political life" (p. 2). Working with an understanding that "it is the function of science to understand and interpret the world, not to change it" (p. 22), Eulau's analysis of his theme has been guided by explicit epistemological considerations. His principal concern has been with the problem of simultaneity. This results from a larger unit and its smaller subunits being simultaneously involved in any political action. Neither the larger unit nor the smaller units are ever free of each other. The larger unit—whether a group, organization, or nation—does not have any independent status apart from the conduct of individuals who comprise it. But in the real world of politics, it is the collectivity and not its individual members that is the effective decision maker.

In order to collect data and analyze the simultaneously acting units, Eulau distinguishes between the subject unit—by which he means the observational unit, the unit whose behavior is observed—and the object unit—the *explicandum*, the unit whose behavior is to be explained. This distinction is crucial because proper analysis requires bringing all of the object unit's properties to the same level. This may involve either reduction (in which case properties on a more inclusive level are treated as contextual) or construction (with properties on a less inclusive level being treated as dis-

tributive—or relational or structural—properties of the object unit).

"Unless the properties of collective actors are obtained through reduction and construction," Eulau rightly warns, "analysis of large units will inevitably remain literary, discursive, and speculative" (p. 95). But if there are perils in not adopting this research strategy, there are substantial rewards for doing so. "Macrolevel construction can go a long way in making microanalytic, psychology-based models of decision-making behavior relevant to political institutions and processes" (p. 450). Just as self and society are inextricably intertwined, so is analysis of behavior and institutions. Political behavior is not a separate field of investigation but an approach to the full gamut of political phenomena.

An ability to move easily across levels of analysis—to transform data gathered on one level for proper use on another—also confers an ease in movement along the micro-macro continuum. As one moves toward the micro end, empirical data are added that make for greater validity and reliability. As one moves toward the macro end, more significant questions can be addressed. But it is not necessary, Eulau says, to give up validity and reliability in order to achieve significance. An understanding of construction and reduction of data allows an analyst to have validity and significance on whatever level. Many arguments in political science result from a failure to grasp this crucial point.

Both of Eulau's most extensive research projects have been informed by a levels-of-analysis perspective. In the four-state legislative study (chap. 6) the data were gathered on the individual level and much of the analysis was also on that level. But the legislature was conceptualized on the institutional level as a determinate role structure that "linked the legislature as a subsystem with other [political] subsystems" and "institutionalized and resolved the social, economic, and political conflicts" (p. 203) with which the legislature had to deal. The levels-of-analysis perspective was even more explicit in the Bay Area study of 82 city councils (chaps. 10, 11, 14, and 15). Here again, data were gathered on the individual level, but the analysis was all on the group level, dealing with integral properties (such as the size of council), relational properties (such as cosponsorship), oppositional activi-

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ity, affect, respect, and expertise), structural properties (such as bipolar, unipolar, and non-polar councils) and contextual properties (such as city size).

In *Politics, Self, and Society* Eulau also expounds configurative analysis and developmental analysis. A configuration is an emergent whole. "Emergent wholes such as 'personality,' 'society,' or 'culture' are units that are something more than the sum of the items, elements, or traits that constitute them individually" (p. 249). The task of the analyst, then, is less one of tracing cause and effect among the elements than of discovering the particular configuration that emerges from the combination of elements. Thus Eulau is interested, for example, in the response style that results from setting a city council in a particular political matrix. And in his most recent analysis, Eulau traces life space and social networks, discovering differences between day dwellers and night dwellers in their use of neighborhoods.

Developmental analysis is based on Lasswell's distinction between goal thinking (analysis and selection of values toward which decisions are directed), trend thinking (analysis of past tendencies and future probabilities), and scientific thinking (analysis of limiting conditions). Therefore, says Eulau, "the methodological problem is nothing less than to connect statements of value or preference, statements of fact, and statements of expectation" (p. 228). *Politics, Self, and Society* includes three developmental analyses: a discussion of policy maps summarizing council members' perceptions of their city's stage of policy development; a quasi-longitudinal, quasi-experimental design for comparative policy analysis; and one of the very few re-analyses of the Miller-Stokes data.

"The behavioral persuasion in politics," Eulau wrote in the early sixties, "has more than one approach, and there are many voices that speak in its name. But they all have in common a commitment to the study of man as the root of things political or, to put it more technically, to the individual person as the empirical unit of analysis" (p. 22). *Politics, Self, and Society* contains the fruit of his personal efforts. There is a little irony to its publication in the late eighties. We are now hearing about a new institutionalism, but if political scientists had paid closer attention to what Eulau was

saying, we would already have a richer institutional analysis. For if Heinz Eulau's subject has been man, his object has always been politics.

JOHN H. KESSEL

Ohio State University

Rational Association. By Fred M. Frohock
(Syracuse, NY: Syracuse University Press,
1987. xi, 201p. \$27.50).

New dimensions of the debates over liberalism and its fate are difficult both to isolate and to advance interestingly. This slim volume does both. Frohock introduces the consistency problems of liberalism by assessing them through the lens of collective-choice theory, which simplifies many of the dilemmas confronting formal understanding of democratic "rationality" as treated in the literature. By attending to the nature of rationality throughout the book, Frohock is able to clarify the problems and introduce possible solutions that could revitalize what many think of as the dying theory of liberalism. His primary question deals with the puzzles surrounding the inconsistency "between individual rationality and collective outcomes" (p. ix). His goal is to avoid the discontinuity between *one* and *all* that plagues rational-choice theory without making the major countererror of adopting a holistic solution.

The problem, embedded in treating utility-based rationality as the ground for quantitatively reaching collective action, is the move from the individual to community (set forth in chap. 1). The analysis then proceeds, through assessment of individuals and structures as set forth in the prisoner's dilemma, to establish the importance of isolation of individuals as a background assumption of the puzzles (chap. 2), especially in a concluding contrast with Aristotle's corporate model—giving a hint about where we eventually will end. Frohock continues his arguments about the tensions arising from Arrow's theorem and exchange theory to conclude (chap. 3) that "there is no mediating device to transform the disjointed features of liberalism into the type of moral community liberalism seeks" (p. 67). His assessments of individuals and groups lead him to conclude his criticisms of liberal theory's linkages with the observation that different

liberal theorists (Hobbes, Locke, and Rousseau) had different approaches to the dilemmas (clarified by his analysis of formal theorists), with Rousseau's general-will solution—adopting community will and morality as one's own—providing the "exit routes" for these dilemmas (chap. 4, p. 98).

Frohock's own skillful and easily followed route through formal theory is continued in his assessments of rationality and reasoning (chap. 5), and rational forms (chap. 6). The path, clear enough for undergraduate use even though it advances the literature, provides a framework for pursuing careful assessment of the options for treating the fit between collective-choice theory and moral decisions, for moving from the rational individual to appropriate grounds for collective commitments on specific actions. Perhaps the single most powerful dimension of the book is that it makes clear to both proponents and opponents of rational-choice analysis just what is missing in the effort to deal with liberal and democratic theories from the choice perspective. Traditional theorists troubled by rational analysis will better grasp what is missing in the formal approaches; formal theorists will see what conditions are not resolved within their theories. Defining *reasoning* as deliberation (dealing within the realm of uncertainties)—as isolation of things that count as good reasons—rather than counting reasoners under a democratic motif permits one to move the discussion out of the constraints of formal choice of individuals (which leads to inconsistencies in collectivizing the will) into individual choices within the discourse that provides reasons for collective action that can be shared: "The task of political inquiry, once individuals are accepted explicitly as reasoning agents, is to recognize the juridical forms that are compatible with individual minds. . . . Individuals who are rational on reasons must rely on rules that govern; and the unity of minds can be located in the rules by means of which self-legislation occurs" (p. 159).

In his final chapter (chap. 7), Frohock carefully focuses on the forms of background assumptions that affect collective-choice applications to liberal values: "These three types of connections between individuals and collectives—decisiveness, fairness, and hypothetical generalization argument—introduce collective considerations to individual choice" (p. 169).

The fairness option, however, is different in that it must provide a reason for cooperating. Fairness as a necessary but not a sufficient condition, for a moral association relies upon the giving of reasons: "A reason-giving form of rationality is the substance on which such an association is formed. Reasons, as we know by now, (a) are formed on a network of rules that can organize communities authoritatively, (b) define individuals holistically as members of rational classes, and (c) provide evaluative criteria to address those issues of fairness in the absence of which cooperation has no meaning" (p. 171).

Frohock provides convincing analysis that confirms the notion that reasons for choice within a collective are the key to rational association. Reasons, not just preferences, permit the establishment of rules and morality within the collective (p. 179). Deliberation, giving reasons and counterreasons while the collective works towards standards that can be shared, is an operation of reason that permits one to join a search for common interest in ways impossible under formal choice theory based on isolated preferences. In his concluding advocacy of "juridical democracy" (p. 181), Frohock has carried us back through reasons for returning to Aristotle's analysis from within formal theory's own logic—an important journey.

GEORGE J. GRAHAM, JR.

Vanderbilt University

Hermeneutics and the Sociology of Knowledge. By Susan J. Hekman (Notre Dame, IN: University of Notre Dame, 1986. 224p. \$29.95).

Susan J. Hekman's *Hermeneutics and the Sociology of Knowledge* has two main goals: first, to point out the similarities between Hans-Georg Gadamer's hermeneutics and Karl Mannheim's sociology of knowledge and second, to indicate the implications of these similarities for social-scientific method. In her view, Mannheim and Gadamer share three notable insights. First, both emphasize interpretation. While Mannheim is confused on this point he nevertheless advocates a "hermeneutic method" according to which historical phenomena are to be investigated "in

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terms of the understandings and presuppositions of the age in which they are manifest" (pp. 69-70). For its part, Gadamer's *Truth and Method* attempts to show that interpretive understanding is fundamental to all aspects of human life. Second, both emphasize the way in which interpretation is conditioned by the situation of the interpreter and thus suggest the way in which Enlightenment conceptions of truth and objectivity need to be modified. Hekman contends that Mannheim is, again, somewhat confused on this point, calling his views phenomenological positivism. Still, like Gadamer, he shows that there is no ahistorical, unsituated Archimedean point from which to acquire an objective explanation or the truth of human beings or society. Rather, the worth of any interpretation is relative to the circumstances from which it is advanced and to the questions it tries to answer. For this reason, both Mannheim and Gadamer stress the importance of what Hekman calls *self-reflexivity*: interpreters must be aware that their perspectives are "prejudiced" (in Gadamer's words) and hence must be willing to subject them to critical examination. Finally, both Mannheim and Gadamer conceive of interpretive perspectives in "collective" rather than individual terms. In a passage that Gadamer might have written, Mannheim therefore asserts, "Strictly speaking, it is incorrect to say that the single individual thinks. Rather it is more correct to insist that he participates in thinking further what other men have thought before him" (p. 80).

What Hekman sees as the chief difference between Mannheim and Gadamer might be expressed in similar terms: namely, that Gadamer simply extends Mannheim's thoughts in a more radical and consistent manner. Not only is he clearer on the role of interpretation and its implications for Enlightenment views of truth and objectivity, he also clearly "reverses Enlightenment thought on the relationship between the natural and the social sciences" (p. 107). Neither theorist thinks that the methods of the natural sciences can provide any model for those of the social sciences. But Gadamer goes further, suggesting that insofar as the natural sciences disregard the conditions of prejudice, they are themselves flawed. As Hekman continues, "the model of understanding characteristic of the human sciences is the universal process by which we, as human

beings, attain knowledge; that characteristic of the natural sciences, on the other hand, seriously distorts this process, hence calling into question the legitimacy of this mode of understanding" (p. 107). From this analysis, Hekman concludes that Gadamer's hermeneutics provides an important "anti-foundational" basis for the social sciences. Rather than universalizing notions of truth and objectivity developed for the natural sciences and seriously distorted in themselves, a hermeneutic social science focuses on the relations between "human thought and human existence" (p. 11).

As far as I know, Gadamer never charges natural-scientific method with internal flaws. His claim is rather that the mode of experience proper to the natural sciences is *not* proper to other fields of human inquiry and that their project is one rooted in the concerns and commitments of a particular community. Hekman's failure to clarify the point she is making here is indicative of similar failures throughout her book. She covers a great deal of ground, makes numerous distinctions and comparisons and contends with much commentary on, and criticism of, the texts with which she deals. But she does much of this far too quickly and therefore leaves significant gaps in her argument. In the remainder of this review I shall briefly consider two.

First, despite her intention, Hekman fails to spell out the implications of Gadamer's work for social-scientific method. She thinks that Gadamer goes beyond the Wittgensteinians in overcoming Enlightenment-type distinctions between natural and social science; for, although the latter emphasize interpretation, they continue to identify the natural sciences with the search for truth and simply exclude the social sciences from it. Gadamer, however, denies that either attains truth. I think there are misconceptions in this account. Of concern at this point, however, is the relatively superficial character of Hekman's exploration of the issues. If both natural and social science employ interpretations, if both are historically situated, is this all that needs to be said? What of that which Anthony Giddens has called the "double hermeneutic" in the social sciences? If Gadamer thinks that neither natural science nor social science attains "truth," what does he mean by talking about truth and denying the exclusive access of method to it?

This last question is related to another dif-

ficulty in Hekman's argument. Throughout her book she denies that Gadamer has a problem with truth because he defines it in terms of the collective conceptions or shared vocabulary of a community. Indeed, she claims that this is what separates Gadamer from the nihilism of Rorty, Foucault, and Derrida: we can make moral judgments and can distinguish between good and bad interpretations in general because we belong to historical traditions to which we can appeal for our normative standards. But what if our traditions are Fascist, racist, sexist or the like? It is on precisely this point that much of the criticism of Gadamer's work has focused. The question is how we examine our shared understandings or reflect on our prejudices. Hekman touches on this question at two points: first, where she discusses the self-reflexivity she finds in both Gadamer and Mannheim and second, in her discussion of Habermas's critique of Gadamer. In the first instance, however, she merely notes that both theorists demand self-reflection without investigating the conditions of its possibility. The problem here is that if all interpretation is prejudiced then so are the interpretations in which we reflect upon the prejudiced character of our interpretations. Hekman herself makes this point in discussing Gadamer's antifoundationalism. Still, if he is antifoundationalist in this regard, what are the consequences for his demand for self-reflection? If our prejudices are racist, will the vocabulary in which we reflect on them not also be racist? Habermas argues that "objectifying" methods need to be brought into the social sciences at precisely this point to allow distance from our own preconceptions. Hekman dismisses this solution, but in doing so she apparently overlooks the difficulty with her own assessment of Gadamer's work.

The attempt to synthesize Mannheim and Gadamer to provide the social sciences with "both a solid philosophical foundation and a viable research program" (p. 11) is interesting and productive. One wishes that Hekman had had time to explore the issues involved in more depth.

GEORGIA WORNKE

Radcliffe College

The Political Ideas of Marx and Engels, Vol. 2, Classical Marxism, 1850-1985. By Richard N. Hunt (Pittsburgh: Pittsburgh University Press, 1984. xiii, 421p. \$37.95).

This is the successor volume to Hunt's well-received study of the political ideas of the young Marx and Engels, and a fitting memorial, given his untimely death, to its author. In his first volume Hunt assumed that the notion of the "parasite state" hovering above civil society constituted the core of Marx and Engels's early political thought. Here he traces in particular the development of a notion of "democracy without professionals," in which large-scale participation and the frequent election of all important officials were to prevent the growth of any permanent, alien political and bureaucratic force. This, Hunt presumes, was the chief achievement of Marx and Engels's later political thinking, the source for their adoption of the model of the Paris Commune, and the main distinction between their work and more authoritarian forms of twentieth century Marxism. Like his chief competitor, Hal Draper (*Karl Marx's Theory of Revolution* [New York: Monthly Review Press, 1977]), Hunt denies that anything like "totalitarian democracy" (in Jacob Talmon's phrase) was the natural outcome of Marx and Engels's critique of the liberal state. Analytically, Marx and Engels's notions of both the parasitic, or independent, state and the class, or instrumental, state developed in the 1850s through their observations of Bonapartism and studies of "Oriental despotism," the latter being gradually seen as the first instance of the parasitic state. After detailing these anthropological explorations, Hunt treats the development of both state theories up to the 1870s and the Paris Commune, to which two chapters are devoted. After a useful excursus (which will fuel the debate on Marxism and morality) into the problem of the degree to which Marx and Engels recognized individual rights and the question of the tyranny of the majority, Hunt examines the notion of classless society and the disappearance of the state, arguing that both have been misconstrued as completely utopian. His final chapters then detail Marx's debate with the anarchists in the International and the debates after 1870 over the possibility of skipping historical stages to reach socialism (notably relevant for Russia)

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and over the strategy of a legal, peaceful road to socialism in democratic countries. Hunt's is a careful, largely textual and very scholarly study. Abjuring ideology and polemic, he nonetheless rejects many well-known interpretations of areas of the topic (e.g., Wittfogel on Oriental despotism). His aim is not to expose Marx and Engels's weaknesses or heap the sins of their disciples upon them, but inevitably some questions of this type must be faced. Hunt indicates, for example, Marx's failure to foresee the expanded role of managers and bureaucrats in the future (cf. pp. 252-63), but neglects to ascertain why this (obviously serious) weakness in the theory persisted (more attention to Saint-Simon and to the problem of economic administration would have been helpful). Similarly he steers clear of some of the more unpleasant innuendoes associated with "proletarian dictatorship" (better handled by Draper). Nonetheless, this is an excellent study with great strengths, and its subtlety and toleration of the ambiguity of the sources help to raise the level of scholarship in this area considerably.

GREGORY CLAEYS

Washington University

James Madison: Creating the American Constitution. By Neal Riener (Washington: Congressional Quarterly, 1986. xix, 203p. \$11.95, paper).

This work, though not labeled as such, is apparently a revised edition of the author's earlier (1968) treatment of James Madison's political thought. Its seven chapters focus on concerns that stem from the question Riener poses in the first sentence of the book: "Is republican government in a large state possible?" (p. 1). These chapters explore four difficulties—"disunion, large size, faction," and the "anti-republican danger"—that Madison confronted in his efforts to "reconcile liberty and large size in the infant American republic" (p. 132). As such, the subtitle is a bit misleading because these concerns take us well beyond Madison's efforts toward a stronger national government or his contributions to the Philadelphia Convention and the ratification struggle. Indeed, as Riener makes clear at the out-

set, he is concerned with Madison's "evolving thought" throughout "his entire life," particularly his "maturing political conceptions," such as his "bold assertion of civil liberties, the organization of a republican political party, and a strategy of constitutional interpretation . . . that would safeguard union and freedom against anti-republican attack" (p. 3).

A goodly portion of Riener's treatment is devoted to accounting for Madison's changes of heart regarding the nature of the union and the relative powers of the national government vis-à-vis the states. According to Riener, Madison's strong nationalism, evident at the convention and in his early political thinking and activities, was modified as he came to realize that ratification of the Constitution required "a more moderate nationalistic interpretation" (p. 89). In Riener's view, his modification on this and other matters, such as political parties, clearly manifest Madison's prudence—"a practical wisdom," "a quality of mind" concerned with "wisely relating means to ends" (p. 13). Thus, as Riener would have it, many of Madison's presumed inconsistencies can be attributed to his "consistent prudential approach to politics" which rendered it difficult, if not impossible, "to maintain a superficial consistency" (p. 16). And where, in retrospect, many find Madison to be imprudent, to have taken extreme positions—for instance, his constitutional opposition to the national bank or the underlying theory of his Virginia Resolutions, drafted in response to the Alien and Sedition Acts—Riener holds that we encounter another basic element of Madison's philosophy: "When the safety and happiness of the people were at stake, Madison's usually high regard for existing legal forms would give way to his insistence upon placing the substance of republican liberty and union ahead of accepted but malfunctioning constitutional or legal forms" (p. 136).

Riener also explores Madison's extended-republic theory at some length. He notes certain difficulties with this theory that are of more than passing interest to the student of U.S. politics today. For instance, Madison seemed to believe that there is an objective "public interest and that it can be identified," a belief that, Riener surmises, "is ultimately grounded in his natural law orientation" (p. 124). Yet his failure to define this central concept means, as Riener remarks, that his theory

"may not be scientifically operational in modern terms" (p. 125). And in this vein, as Riemer notes, his theory is concerned with preventing factious rule—which "limits [its] relevance . . . for those who favor a broad exercise of national power in what they hold to be the national interest" (p. 118).

While noting that Madison "was by no means a systematic, comprehensive, sophisticated student of political theory," Riemer concludes by pointing out the ways in which his thought provides "the germ of a durable, penetrating, and persuasive democratic theory for man today" (p. 158). Though his view of human nature prevented him from even contemplating a heaven on earth, Madison did repose sufficient faith in man to take the "calculated risks" (p. 164) involved in establishing an extensive republican regime under the forms of the Constitution.

GEORGE W. CAREY

Georgetown University

Self and Society: A Study in Gandhian Thought. By Ramashray Roy (Beverly Hills: Sage, 1985 [in collaboration with United Nations University, Tokyo]. 205p. \$25.00).

The author maintains that the predicaments of Western modernity can be overcome through the Gandhian conception of the self and of the relationship of the self to society and nature. According to him, the pathology of modern civilization has its origin in the seventeenth century shift in European thought—and practice—from the conception of man as being an integral part of a larger order to the concept of man as an autonomous, self-defining, self-sufficient and possessive-individualistic ego. In Roy's view, such Western attempts to repair this "broken totality" as expressivism, Hegelianism, Marxism and romanticism do not lead us out of the "satanic civilization," which remains committed to "the multiplication of wants and machinery contrived to supply them." What is required, according to Gandhi, we are told, is not mere "external" or structural changes in social and political organization but the self-transformation, or *swaraj*, of human beings, leading to the realization of "the essential unity of God

and man and for that matter of all that lives." Against the "broken totality" of modern Western civilization, Roy advances Gandhi's peculiarly Hindu philosophy of life, according to which, as noted by Gandhi, "all life (not only human beings, but all sentient beings) is one" (p. 73). Salvation, accordingly, is not to be confined to human beings; it has to extend to *all* God's creatures. This Hindu philosophy of life, Gandhi believed, excludes all exploitation. It defines salvation as the realization, by the self, of Truth, which means "not only truthfulness in word, but truthfulness in thought also, and not only the relative truth of our conception, but the absolute Truth, the Eternal Principle, that is God" (p. 71). This Hindu conception of the integral unity of all life, as interpreted by Gandhi, implies that the process of self-realization, that is, Truth realization, can only be achieved through the moral-political actions of *satyagraha* and *sarvodaya*. Satyagraha, by rejoining politics and religion, we are reminded, repairs the broken totality of modern times.

Roy succeeds in bringing out the important contrasts between the modern Western paradigm of politics and the Gandhian conception of the self, society, and nature. The treatment of the distinctive philosophical foundations of those two conceptions or models of social and political life is the chief merit of this book. It suffers, however, from the limitations of the author's self-acknowledged training as a behavioralist. The book could have gained from a greater use of the available literature on Indian philosophy and on Gandhian thought. While it is indeed true that Gandhian thought has its roots in Hindu philosophy, it has also to be recognized that the former was influenced considerably by such strands of non-Hindu thought as Tolstoy, Ruskin, Thoreau, the Bible, and British jurisprudence.

While I would still regard the book as giving important and useful insights into Gandhi's philosophical undermining of the technological or scientific model of modern Western politics, I am somewhat disappointed by the author's inadequate treatment of Gandhi's *satyagraha* mode of liberative political action. How *satyagraha* constitutes a realistic and effective mode of emancipatory action against the technological model of politics remains to be demonstrated. From the author's treatment of the subject, I am left to wonder if Gandhi's

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alternative philosophy of the self and society is simply an utopian blueprint. Also left unresolved is the question of whether or not the Gandhian satyagraha is merely a voluntarist conception of social change.

It seems to me that for a proper understanding of the distinctiveness and relevance of Gandhi's philosophy and method of liberative action by those who suffer from the predicaments of "modern industrial civilization," it is necessary to compare and contrast the Gandhian "project" with both liberalism and Marxism. This has not been the specific objective of Ramashray Roy, but his book will be of great help to anyone undertaking such a larger study.

THOMAS PANTHAM

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The Evolution of Rights in Liberal Theory.

By Ian Shapiro (Cambridge: Cambridge University Press, 1986. x, 326p. \$39.50, cloth; \$11.95, paper).

Ian Shapiro's book is both more and less ambitious than its title suggests. It is more ambitious because Professor Shapiro's concern is not only to trace the evolution of liberal theories of rights, but to reveal the ideological force of such theories. As he says in the concluding chapter, "My aim has been to show . . . how conventional liberal values have come to function, and continue to function, conservatively in today's world" (p. 304). As an account of the *evolution* of right in liberal theory, however, it is less ambitious than the title suggests, for Professor Shapiro concentrates his attention on only four "moments" in the development of liberalism, "moments" represented by Thomas Hobbes, John Locke, Robert Nozick, and John Rawls.

Indeed, the author proceeds by fours in this book. In the first chapter he states that a "claim about rights generally involves a four-fold assertion about the *subject* of entitlement, the *substance* of entitlement, the *basis* for entitlement, and the *purpose* of entitlement" (p. 14, Shapiro's emphasis). In the succeeding chapters on the four "moments," then, Professor Shapiro analyzes the theories of Hobbes, Locke, Nozick, and Rawls in terms of their conceptions of the subject, the substance,

the basis, and the purpose of rights. In each case, he follows this analysis with an account of the implications—intended or not—of the philosopher's theory for the development of liberalism as an ideology.

These accounts of ideological implications are to my mind the most interesting parts of a most interesting book. According to Shapiro, for instance, Hobbes's political absolutism was due largely to the fact that he was caught in the "transitional moment" between feudalism and capitalism, a time when the economic and social practices necessary to the survival of the "negative-libertarian" society Hobbes really favored were neither fully nor firmly established. In these circumstances, Shapiro argues, Hobbes had to insist on the need for a "decisive authoritarian power" to enforce "the rules and norms essential for social action" (p. 66). Later, however, when the necessary practices and institutions had come to seem part of the natural order of things, the implications of Hobbes's arguments pointed away from absolutism and toward liberalism. Thus Hobbes's identification of "natural man" with "private man" led him to conclude that the purpose of a public sphere "was merely to facilitate private interaction. This is a move of immense ideological significance because it provides for the germination of the liberal notion that the state . . . is fundamentally undesirable, an illegitimate intruder except where its actions facilitate private interaction" (p. 60).

With regard to Locke, the representative of "the classical moment" of liberalism, intentions and implications again point in somewhat different directions. In arguing for toleration and against conformity in religious matters, for instance, Locke conceived of rights themselves "primarily in terms of toleration, of the spheres of people's lives in which they may be left alone" (p. 117). Such a conception leaves little room for a "doctrine of democratic participation," Shapiro says, and once religious toleration and conformity were no longer at the center of controversy, "the negative libertarian view of freedom Locke embraced had vastly different and more conservative ideological implications" (p. 117).

Both Hobbes and Locke, then, contributed by implication to the "negative-libertarian" view of rights and society—a view shared, the author says, by Nozick and Rawls, represen-

tatives respectively of the "neoclassical" and "Keynesian" moments. The chief difference between the two on Shapiro's analysis is that Nozick advocates the "minimal state" because he clings to the equilibrium theory of the self-regulating market while Rawls, who follows Keynes in rejecting this theory, advocates "the liberal corporatist state" (pp. 262-64). This is the only significant difference between them, though, and in the end we find that they join Hobbes and Locke in regarding "the purpose of political organization generally, and the civil rights constituted by it in particular, as being to preserve a negative libertarian society in which individuals can pursue their different individual goals unimpeded" (p. 283). If one is tempted to say that a political organization ought to serve this purpose, Shapiro is ready to point out that such a vision of politics and rights denigrates political participation and the public sphere, fails to foster a sense of community, and succeeds all too well in legitimizing the dominant interests in capitalist societies.

Professor Shapiro is not the first to bring these charges against liberalism, of course, and perhaps that is why he does not pursue them here. He sees his task, instead, as "historical anthropology" (p. 303), with the particular aim of providing "an evolutionary anthropology of establishment values" (p. 8). Does he succeed in this endeavor? He does if one is willing to count well-informed, closely argued, and provocative readings of Hobbes, Locke, Nozick, and Rawls as the full measure of success. Certainly it is a triumph of sorts for Shapiro to find something fresh and interesting to say about each of these thoroughly scrutinized philosophers. If one takes his notion of an "evolutionary anthropology" seriously, however, then one must conclude that there are some links missing from *The Evolution of Rights in Liberal Theory*.

This is most evident when one considers that Shapiro devotes his attention to two English philosophers of the seventeenth, and two U.S. philosophers of the twentieth, century. Between his pairs of subjects stretches more than 250 years of ideological evolution of which we are told almost nothing. To be sure, Shapiro does refer several times to the importance of the rise of the concept of equilibrium in the eighteenth century, and Bentham, Mill, and Sidgwick do appear briefly, but there is no

attempt to trace shifting conceptions of rights or developments in liberalism through the eighteenth and nineteenth centuries. Nor is there any attempt to explain why so many influential philosophers of the nineteenth century, both within and without the Anglo-American tradition, were suspicious of, or even hostile to, the natural-rights theories associated with Hobbes and Locke. A second kind of missing link is more a matter of method than content. Through the use of such evolutionary metaphors as "lethal mutations" (p. 5) Professor Shapiro suggests that the fate of a theory rests in large part on its ability to adapt—or, more precisely, to prove adaptable—to a changing environment. Hobbes did not intend to justify the capitalist market, for instance, but his "negative libertarianism survived and achieved the preeminent status it did in the dominant ideology because of its affinity with these emerging economic and social relations" (p. 63). Clearly there is some relationship between ideas and institutions here (or *superstructure* and *structure* in another idiom) but what is it? Shapiro rejects any sort of simple deterministic explanation of the relationship, certainly, but aside from invoking the evolutionary metaphor, there is no clue as to what he would put in its place.

There is only so much that one can do in a book, however, and I am inclined to think that Ian Shapiro has done quite a lot in this one. There is much to admire in *The Evolution of Rights in Liberal Theory*, and much to argue with, too, which is one of the reasons for admiring it. Professor Shapiro writes clearly, argues vigorously, and, for an author intent on unmasking the ideological functions of rights theories, remains remarkably free himself of ideological dogma. If he does not tell us all we might wish to know in this book, then we may well wish that he will tell us more in another.

RICHARD DAGGER

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Book Reviews: Political Theory

The Longing for Total Revolution: Philosophic Sources of Social Discontent from Rousseau to Marx and Nietzsche. By Bernard Yack (Princeton: Princeton University Press, 1986. xvii, 390p. \$35.00).

Bernard Yack has both historical and critical projects in this book. The historical project locates the heritage of German idealism within revolutionary thought from Schiller and the Left Hegelians through Marx and Nietzsche. These thinkers are often viewed as inheriting and secularizing Christian ideals. Yack argues, however, that their ideals are distinctively modern, reflecting notions of human dignity and freedom introduced by Rousseau and philosophically expressed by Kant. According to Kant, human dignity stems from capacities for freely determined rational judgments, that is, judgments not determined by natural contingencies. Yack is especially concerned with the social conclusions various thinkers drew from this polarity of freedom and determination. Kant and Hegel saw in it a more or less inevitable conflict between inner freedom and social contingency. In contrast, their leftist successors held that freedom should and could be realized within social relations. To the extent that social relations do not express freedom, they are dehumanizing and ought to be transformed.

Yack's historical treatment of this theme is wide ranging, scholarly, fluid, and often insightful. His view that German revolutionary thought can be seen as providing answers to "left Kantian" questions is an extremely useful context of interpretation. The book's critical project, however, will prove controversial. Yack's targets are Marx and Nietzsche, who, he argues, both fail to understand that extending Kant's notion of freedom to social relations is self-contradictory. For Kant, freedom is an inner, "noumenal" quality. Social existence is "phenomenal," and hence subject to causal contingencies. By demanding that human dignity qua freedom be realized within economic and cultural relations, Marx and Nietzsche misunderstand the possibility of extending Kantian conceptions of freedom to the realm of natural contingencies. Because they retain Kant's conception of human dignity while rejecting its dualism, they fall into self-contradiction. This in turn reflects Marx's and Nietzsche's failure to understand that

social contingency will always exist, and that therefore "total revolution" will always fail. Hegel, on the other hand, recognized these limits to freedom, and was therefore able to conceive of a society in which inner freedom and external social contingencies might co-exist. Thus Yack's philosophic aim is to show that a Hegelian critique of Marx and Nietzsche is more plausible than their own critiques of Hegel. His political aim is to show that revolutionary demands are misplaced, the product of wishful thinking based on self-contradictory premises.

Perhaps because Yack presents his critical thesis in genetic terms—through a history of ideas—it lacks the philosophic argument one might wish, especially given its sweeping qualities. Key arguments are often presented as if they were self-evident. For example, the most important claim for Yack's thesis is that Kant understood that inner freedom could not be expressed in social relations (Chap. 3). Yack asserts that the "left Kantian" demand that freedom be realized in social relations rests on a very obvious category error: the noumenal realm of freedom is incommensurable with the phenomenal world of social relations. But Yack surely misses the form of Kant's questioning. Kant assumed that moral and rational experiences exist and that they already have an imperfect reality in the social world. His critical thought had to do with identifying what we must presuppose (such as our freedom) for our moral experiences to be intelligible. The duality is an epistemological, not a cosmological, one. This makes a difference for political conclusions: although, as Yack points out, Kant was no revolutionary, he did see reforms toward legal, parliamentary regimes as conditions of extending and universalizing moral experience (see *The Metaphysics of Morals*). He even had a theory of the transformation process (see his *Idea for a Universal History*). Without the benefit of argument, it is difficult to see how Yack can sever Kant's notion of inner freedom so cleanly from social relations. Yet this assumption is what allows him to take for granted that left-Kantian demands for realizing freedom in social relations are misplaced.

A second limitation is that too much of the argument is carried by rhetorical attributions of totalistic goals. Of Marx and Nietzsche, for example, Yack writes that "by providing a

single focus for all our dissatisfaction with the world, the longing for total revolution raises great hopes for the transfiguration, however defined, of the human condition" (p. 368). There is certainly plenty in Marx and Nietzsche to support Yack's many statements to this effect. But there is also much more. Because he carries out his own analysis in such sweeping terms, one feels Yack has missed the often finely textured and subtle ways Marx and Nietzsche deal with the problems he so often acutely identifies.

Finally, Yack's project itself is likely to be controversial. By focusing on "philosophical sources of social discontent," he in effect brackets what many will see as equally important issues, namely, whether Marx, Nietzsche, and others provide new insights into rapidly changing institutions. Yack is certainly correct in arguing that demands for change can result from new ideas about what is desirable and about what can and cannot be changed. And he claims that his type of analysis does not pre-

clude others (e.g., p. 254). Nonetheless, Yack's critical conclusions are hard to separate from his approach. Because he investigates only conceptual sources of discontent—in isolation from both practices and institutional contexts—he adopts a perspective in which the individual is simply a posited internal conceptual state, whose qualities are explained only by inherited ideas. Likewise, institutions appear as undifferentiated, external "obstacles" to individual demands. Yack continually writes of institutions only in the abstract, as if they had the same status as any natural contingency in terms of their independence of human will. In spite of Yack's protestations to the contrary (p. 368-69), one cannot stay within his project and avoid the neoconservative conclusion that the best solutions to social problems are lowered expectations.

MARK WARREN

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AMERICAN POLITICS

Justice Downwind: America's Atomic Testing Program in the 1950s. By Howard Ball (New York: Oxford University Press, 1986. xviii, 280p. \$21.95).

The Whale and the Reactor: A Search for Limits in an Age of High Technology. By Langdon Winner (Chicago: University of Chicago Press, 1986. xiv, 200p. \$17.50).

The study of the social, economic, and political impacts of technology upon human society and its institutions has attracted the attention of numerous scholars in recent decades. Faith in the utopian conviction that technological innovation would lead to previously unimaginable improvements in the quality of life has been replaced by skepticism and ambivalence. The two volumes reviewed here use very different approaches to explore the unintended effects of new technologies.

Howard Ball's *Justice Downwind* is an absorbing case study of the aftermath of atomic-weapons tests conducted by the U.S.

Atomic Energy Commission (AEC) at its Nevada test site between 1951 and 1958. In the Cold War atmosphere of the 1950s, the goal of maintaining U.S. nuclear superiority over the Soviet Union took precedence over the AEC's obligations to protect thousands of residents near the test site from radioactive fallout. While the patriotic, fervently anti-Communist Mormons of Southern Utah initially viewed the tests as necessary for national defense, such acceptance gradually gave way to a growing uneasiness. The AEC responded with public relations gestures and suppression of research data that cast doubt on its public reassurances. Increasingly cases of leukemia in areas directly in the path of the fallout eventually led a group of "downwinders" to sue for damages in a federal district court in 1979. Ball does a masterful job of discussing the legal obstacles faced by the plaintiffs under the 1946 Federal Tort Claims Act, the strategies and arguments used by attorneys for both sides, and the judge's 1984 ruling that AEC negligence was

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"more probably than not" responsible for the injuries of 10 of the 24 plaintiffs (p. 138). He argues, however, that judicial remedies may not be the most appropriate ones in such cases because of the time required for litigation and the difficulty of establishing causation in situations where the adverse health effects of toxic substances may not appear until many years after exposure. A better remedy, in Ball's view, would be some form of legislative compensation. Efforts in that direction, however, have been stalled by the uncertain costs involved, congressional indifference, and opposition from the nuclear industry, insurance companies, and executive-branch agencies.

Justice Downwind's strongest points are its solid historical documentation and clear explanation of relevant points of law and bureaucratic politics. Several chapters are marred, however, by repetitious writing that could have been made leaner and more concise by better editing.

More ambitious in its aims and more theoretical in its approach, Langdon Winner's *The Whale and the Reactor* is a call to develop a "political philosophy of technology": "Just as Plato and Aristotle posed the question, What is the best form of political society? so also an age of high technology ought to ask, What forms of technology are compatible with the kind of society we want to build?" (p. 52). The scientists, engineers, bureaucrats, business executives, and politicians who make technological choices rarely pause to consider what values or ends are served by the technological marvels they create. Winner contends that certain technologies "constitute political phenomena in their own right" (p. 21) because their use encourages or requires particular kinds of political relationships—"centralized or decentralized, egalitarian or inequalitarian, repressive or liberating" (p. 29). He discourses on a wide variety of topics, ranging from the computer "revolution" to the politics of language and differing conceptions of "nature" in the writings of economists, philosophers, and ecologists. Conventional approaches to understanding technology, such as those offered by risk assessment, technological determinism, "appropriate" technology, Marxism, liberalism, or technology assessment, are seen as inadequate foundations on which to construct a philosophy of technology because they fail to ask or answer the most profound questions concern-

ing the social and political ends of technological developments.

The Whale and the Reactor is more readable, coherently argued, and accessible to a wider audience than the author's earlier *Autonomous Technology* (1977). Its greatest strengths are its elegant writing and reasoning and its skillful exposition of the broader issues lying beneath the surface of contemporary discussions of technology. The book offers only vague generalities, however, in tackling the problem of reconciling technical expertise and political democracy. While Winner suggests that technocrats and ordinary citizens alike join in a dialogue to discover which technologies will best promote social justice, equality, and other important political values, he acknowledges that such an enterprise would require "qualities of judiciousness in the populace" that are now rare, as well as the radical transformation of many existing institutions (p. 55). The reader is left wondering about the feasibility and effectiveness of such proposed solutions to the problems he rightly deplores.

The flaws in both books are minor, however, and both are valuable additions to the literature on science and technology policy. Students of bureaucracy and the law of torts may find the Ball book of particular interest, while those inclined to ponder deeper and more fundamental questions concerning relationships between humans and machines may turn to Winner.

JAMES R. TEMPLES

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Science, Technology, and Public Policy. By Richard Barke (Washington: Congressional Quarterly, 1986. ix, 245p. \$10.95, paper).

Science and technology policy, as a field, confronts two large problems. First, the subject matter is exceptionally complex and wide-ranging. While there are discrete policies for science and technology, there are also many less-clear relations between science and technology and nonscience policies. This is because science and technology is a part—generally the "front-end" or new part—of most other policy functions, such as defense, energy, transportation, and so on. Second, the field is inherently

fast moving, a fact that means that the subject matter changes so quickly that books are soon outdated.

Hence, those who teach in the field have difficulty finding readings that overcome these problems. There is a need for material that covers a great deal of territory and at the same time uses illustrative examples that are timely.

Richard Barke's *Science, Technology, and Public Policy* attempts to address these issues. It is successful in that it has much fresh information which will make it especially helpful in teaching. It is not as successful in aggregating and synthesizing the disparate elements of the field.

Barke uses a framework of four types of constraints to investigate general characteristics of all science and technology policies. These are law, knowledge, coordination, and politics. Using this framework, Barke essentially takes the reader through the interactions of science and technology with various institutions of U.S. government (president, Congress, bureaucracy, courts). He also discusses the public and scientists as participants in the science-and-technology policy process. He includes chapter-length case studies of science in policy (hazardous waste) and policy for science (the space telescope). A concluding chapter sums up the author's view of the problems associated with science-and-technology policy and of what, if anything, can be done about them.

What is wrong appears to be a problem specific less to science-and-technology policy than to contradictions in democratic government generally. While Barke does not propose any grand reforms, he does say that whatever reforms in the system are considered must deal with the sheer "complexity of the science and technology policy process." This complexity is manifested in three ways: "fragmentation of decision making, the inseparability of facts and values, and the balancing of considerations" (p. 213).

Barke has written a useful book. However, his framework does not really bring about the coherence he surely sought. Each chapter contains a sequence of topics that do not always flow together. There is a certain choppiness, and this characteristic is exacerbated by an overuse of quick examples.

The author seems aware that the chapters contain a lot of trees in search of a forest. He

points out that he hopes the two cases at the end of the book will help the reader "to see how the pieces fit" (p. vi). The cases do help. But they are late in the book, and might have been integrated with earlier material to advantage. Hence, the framework of constraints is only a partial solution to the dilemma cited earlier, namely, a field that is extremely disparate and wide-ranging.

These reservations notwithstanding, I would expect many teachers of science-and-technology policy courses to adopt this book. It helps to fill a genuine need. It is up-to-date and interesting—and its very limitations in conceptual approach may stimulate others to try alternative methods. The challenge is large but, as Barke has shown, involves a subject that is worth the effort.

W. HENRY LAMBRIGHT

Syracuse University

American Women and Political Participation: The Impacts of Work, Generation, and Feminism. By Karen Beckwith (Westport, CT: Greenwood, 1986. xiv, 185p. \$29.95).

Karen Beckwith has written a straightforward and useful book that uses mass-level data from the Michigan presidential election-year surveys to explore the extent and roots of gender differences in political participation over the period from 1952 to 1976. She proceeds thoroughly and systematically to investigate not only the bivariate relationship between gender and political activity but also the effects upon political involvement of several variables—among them, work-force participation; membership in a cohort that comes of age in a decade of feminist protest; commitment to a feminist ideology; class; and race.

The major findings are hardly astonishing. First, with the single exception of the greater likelihood that women will agree that politics is too complicated to be comprehensible to the average person, any differences between men and women are minimal. Second, employment status, political generation, and feminist ideology are less potent predictors of political activity than is socioeconomic status. Along the

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way, various interesting tidbits appear—such as the fact that the subjective evaluations of respondent's level of political information made by interviewers (which give men the edge) are not borne out by the actual replies to questions testing political knowledge (which show no consistent relationship between gender and information).

Both the data and the analysis have some weaknesses that bear mention, however. First, as Beckwith herself points out, she is confined by the limitations of the National Election Study—which, given its emphasis upon electoral activity, is a less than perfect source for a study of the multiple ways in which U.S. citizens take part in politics. In addition, the data are a bit stale. Beckwith argues that "by ending our examination of women's political participation in 1976, we allow ourselves the advantage of avoiding a consideration of the rise of an intense, antifeminist minority and the confounding effects of the conservative Reagan administration and its opposition to women's rights" (p. 5). However, this seems to be making virtue of convenience, especially when recent Michigan surveys contain many items of relevance to her subject.

With respect to data analysis, there is a problem with data reduction. Even when Beckwith creates several activity scales (some of which, of necessity, mix attitudinal items with measures of political participation), there are six indicators of political activity and seven presidential-election years. Naturally, the results for 42 data points contain lots of noise. Happily, Beckwith eschews the temptation to invent an explanation for each random blip in the data. However, the end result is a certain mushiness. Finally the great care with which the analysis proceeds—on occasion the book's greatest asset—is something of a mixed blessing. For example, Beckwith devotes several pages (pp. 35-39) to a well-documented discussion of women's employment. Taken on its own terms, the argument proceeds quite reasonably. However, readers of this volume probably do not need to spend several pages learning that women have traditionally taken responsibility for child care and household management or that they are paid less than men when they enter the work force.

In spite of these shortcomings, Karen Beckwith has given us a sensible and useful volume that makes an addition to the library of any-

one concerned with citizen political behavior or women in politics.

KAY LEHMAN SCHLOZMAN

Boston College

Administering the New Federalism. Edited by Lewis G. Bender and James A. Stever (Boulder, CO: Westview, 1986. ix, 369p. \$39.95).

This is an edited book which seeks to examine the implications of President Reagan's domestic program, called the New Federalism, for state and local governments and for selected policy and management issues. For the most part, the focus is on the first four years of the Reagan administration, although some attention is given to the more recent period. In the introduction, editors Lewis Bender and James Stever attempt to draw some parallels between large organizations and the U.S. federal system. They argue that many administrative federal issues are but a subset of similar issues facing any large organization. These include those dealing with questions of leadership, coordination, regulation and accountability, authority, and efficiency. The book's first section features articles by Timothy Conlan and Richard Williamson who discuss the origins of the New Federalism agenda and some of the competing values apparent in the Reagan program.

Section 2 focuses on the implications of New Federalism for state and local governments. Irene Rothenberg and George Gordon in their chapter compare some of the administrative guidelines issued during the Reagan years with those of previous presidencies. They conclude that the major effect of New Federalism for local governments "has been to interject another government between themselves and funding agencies" (p. 90). In one of the book's best chapters, Paul Posner argues that our traditional distinctions between categorical and block grants may no longer be as useful as other methods of classifying grant programs. He suggests degree of state-management authority to be the most important variable. In an equally reflective article, Susan MacManus, Robert Stein, and V. Howard Savage assess the impacts of New Federalism for Texas cities

in the 1981-1983 period, focusing especially on Houston. They find little "short-term" impact of the Reagan initiatives, citing Texas's changing economic and demographic conditions as more important. Focusing her essay on small cities in North Carolina, Beverly Cigler finds by contrast, "major impacts on North Carolina's small communities, influencing their ability to undertake their responsibilities" (p. 176). Examining the situation in Tulsa, Raymond Rosenfeld and Alan Frankle in their chapter suggest that the potential hardships of the New Federalism agenda prompted Tulsa voters to be more supportive of local taxes and higher service fees, thus blunting some of the negative aspects of the administration's program.

Part 3 looks at the implications of New Federalism in various policy areas. Charles Moore and David Sink examine housing policy in the Reagan years with special attention given to Birmingham, Alabama. They find the situation in Birmingham to be "grim" (p. 221). The Reagan program, they say, is contributing to a "halt of federal efforts to provide additional decent housing for those residents who cannot secure it on their own" (p. 221). Pinky Wassenberg examines state response to reductions of federal funds for water-pollution control programs and the administration's attempt to return responsibility for establishing and enforcing pollution control standards to the states. The result, Wassenberg argues, is to deprive states "of the increased federal assistance they [need] to increase the administrative and technical capacity of state programs" (p. 243). Dale Krane in his article examines the Community Development Block Grant (CDBG) program in Mississippi and state takeover of that program resulting from the Reagan initiatives. He concludes that earlier "fears" of state takeover "appear to be unfounded.... Mississippi's track record to date is positive" (pp. 265-66). In a similar vein, Susan MacManus finds little negative impact of CDBG restructuring in Houston. By contrast, she finds the new guidelines "offered the new mayor and the CD director the opportunity to get the program under control" (p. 289).

The final two chapters of the book offer something of a point-counterpoint exchange concerning the merits of New Federalism. Robert Agranoff and Valerie Rinkle suggest

that New Federalism may represent a major opportunity for opening new channels of communication—and thus new structures for planning and problem solving—between state and local governments. David Walker, in the book's concluding chapter, observes that the weakened nature of state governments in the U.S. federal system poses significant obstacles to any domestic program that rests for its success largely on the will and the capacity of sub-national governments for significant policy innovation and implementation.

One of this book's strengths is also one of its weaknesses. To their credit, the editors attempt to cover the full spectrum of issues surrounding the New Federalism program, including its history, philosophical background, administrative and programmatic impact, policy implications, and historic role in the U.S. federal system. Students will appreciate having one volume that attempts to cover all these topics; but, at the same time, the book cannot give adequate and complete attention to all these important issues. Perhaps a focus on only one or two major issues would have been advisable.

Further, the provocative framework established by the editors in their opening chapter is virtually ignored in the remaining chapters. The reader is left to draw his or her own conclusion as to how the points made by contributors relate or do not relate to the editors' opening arguments. Finally, the book would benefit considerably from a concluding chapter drawing together the various themes and findings suggested in the previous chapters. Here the editors might have discussed some of the contradictory findings of contributing authors, provided an assessment of New Federalism in all areas covered (is New Federalism working or not?), and might have placed these findings in the context of the themes they assert at the outset.

In spite of these limitations—and the problems apparent here do not differ markedly from those in other studies of the New Federalism—this will be accepted as a valuable addition to the growing literature assessing the implications of the Reagan presidency for domestic policy and politics. The editors and contributors bring us "up-to-date" on current assessments of New Federalism and, while those assessments ultimately may change, this volume has considerably advanced both for

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the lay and the scholarly reader our understanding of the Reagan years.

RICHARD L. COLE

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Intergovernmental Relations and Public Policy. Edited by J. Edwin Benton and David R. Morgan (Westport, CT: Greenwood, 1986. viii, 224p. \$35.00).

With the editors' assertion that "a wedding of IGR [intergovernmental relations] and public policy was inevitable" (p. 5), I tend to agree. Certainly it is an appropriate focus for an anthology. Unfortunately, it apparently proved easier to assert the theme than to find authors with ready papers truly combining the two elements. In consequence, this volume gives us several competent pieces on IGR as such (e.g., on the Berger court's view of IGR and on metropolitan regional associations), but rather fewer that really tie the two together.

Among those that impressed me (such judgments always involve partly subjective criteria of interest) are Thompson and Scicchitano's comparing state with federal Occupational Safety and Health Administration enforcement in a seven-year period ending in 1983; Hedge and Menzel's similar focus, but in the Surface Mining program; Ervin and Watson's on state-local patterns in Community Development Grant administration; and one on changes in fiscal centralization in the states, 1957-83, by Stonecash. Each of these substantially and successfully blends the two foci of substance and process.

Other topics covered include federal-aid cutbacks, environmental policy, a comparative piece on local finance in West Germany, and an interesting brief look into the crystal ball by Beam and Benton. This last has a couple of thoughtfully provocative pages (pp. 209-10) on a reverse twist: "the localization of national programs."

It should be noted that earlier versions of one-third of the papers in this volume appeared in *Policy Studies Journal* (vol. 13 [March 1985]).

Stimulating as some of the individual empirical papers and essays are, the thought occurs to me that anthologies need more than a com-

mon nominal topic to constitute coherent volumes. If editors can arrange for authors to work within some common framework of hypotheses to be addressed, of method, or of values to be explored, we readers would be most grateful. In the book covered here, the closest to a common thread is that most pieces use statistical analysis. Most are also stronger in data than in suggesting ideas and propositions about the nature of the reciprocal relationship between the two variables contained in the volume's title.

MICHAEL D. REAGAN

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Re-electing the Governor: The 1982 Elections.

Edited by Thad L. Beyle (Lanham, MD: University Press of America, 1986. xli, 336p. \$15.75, paper).

Re-electing the Governor: The 1982 Elections is the result of a larger effort to understand the determinants and consequences of gubernatorial elections. In 1982, a network of state analysts was established to study two different phenomena: in the 36 states holding gubernatorial elections the successful reelection of incumbent governors and, in cases where power changed hands, the gubernatorial transition process. This volume, edited by Thad Beyle, focuses on the reelections of 16 of the 19 governors who were successful in their bids for another term. The book begins with an introductory chapter by Beyle that quite nicely summarizes some of the important aspects of gubernatorial incumbency, both in elections and in the operation of government. This introduction is followed by two interesting chapters on the role of money in gubernatorial elections, the first written by Malcolm Jewell and the second, again, by Beyle. Unfortunately the book goes downhill rapidly from this point. What comes next are 16 separate state "analyses" of the reelection triumphs of incumbent governors in 1982.

I have serious reservations concerning the utility of this book for social scientists interested in the determinants of gubernatorial election outcomes. The strategy of inquiry pursued in this book is not well suited to answering this or any other general research question. The 16 state studies are almost without excep-

tion simply descriptive accounts of the political environment surrounding the election, the election itself, and the political changes that followed the reelection. The one exception to this approach is the chapter by McDonald and Morgan, which complements the descriptive account of the reelection of Oklahoma's George Nigh with a county-level analysis of socioeconomic and political variables as well as some descriptive evidence from survey data. Even if all other chapters had pursued this type of within-state systematic analysis, the book would still come up short. The problem with this book is not just its descriptive nature (although this is certainly a major problem) but also its lack of a comparative focus. Instead of coming away from this book with a general explanation of successful gubernatorial reelection bids in 1982, what the reader gets is 16 separate descriptions of gubernatorial elections.

Even if such idiosyncratic descriptions could be melded into a more general explanation it would be of little utility due to problems of case selection. This book includes "analyses" of only 16 of the 19 successful bids for reelection. The problem here is not just that three successful reelection bids are excluded but also that the six unsuccessful bids and the eleven cases where no incumbent ran are overlooked. To explain successful gubernatorial reelection bids, you cannot limit yourself to examining only those cases where governors are reelected. Surely the different conditions surrounding successful and unsuccessful bids for reelection, as well as the races not involving an incumbent, are important to an explanation of reelection successes. In short, to explain successful bids for gubernatorial reelection adequately you must be able to explain the determinants of gubernatorial elections in general. This is not accomplished by relying on descriptive accounts of a subset of elections "analyzed" one at a time.

The idea of postelection studies has a good deal of merit. My recommendation, however, is that the methodology be comparative and systematic rather than idiosyncratic and descriptive. This book is of the latter type and, therefore, is of little utility in a search for answers to general questions such as why some incumbent governors are successful in their bids for reelection.

As a final matter, I feel compelled to com-

ment on the poor quality of craftsmanship on the part of the publisher. The most glaring example of this is that the footnotes at the end of the introductory chapter bear no relationship whatsoever to the footnote marks in the text. As near as I can determine, the footnotes at the end of the chapter belong at the end of a chapter from the companion volume on gubernatorial transition. Other examples are of the more nitpicky variety such as misspellings, and asterisks in the tables with no corresponding explanation.

THOMAS M. HOLBROOK

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Presidential Transitions: Eisenhower through Reagan. By Carl M. Brauer (New York: Oxford University Press, 1986. xvii, 310p. \$22.95).

Presidential transitions, especially since receiving federal subsidies in 1968, have become something of a growth industry. Eisenhower's transition team was small, consisting of a few intimates; Reagan's, by contrast, was an elaborate effort with over one thousand salaried staff and numerous volunteers. There has, however, been no comprehensive book on the topic since Laurin Henry's *Presidential Transitions*, which appeared in 1960. Historian Carl Brauer, currently associated with the Center for Business and Government at the JFK School, has undertaken to update the story.

Brauer writes for a general, rather than scholarly, audience as he examines the basic dynamics of the Eisenhower, Kennedy, Nixon, Carter, and Reagan transitions. His treatment is broader than just the interregnum period (from election to inauguration) and spans the time from preelection transition planning to "established incumbency"—generally through the first year of the administration.

Each chapter-length case study of the five transitions explores a variety of themes, including the new president's interpretation of his mandate, his relations with the outgoing president, his stance vis-à-vis the Congress and the development of a legislative program, his relationship with the media, his appointment of White House office staff and cabinet and subcabinet officials, and a *tour d'horizon* of

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the various policy issues—foreign, military, domestic, and economic—that faced his administration.

Brauer's research is thorough, relying on memoirs, interviews with key participants, secondary reportage, and, in the cases of Eisenhower and Kennedy, presidential archives. The lack of archival material pertaining to the three most recent presidencies, though unavoidable, is regrettable. As scholars of the presidency know well, there are often considerable discrepancies between what is remembered by participants—who, as in the case of the Nixon administration, may have an ax to grind—and what is demonstrated by the historical record. (As Brauer volunteers, this renders his discussion, especially of the Nixon transition, "relatively tentative and speculative" (p. 122). Also disturbing is Brauer's apparent neglect of standard comparative analyses of several of his major themes, for example Calvin MacKenzie's work on presidential appointments (*The Politics of Presidential Appointments* [New York: Free Press, 1981]) and Richard T. Johnson's on White House organization (*Managing the White House* [New York: Harper & Row, 1974]). The result is a treatment which, at least for political scientists, is rather long on description and short on comparative analysis.

Brauer does extract some salient patterns from the transitions he studied. First, he finds, newly elected presidents fail to learn from the mistakes of their predecessors. They pay little heed to the incumbent's advice, for the latter, after all, has just been repudiated at the polls. In the postelection euphoria, "hope, sometimes arrogance, hubris, and a sense of infallibility run high. . . . This atmosphere . . . largely explains why new Presidents have been unable or unwilling to learn from their predecessors" (p. 258). Moreover, new presidents tend to overreact to perceived flaws in the man and administration they follow. Ike was too anti-political, Kennedy too antiorganizational, Carter too antifederal.

Second, new presidents are overly prone to become enamored of organizational reform and to bow to the false God of "cabinet government." All save Kennedy entered office proclaiming the virtues of the cabinet and underestimating the importance of the White House staff. Later, they were forced to recant.

Third, in spite of campaign rhetoric promis-

ing dramatic changes in style and policy, basic continuity between one administration and the next, Brauer argues, is the rule rather than the exception. Real policy change is basically incremental, occurring over the life of several administrations. "Symbol and rhetoric tend to change much more than does the substance of policy" (p. 266).

Lastly, and somewhat ironically, the caliber of the transition itself—though it may help the new president hit the ground running—may not have all that much influence on the overall quality of the administration. Carter's transition effort, which he subsidized with his own campaign funds, was among the most comprehensive and carefully planned. Yet in Brauer's view, Carter's administration was among the least successful. In spite of federal funding for transition efforts, Brauer observes, "a newly elected President's most fundamental problems are far more attributable to his own shortcomings" (p. 183).

Brauer makes several suggestions he urges new presidents to heed. Noting that Carter, a stranger to Washington, surrounded himself with Georgians and that Reagan, unfamiliar with foreign policy, made little effort to remedy his ignorance, Brauer counsels the victors to "take a good hard look at themselves and determine how to compensate for their weaknesses—their strengths just naturally assert themselves" (p. 257). They should also examine more closely the errors made by their predecessors, give up utopian notions about cabinet government, pay closer attention to selecting their senior White House aides and subcabinet appointees, and stop bashing the career bureaucracy—with whom they will need to establish effective working relationships.

It is much easier to criticize books than to write them. I wish, however, that Brauer had spent more time on the organizational detail and dynamics of the crucial interregnum period. Further, though he alludes to the influence of the president's personality and beliefs on his transition and early incumbency, this theme remains largely undeveloped. In spite of its shortcomings, however, Brauer's study is informative, well written, and a useful addition to the literature.

RICHARD L. SCHOTT

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Managing the Presidency: Carter, Reagan, and the Search for Executive Harmony. By Colin Campbell (Pittsburgh: University of Pittsburgh Press, 1986. xx, 310p. \$19.95).

Colin Campbell's major new book concerns itself with the institutionalized presidency, the performance of presidents, and presidential underutilization of the state apparatus. Campbell feels that writers on the presidency have created a false dichotomy between bureaucratic neutral competence and responsiveness. To Campbell, neutral competence need not lead to bureaucratic insularity and lack of responsiveness to the president. Neither does emphasis on creating a bureaucracy responsive to presidents mean that it will be incompetent. Rather, Campbell suggests that a mix between the two is possible and desirable, a situation that he terms "policy competence." Campbell's theory, then, is built on the interaction between neutral competence and responsive competence. A resulting typology occurs such that a bureaucracy that is both competent and responsive is the desirably "policy competent" bureaucracy. If it is competent but unresponsive, then neutral competence results; if incompetent though responsive, political incompetence arises; and lastly, if it is both incompetent and unresponsive, nonpolitical incompetence results. According to Campbell, presidents have some control over this mix.

In successive chapters, Campbell discusses the historical evolution of the cabinet, compares the U.S. cabinet experience to the British, looks at presidential organization of the White House, economic policy-making, and budgeting and management issues (chaps. 2-6). The chapters are presented as comparative case studies of the Carter and Reagan experiences, though a wealth of other materials are brought into the analysis, including British practices and presidential developments from FDR through Ford. Campbell's breadth and depth of knowledge is impressive, and he uses this knowledge to inform his case studies. Further, these case studies are presented within the analytic framework already mentioned and further developed as appropriate to each topic. As such, these chapters are not only informative but theoretical and analytical and therefore should serve as a model for the comparative-case approach.

These chapters provide a wealth of illuminating

information and insights as well. Some that struck me include the illusion of decentralization and cabinet consultation in the Carter administration. Rather, Carter's penchant for detail and his reliance on his Georgian aides centralized decision making. Secondly, Reagan's mixed collegial-hierarchical approach, using the troika of Meese-Baker-Deaver early on, not only suited Reagan's personal style, but may have been an effective device to ensure both collegiality and discussion. Further, Campbell argues that economic pressures on the budget of the 1970s and 1980s, which led to budgetary retraction, have undermined the Office of Management and Budget (OMB) as much, if not more, than its politicization in the years since Nixon. When budget expansion reappears, the neutral competence and institutional strength of OMB may reassert itself.

Chapter 7 is a quantitative discussion of career bureaucrats and political appointees in the Carter and Reagan years. These are based on interviews; the interview instrument is presented in the appendix. It is to be hoped that future scholars will rely on that instrument by repeating it, expanding and refining it. Thus, Campbell's work, along with Aberbach and Rockman's and Kessel's, should be seen as a replicable data base upon which to build.

I strongly recommend this book. Though long, it is well written and well reasoned. Its theoretical arguments are strong and usually persuasive. A range of knowledge is brought to bear. Hence, this should be regarded as a model for studying the presidency. Lastly, we should thank the University of Pittsburgh Press for issuing it so economically. The book should enjoy a wide readership, one that extends beyond presidency scholars to those interested in bureaucracy, public policy, and U.S. politics as well.

JEFFREY E. COHEN

University of Illinois, Urbana

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Presidents and Foreign Policy Making: From FDR to Reagan. By Cecil V. Crabb, Jr. and Kevin V. Mulcahy (Baton Rouge: Louisiana State University Press, 1986. xiv, 359p. \$15.95).

Among the many points of interest in this book, there is the quite remarkable circumstance of its timing. The authors indeed could hardly have asked for a more perfect example of life imitating art. The basic purpose of their analysis is to examine "the challenge presented to the State Department's traditionally paramount position in the field of external policy by a growing number of executive departments and agencies—a phenomenon that has become pervasive and progressively acute since World War II" (p. 42). For academics and the general public alike, bemused by revelations concerning the activities of Colonel Oliver North, Admiral Poindexter, and others on the National Security Council staff in the "Iran-gate" affair, this book provides an intellectual and historical perspective from which to view the recent events.

The authors remark early on that the existence of the National Security Council has not really solved any of the basic problems of coherence and direction in U.S. foreign-policy making. Indeed, the NSC "may have aggravated the problem by adding another layer to an already complex foreign policy hierarchy [and] by generating confusion about who is really in charge of American foreign policy" (p. 81). To this judgment, Secretary of State George Shultz presumably could offer only a heartfelt *amen*. The basic difficulty that Crabb and Mulcahy face, however, is in offering evidence or even hope that the current disarray in U.S. policy-making might somehow be put right with appropriate changes in the institutional framework of the decisional process. The authors have done a very good job of describing the disease. Whether they may be judged equally successful in suggesting cures is another matter.

The book essentially falls into three parts: an initial overview of basic problems in the U.S. foreign-policy process, a series of case studies of various administrations from Roosevelt to Reagan, and a concluding chapter offering some potential solutions to the disarray they have so effectively described. The tale told is a familiar but still depressing one: the

increasing fracturing of authority within the executive branch on foreign-policy matters, with resultant confusion and lack of direction on fundamental issues. A particular emphasis, as noted above, is on the gradual withering of the primacy of the State Department in the policy process, and the rise of competing centers of advice and even execution, particularly within the White House itself. The authors quote with approval the plaintive comment of former Senator Arthur Vandenberg that Congress could "only cooperate with one Secretary of State at a time." Unfortunately, there seems little prospect that the increasing babel of different voices on foreign policy will be replaced by a greater sense of order. Crabb and Mulcahy do offer some specific ideas for reform, such as giving the vice-president a much more substantial role in policy-making and of course restoring some of the perquisites of the State Department. They do concede that ideas for reforming the policy process have been something of a cottage industry over the last 40 years, and they are quick to admit that they offer no panacea of their own to the prevailing difficulties.

A rather depressing but perhaps also realistic conclusion from all this is that Tocqueville's famous analysis in *Democracy in America* (1835) concerning the *inherent* tendency toward confusion in democratic foreign policy may be a permanently operating factor. Certainly nothing written or done during the last several decades holds out much promise that his gloomy analysis can be refuted. The fact that this admirably researched and well-written analysis itself fails to promise any enduring solutions is perhaps the best testament to this fact.

STEPHEN A. GARRETT
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Cities of the Prairie Revisited: The Closing of the Metropolitan Frontier. By Daniel J. Elazar, with Rozann Rothman, Stephen L. Schechter, Mauren Allan Stein, and Joseph Zikmund II (Lincoln: University of Nebraska Press, 1986. 288p. \$17.75).

This book examines the far-reaching effects of Lyndon Johnson's Great Society and Richard Nixon's New Federalism on local gov-

ernment in the United States or at least in some of the small metropolitan areas of the Midwest. The study is a second look at the metropolitan areas, first studied in 1970 by Elazar, and is one of the few comparative urban studies conducted over an extended period of time. It is contended by the authors that other studies of urban political power give only snapshots of single cities at single points in their history.

Elazar and his coauthors return after an absence of 15 years to Champaign-Urbana, Decatur, Joliet, Moline, Peoria, Rockford, Rock Island, and Springfield, Illinois; Davenport, Iowa; Duluth, Minnesota; and Pueblo, Colorado. In the first book, Elazar examined the political and social changes in these cities from the end of World War II to the beginning of the Kennedy administration. In this second visitation, an attempt is made to consider how the areas adapted to the changes brought about by the Great Society, the Vietnam War, and the Nixon years. An attempt is also made to look at the efforts of the federal and state governments to impose new and different standards upon the local governmental units. The authors claim to analyze the struggle between federalism and managerialism in the local political arenas; and they correctly assert that the cities by and large were able to assimilate external pressure and adapt it to local mores.

This book may well be the forerunner of many. It may bring to life more studies of community programs revisited after a period of years. Indeed, it may be necessary to revisit these cities of the prairie again *after* the full effects of the Reagan years have been felt upon their local revenue patterns.

The issue of continuity and change in the local power system is one of the central questions in community studies, one that the "snapshot" approach cannot answer. Examining the question over time, as is done in this study, does enable one to draw "tentative" conclusions. As the authors contend, it would appear that the more institutionalized the broad base, the more the continuity. Similarly, the least continuity is found in the activities of the "reformers" who enter for a specific change in the formal constitutional structures.

The authors make an interesting comparison between the 1972 Federal Revenue Sharing Act and the Morrill Act of 1862. Their basic argu-

ment, however, is that the solution to the real problems of U.S. cities and metropolitan areas is not in simply increasing federal largesse but in recreating the civil community.

Another contention is probably equally valid, that the problem of growth is a problem of accommodating not just numbers but newness and transience—the conditions that have always been present in the U.S. frontier.

RUSSELL M. ROSS

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Corrigible Corporations and Unruly Law.

Edited by Brent Fisse and Peter A. French
(San Antonio: Trinity University Press,
1985. 235p. \$30.00, cloth; \$14.95, paper).

This is a valuable addition to the libraries of people interested in the control of corporate behavior. Its interdisciplinary perspective could have been even more valuable with stronger contributions on a few topics. The volume contains the leading and most innovative thinking about its subject; unfortunately, it also occasionally lapses into some less well-honed analysis.

The chapters aggregate to a complete inventory of ideas for promoting compliance with law. Not surprisingly, Christopher Stone both leads off and leads with provocative proposals. He reflects on the limits of what the editors characterize as "the bold interventionist theories" he advanced in *Where the Law Ends*, and describes new strategies such as committees that would collect in-house data on problems related to company activity, problems insufficiently understood to merit a full rule promulgation by a regulatory agency. John Braithwaite suggests lodging with enforcement agencies a clearly defined set of principles to indicate who will be held responsible for specified types of noncompliance and, most radically in this country, discouraging the use of lobbyists, "professional purveyors of conflict." Fisse and French review alternatives to the traditional fine and both discuss the community service order and forced negative publicity.

The term "idea" to characterize this inventory is consciously chosen. Some of the chapters state explicitly and others suggest that

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much of the analysis of corporate compliance is at the level of thoughtful speculation by intelligent and experienced scholars or extrapolation from very limited data. As Fisse and French say in their useful introduction, "the interdisciplinary study of corporate activities crawls in its infancy." Braithwaite, comparing the public-interest director unfavorably with his notion of creating tension in the suites and tainting the chief executive with knowledge of illegalities, recognizes that the former strategy has not been found lacking on the basis of testing and empirical work. The volume communicates this message in a less satisfying way by including a couple of chapters that seem driven by ideological conclusions and read like sermons on the evils of capitalism and bureaucracy. For example, a slap at the agency bureaucrat seems misplaced in a scholarly treatment of a complex topic. Certainly, there is variability in competence and professionalism among public employees, but that should be a starting point for an organizational analysis of compliance rather than a self-evident principle explaining law violations.

Geis's thoughtful chapter notes that even careful empirical work may not be conclusive from a policy perspective. His is a review of recent research with some surprises on who is doing the major work. His skepticism about some of the more modern approaches for studying corporations is explicit: "We will have further detailed empirical work, with its precision purchased at the price of reducing complex matters to operational terms" (p. 80). (He feels that approaches that combine legal and social-science perspectives hold promise.) The reader joins in the skepticism that stems from certain results Geis reports including the findings that "sentences are generally tougher for college-educated offenders than for persons in other categories . . . [and that] . . . the possibility of imprisonment rose with an increase in the occupational status of the offender."

Another of the book's strengths is its identification of sources of corporate noncompliance. Stone informs us that some theory holds that legislative fines may be so designed to encourage noncompliance for a greater social good. Braithwaite recognizes the importance of individuals in an arena that is often analyzed with systems ideas and abstractions. These can lead us and enforcers to forget that identifiable people occupy significant and highly influential roles in even the largest cor-

porations. Executives set a tone, which may make them culpable and responsible. The Australian criminologist notes that those at the top can create unreasonable pressure that fosters unethical and noncomplying behavior. In a report on his study of the coal-mining industry Braithwaite describes the opposite phenomenon. Safety leaders in the industry had several things in common but the most interesting and generalizable was "a corporate philosophy of commitment to safety and communication of the message that top management did not perceive cutting corners on safety to achieve production goals as in the interest of the corporation." He concludes that "if we wish to maximize the compliance impact of additional investment in enforcement, the most productive targets will be chief executives."

A particularly strong chapter is Byrne and Hoffman's which uncovers the religion that sometimes masquerades as scientifically tight analysis of organizational behavior. This is a careful and comprehensive critique of the law and economics school that has dominated studies of organizations in recent years. Special attention is given to antitrust, but the chapter has broader implications and employs a wide literature.

The breadth and coverage of the volume are appropriate even if difficult to circumscribe conceptually. There is, for example, a chapter on factors that influence the exercise of prosecutorial discretion, written by a former U.S. attorney. This is a lawyer's article but it is written clearly and may be of general interest to those who wish to explore within the black box of the enforcement variable. Rakoff educates us well on constitution-based limitations, as well as on ethical and eye-opening practical considerations in the exercise of discretion, including the prosecutor's estimate of the propriety of the motives of the complainant.

Fisse and French have assembled some very good thinking by a leading group. The book could have been somewhat more fully edited (the same few cases and anecdotes are used throughout); nonetheless it should be welcomed by all students of corporate compliance. It moves the interdisciplinary understanding of the corporation's reaction to law a large step forward.

JOSEPH DiMENTO
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Controlling Bureaucracies: Dilemmas in Democratic Governance. By Judith E. Gruber (Berkeley: University of California Press, 1987. xi, 232p. \$29.95).

Try ordering a bureaucrat around, Judith Gruber argues, and the result is likely to be trouble. The effectiveness of an agency's functions will suffer, and enforcement of controls can consume vast resources. Understand how the agency goes about its work and how the bureaucrat views his or her job, on the other hand, and things can be different, for bureaucrats see some controls on their behavior as acceptable, or even useful, and will accept them in exchange for the important resources of outsiders.

Gruber's focus is the classic question of how unelected officials can be democratically controlled. Her book begins with an analysis of the options for controlling bureaucracies. It continues by linking various forms of control to theories of democracy, and by specifying their costs. Then comes evidence, gathered from interviews with housing, school, and fire-department employees in a "moderate-sized northeastern city," on the ways bureaucrats view themselves and their work. A theoretical chapter follows, on the differing meanings and forms of control in differing policy areas; and then the interviews are reintroduced in order to show how bureaucrats perceive such control efforts within their own domains. A final chapter returns to basic questions and begins to develop the notion of "control as exchange."

This is a successful book. It pulls together, in a disciplined way, much of what we know about bureaucracies and the ways we have tried to control them. And instead of relying for evidence upon hypothetical cases or anecdotes from policy battles long gone by, Gruber uses her interviews to construct a much more dynamic picture of the bureaucrat's world than we are typically given. The result is an understanding of the generally dismal record of bureaucratic-control efforts, and a surprisingly hopeful conclusion that we can indeed do better.

"Doing better," however, will require a sophisticated understanding of the bureaucrat's world, and of the variations to be found in different policy areas. This book offers the beginning of such an understanding, but much remains to be done. The notion of control

through exchange is presented and briefly discussed at the end of the book. I wished for a full chapter offering a more complete development of the idea, perhaps showing how it might be applied to the three agencies under study. Another issue underlying the whole analysis—that of whether or not the people and/or their elected representatives are capable of democratic self-government—also needs more discussion than it gets. And while an organization's environment is identified—along with its core technology—as a factor defining important variations among policy fields, the idea seems quickly to lose out to technology as a focus.

But these complaints amount mostly to a wish to see the analysis extended further. The book is well argued and is rigorous at the theoretical level while remaining sensitive to the variations to be found among public agencies and types of policy. By talking to the bureaucrats, listening with a sensitive ear, and carefully presenting their views, Gruber has avoided the aridity which characterizes too many discussions of bureaucracy. And her conclusions on control-as-exchange remind us that, while our elected representatives often turn over significant funds and powers to unelected officials in the pursuit of goals they have only vaguely defined, bureaucrats still need the resources and backing of outsiders. Within their agencies, everyday concerns may well be defined in administrative terms, not in the language and concepts of democratic theory; but politics still matters, and therein lie opportunities to reconcile bureaucracy with democratic control.

MICHAEL JOHNSTON

Colgate University

Going Public: New Strategies of Presidential Leadership. By Samuel Kernell (Washington: Congressional Quarterly, 1986. xi, 251p. \$16.95, cloth; \$11.95, paper).

Over the past few years, the number of studies examining the presidential leadership strategy termed *going public* has mushroomed. Undoubtedly spurred in large part by Ronald Reagan's frequent use of the strategy, these studies grapple with what constitutes *going public*, why it is on the rise, and how to deter-

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mine if it is successful. Samuel Kernell's book, *Going Public: New Strategies of Presidential Leadership*, is the latest of these works and is certainly among the best. Still, Kernell's effort like those before, leaves the inquisitive presidential scholar with many lingering questions.

The book defines *going public* as a president's promoting "himself and his policies in Washington by appealing to the American public for support" (p. 1). Such appeals are on the increase, according to Kernell, because institutionalized pluralism is being replaced by individualized pluralism; the latter characterized by individuals who have few group loyalties and prefer immediate payoffs. In short, as the modus operandi of the Washington political community has shifted, Neustadt's bargaining president has been replaced by a public president. One is left wondering, though, how completely going public has actually supplanted bargaining. After all, an administration pursues hundreds of policy initiatives during its tenure, far more than its president could publicly push.

More troubling, however, is Kernell's reliance on individual pluralism as an explanation for the rise in going public. A more complete job needs to be done of disentangling the complex relationship between the shift from institutionalized to individualized pluralism and recent advances in communications and transportation. Kernell correctly recognizes that going public would be virtually impossible without telecommunications and high-speed air travel, but he is less clear about where these technologies fit in the causal chain culminating in the public presidency. It seems quite plausible that a president with access to modern technology would often opt to go public even if bargaining reigned supreme in Washington. Put differently, the relationship between individualized pluralism and going public may prove spurious when controlling for advances in technology.

Also intertwined among these forces is the Washington press bureau. Kernell does a superb job of summarizing the changing relationship between the president and his press corps, but his treatment of the role of the press in the dynamic process of going public is less complete. Does the aggressive contemporary Washington bureau force a president to go public, perhaps even "create" a public president? Or does going public shape the press,

perhaps making it part of the new individualized pluralism? The reciprocal relationships between these and other key forces need to be fleshed out, first in theory and then in empirical analysis.

The book excels as a chronology of going public. A good number of examples are described in detail, such as the Truman Doctrine speech and Reagan's budget crusades. *Going Public* would be strengthened, though, if a comprehensive scorecard were kept. How often have presidents' public appeals met with success? Also useful would be a more complete breakdown of going public by administration, party, policies (e.g. domestic or foreign), medium used, and whether the appeal was national or group-specific.

The strongest section reexamines an old question, What determines presidential popularity? Kernell argues that public approval is the "keystone" of going public; that without it neither the populace nor the Washington elite will likely be persuaded by presidential appeals. Maintaining a high popularity rating is crucial, then, to the success of the new leadership strategy. And through some nifty original analysis, the author demonstrates conclusively that high popularity and healthy economic conditions go hand in hand.

Going Public addresses many of the tough questions raised by the emergence of the public presidency. While its answers sometimes fall short, the book successfully frames these questions, thus paving the way for much fruitful research in the future. For this, Kernell deserves substantial praise: bringing some order to such a complex phenomenon as going public is no easy task.

TOM RICE

University of Vermont

Taking Reform Seriously: Perspectives on Public Interest Liberalism. By Michael W. McCann (Ithaca, NY: Cornell University Press, 1986. 345p. \$29.95).

It has been roughly 20 years since the liberal public-interest movement began to gain widespread attention for its advocacy in Washington. During this time there has been little critical evaluation of the movement's goals and political ideology. This is the task that Michael

McCann sets for himself in his new book, *Taking Reform Seriously*.

In the first half, McCann offers a careful and detailed analysis of the political and social philosophy of the liberal public-interest movement. This was no easy task, as there is no collection of great works by public-interest activists to consult nor any commonly accepted manifestos to review. To describe the world view of the public-interest movement, he had to piece together the work of a wide variety of scholars and activists from a highly diverse set of sources.

McCann's synthesis of the political ideology of the liberal groups alone makes this book an important contribution to the literature. But he goes much farther to provide a thoughtful and provocative argument that the philosophy of the movement has in many ways worked to undermine its long-term viability. He emphasizes how the movement's consumer and environmental proposals ignore the very real concerns of working-class citizens. McCann views the liberal groups as the political arm of an elitist class, far removed from the mainstream of U.S. life and thus all the more vulnerable to the vicissitudes of the political environment.

McCann's harshest criticism is directed at the movement's failure to help build a true participatory democracy. He notes that "the reformers have managed to mobilize voluntary support from millions of card-carrying members," but at the same time "they have actually engaged very few of these members as genuinely active participants in political life" (p. 170). Although it is one of their pronounced goals, Washington-based public-interest lobbies have generally done little to nurture and revitalize citizenship in the United States.

McCann sees the tensions and contradictions of public-interest liberalism as a cause of its downfall. After many important victories in its early years, the movement is said to have faltered badly as opposition to its goals has mounted. In this vein, McCann makes a surprising judgment about the performance of public-interest groups during the Carter years. He holds that the movement's serious decline began at this point as public-interest groups found the Carter administration to be "a bitter disappointment" (p. 127). There were some disappointments to be sure, such as the defeat of the proposed Consumer Protection Agency.

Yet day in, day out, public-interest lobbyists had generous access to agency policymakers throughout the administration, and they played an important role in developing public policy during those years. At the USDA Food and Nutrition Service, for example, former public-interest lobbyists ran the whole show. The open access that hunger lobbyists enjoyed at this agency would make a Tommy Boggs or Charles Walker jealous.

McCann's view of the Carter years is important because it goes to the heart of his argument that the movement has suffered a fundamental decline because of its unsound philosophical foundation rather than a more transitory downturn because of a turn to the right that came with the election of Ronald Reagan. There's room for disagreement on this question, but McCann weakens his own case by not offering clear criteria for evaluating the relative level of success for these groups.

Although I wasn't convinced on all counts, I found *Taking Reform Seriously* to be an original and enlightening book. McCann's work is valuable reading for all those interested in contemporary interest-group politics.

JEFFREY M. BERRY

Tufts University

Thinking in Time: The Uses of History for Decision Makers. By Richard E. Neustadt and Ernest R. May (New York: Free Press, 1986. 329p. \$19.95).

Few would quarrel with Machiavelli's observation that in order to know what is going to happen, one must know what has happened. But the lessons of history can be obscuring and misleading as well as illuminating and orienting. Clio is an enigmatic muse, and her analogies are Delphic. Today, we would hardly share young Ranke's optimism when, in a fervent address to Goethe in 1817, he spoke of "the solid ground of history."

This collaborative work by Neustadt and May, which is based in large part on their jointly taught course at Harvard on the pitfalls of decision making, returns to the major theme of a previous book by May on the (misapplied) "lessons" of history ("Lessons" of the Past [Oxford University Press, 1973]). The present

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study is replete with examples and case studies, mostly drawn from the U.S. experience, that illustrate the dangers of being guided by historical analogies that turn out to be inappropriate for the contemporary context to which they are applied. The central theme of the book is that policymakers often use history badly and that they must learn to use it more discriminately. Neustadt and May recommend that decision makers divide the elements of a decision situation into three categories—*known*, *unclear*, and *presumed*—and that they carefully examine historical analogies to determine if and how they are applicable to the case at hand. Issues should be identified clearly, their “time lines” should be connected with considerations of feasibility, and events should generally be seen within the “stream of time.” The authors argue that even marginal improvements in decision making are worth seeking, and that they are obtainable if one would only raise the helpful journalistic questions *who*, *what*, *when*, *why* and *how*.

The “catalog of issues” developed by Neustadt and May is drawn from both domestic and foreign-policy issues; it includes both successes and failures, and it is extensive. The case studies are examined in some detail and are grouped under the following headings: “Drawing Analogies” (Vietnam, the onset of the Cold War, Korea, the 1976 swine-flu scare, Verdun, and Versailles); “Inspecting Issue History” (the Mayaguez incident, the Pittsburgh political machine, Social Security, and wage and price controls); “Probing Presumptions” (the Intelligence Process, the Bay of Pigs, and U.S. aims in the Korean war); “Placing People” (Mary Anderson and the Women’s Bureau, J. W. Fulbright, Martin Luther King, Malcolm X, Frances Perkins, and Vietnam advisers); “Placing Organizations” (the CIA and the United Mine Workers of America); “Noticing Patterns” (economic growth, Keynes and Friedman, U.S. analyses of the Soviet economy, and other cases); and “Judging Change” (the Great Crash, the Civil War secession, Prohibition, George Marshall, and George Washington).

This book is full of common sense and the wisdom that its persistent application engenders. It is a remarkable achievement, rich in analytical insights, written in a lucid and relaxed style, measured and fair in its political judgments. And yet there is something

curiously unsatisfying about this enterprise, perhaps because its central premise does not fully correspond to what we experience in our daily lives. Surely we (as well as decision makers) make our choices not only by considering possibly appropriate analogies that come to us from the past, but also by interpreting those that are offered to us by the present. We apply the lessons we obtain currently in one context to the decisions we need to make in another context. We may see ourselves in a diachronic stream of time, but we also occupy a place in a synchronic range of space. Our use of “history” is not only linear and vertical but lateral and horizontal. Indeed, there is ample evidence that decision makers are guided in their actions by present as well as past experience—and both may be misapplied. For example, our early involvement in Vietnam in the fifties stemmed both from misplaced historical analogies of the past and from the seductive contemporary “lesson” that dividing Vietnam could, like the concurrent cases of Germany and Korea, serve as the basis for an accommodation with our opponents. We sought to apply the principles and assumptions of our containment policies, which were taking hold in Europe, to other parts of the world, where they obtained little leverage.

By placing such a heavy and exclusive emphasis on the dangers of improper thinking in *time*, Neustadt and May ignore the equally compelling and perilous analogies that come from thinking in *space*.

WOLFRAM F. HANRIEDER

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Liberalism at Work: The Rise and Fall of OSHA. By Charles Noble (Philadelphia: Temple University Press, 1986. x, 292p. \$29.95).

Liberal reforms such as the worker protection regulation implemented by the Occupational Safety and Health Administration (OSHA) are unlikely to succeed in the United States without strong grass-roots organization of beneficiaries—in this case workers—and greater willingness on the part of the state to intervene more directly into the investment decisions of the corporate United States. Noble's underlying argument in this book is

not simply that OSHA failed to have much impact on worker safety and health but that reformers missed a major opportunity to use this legislative initiative in order to introduce structural changes to alter power relationships at the plant level, where dominant corporate interests work against safety and health improvements.

During the past century of capitalist development, market pressures have led management to two strategies that ultimately lead to underinvestment in worker safety and health, one emphasizing full control over the production process and the other emphasizing ways to minimize costs of production. Factors such as wage premiums, production losses attributable to accidents, and the need to conserve labor may partially offset investment disincentives, but Noble notes the problems with each of these factors and concludes that they are not enough. Liberal statist reforms developed to counter recurrent problems of workplace safety, primarily workers-compensation schemes and factory legislation, are also ineffective because weak government agencies were never given sufficient authority and enforcement resources to challenge the dominance of business interests seriously, particularly in matters affecting the production process. Only workers organized at the factory level could have provided the government agency with sufficient resources for effective intervention in production processes, but the one likely source of this organized effort, the industrial unions, had chosen to seek economic rewards rather than challenge management's strategy of controlling the production process. In combination, reforms favored by unions and liberal reformers, according to Noble, are doomed to failure unless the need for structural changes are recognized.

Compared with the half dozen existing political-science studies about OSHA, Noble's broad historical account of capital-labor-state relations in the United States as they impinge on safety and health issues in the workplace uses more radical terms to analyze government interventions in "industrial capitalist economies." Yet the tale Noble tells provides a skilled analysis of familiar themes of regulatory capture. Increasing workplace problems during the 1960s discredited existing health and safety policies and indicated rank-and-file discontent. A number of fortuitous circumstances

led to the passage of OSHA, including analytic groundwork by a few legislative activists, the political entrepreneurship of the Johnson White House intent on appealing to union workers, the support among Democrats in Congress for protective social legislation, and the disarray of business lobbies. Noble's detailed account of the origins of OSHA, based on interviews and memos from administrative officials, is one of the most interesting contributions of the book to understanding why a major legislative bill emerged despite the disinterest of unions (the presumed beneficiary). Factors increasing the impracticalities of policy processes are well portrayed, particularly in the amusing account of how business allies inadvertently strengthened the bill with suggestions intended to weaken it.

The rest of the story is familiar but not as convincingly told. Business groups organized to oppose the burgeoning social regulations, while labor failed to take initiative. With the help of a declining economy and a White House review process responsive to economic concerns of business and academic allies, businesses succeeded in rehabilitating the values of market capitalism. OSHA's activities were systematically curtailed. "By the end of 1984, social regulation was dead in the water" (p. 161). But the accounts of changes in OSHA's actions, particularly under Carter, point out the variety of forces behind these changes, many of which counter the main theme of inevitable decline. OSHA is found to have some effect but not enough. Noble does an excellent job of documenting the political forces aligned against "liberal" reforms based on regulatory agencies but says little about how his preferred strategy of increasing worker controls at the factory level and more extensive state controls over investments is likely to bring about greater effects in the same political setting. Similar strategies to mobilize new groups in the war on poverty politically, failed for predictable reasons. Nonetheless, Noble's broad perspective, extensive scholarship, clear presentation, and reformist sympathies make this book a delightful change from the normal market perspective currently dominating discussions of social regulation.

JOHN SCHOLZ

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Alternative Americas: A Reading of Antebellum American Culture. By Anne Norton (Chicago: University of Chicago Press, 1986. ix, 363p. \$24.95).

This book follows the tradition of Tocqueville, Louis Hartz, Sacvan Bercovitch, and others who have attempted to explain the United States' cultural tradition. In *Alternative Americas*, I think we have a valuable, although perhaps controversial, addition to that corpus. Although her narrative encompasses antebellum culture in the U.S. West as well as in the North and South, her primary focus is on the North and South, and her major contribution lies there.

Norton argues that U.S. political culture prior to the Civil War was a political culture marked by an increasing separation between the North and the South. This separation reflected a difference between the regions that arose not because of a disagreement over slavery, or between different views of federalism, or even between the existence of an increasingly commercial North and a feudal South. These arguments have been made by others. Norton argues that such views of the South "ignore the language of the era" (p. 4). Instead, she argues that the distinction between the two regions is one that is fundamentally and explicitly sexual in nature: "Throughout America, throughout the antebellum period, the North was identified as masculine and the South as feminine, in an elaborate constellation of metaphors" (p. 4). These perceptions of the South and the North are revealed in the relationship that developed between white Anglo-Saxon Protestant males and groups which were marginal, or liminal, to these societies. In the North, the liminal groups were immigrants—especially the Irish—and women. Slaves, women, and Indians were the liminal groups in the South. In the case of both regions, however, the relationship between liminal and dominant group was perceived in sexual terms. Thus, Norton argues that in the North, the needs of industrialization resulted in a valuation of "masculine traits," for instance, rationality, hierarchy, and community defined by law. The South, with its primarily agrarian economy, tended to see itself primarily in "feminine" terms, valuing self-expressiveness, individual autonomy, and emotionality.

The different development of North and South has important consequences for the relationships between the dominant and liminal groups in each region. In the North, feminine characteristics were ascribed to liminal groups, and resulted in attempts to "reform" them. The goal here was to assimilate these groups into society not as equals but as part of a docile labor class. Liminal groups were to perform as docile "wives" to a masculine paternalistic industrial elite. Thus, the North proposed a paternal state that would use its power both to control and to mollify. The South chose a different path, appropriating the characteristics of liminal groups and identifying with them. Thus, because they identified with women and their maternity, southerners chose to adopt a model of social relations that mimicked familial ones. For example, Norton argues that southerners identified themselves with the Indian and that that identification led them to describe the supplanting of the Indian by the white man as a fraternal conflict, like that between Cain and Abel. Southerners saw that the Indians "must be torn from the breast to make room for the encroaching whites" (p. 145). The South rejected the model of state paternalism embraced by the North but exhibited a plantation life that was itself paternalistic. In both cases, the relationship between dominant and liminal groups was one that ultimately denied the equality of the members of the liminal groups.

This very short synopsis does great violence to the richness of Norton's argument. However, rich as the argument is, it is not entirely convincing in its discussion of the South. The basis of the South's feminine character is grounded by Norton in the agrarian nature of the region. The continued reliance on the earth, and the resultant ties to natural process of growth and season, she argues, results in the image of the South as "motherland." Two points should be noted here, though. The first is that at bottom, I think it might well be argued that the nature of the relationship between the southerner and the land is not harmonious, but instead domineering. It is plausible that the southerner perceived land as something to exploit, particularly given the historical accounts describing the wastefulness of Southern agricultural methods. This would make their attitude toward nature no more salutary than that of their northern counter-

parts. Second, although the southerner might well have appropriated "feminine" traits to define his self-image, it seems to me that this led to a redefinition of masculinity—the image of the indulgent, sensual, violent, and independent Southern male is that of the typical "good ole boy," who would quail at the notion that he was "feminine" in any way—rather than a true identification.

Finally, I found it interesting that Norton makes an association between agrarianism, femininity, and a republican political culture. Certainly, Rousseau, that old advocate of republicanism, would find the association of femininity with republican government surprising. One might well ask why this association would take place in the United States, but that goes beyond the scope of this work.

These comments are hardly meant to be criticisms. This book is thought-provoking and well argued and will be of great interest to those who work in political theory, women's studies, Afro-American studies, and U.S. politics. It is a fine book well worth a first taste and, like a fine wine, a second as well.

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Making Cancer Policy. By Mark E. Rushefsky
(Albany, NY: State University of New York Press, 1986. xiii, 257p. \$39.50, cloth; \$12.95, paper).

The sudden proliferation of federal health, safety, and environmental regulatory programs in the 1970s is one of the major events of recent U.S. history and governance, yet one whose substance has received surprisingly little attention from political scientists. For more than 10 years now, a primary objective of these programs has been the control of chemicals that may cause cancer and to that end the elaboration of detailed policies—"cancer risk-assessment guidelines"—setting standards of evidence for use in regulatory proceedings.

Mark Rushefsky's *Making Cancer Policy* provides an excellent analysis of the interpenetration of facts and values, of science and politics, that he argues is inevitable in such a process. Unlike many other books on environ-

mental politics and policy, it is neither an advocacy tract nor merely an account of the interest-group politics surrounding these issues, but a penetrating and insightful synthesis of theory, history, and substantive analysis of a complex and technical policy issue. It should be valuable not only to readers interested in cancer policy, but also to those interested in risk assessment as a form of policy analysis and in the politics of science and regulation more generally.

Four themes run through this material: the problem of imperfect knowledge and scientific uncertainty; the existence of controversy within both the scientific community and the larger society; the inevitability of mixing political judgments and assumptions into ostensibly scientific assessments supporting regulation; and the political uses of science itself by advocates of particular policies. A central principle of recent Environmental Protection Agency (EPA) policy, for instance, is that "risk assessment" is a purely scientific enterprise, to be clearly distinguished from "risk management," in which political, economic, and other value considerations are added in and regulatory decisions made. Rushefsky argues persuasively that this distinction is untenable, that policy judgments and value-laden assumptions are inevitably commingled with regulatory uses of science even in the assessments, and that regulations are therefore more appropriately developed through such mechanisms as "consensus dialogues"—providing for open debate over what assumptions are to be used—than through the pretense of scientific neutrality.

Rushefsky develops these themes first by describing the regulatory use of science to produce risk assessments, then by documenting the evolution of cancer regulation and related policies (with particular emphasis on the period since 1970), and finally by critically analyzing the issues that must be addressed in cancer regulatory policy today and the arguments offered by other writers on the subject. For a theoretical framework he uses two sets of concepts: John Kingdon's characterization of policy agenda setting as growing out of the interactive evolution of problems, policies, and political events (*Agendas, Alternatives and Public Policies* [Boston: Little, Brown, 1984]) and to a lesser degree Patrick Hamlet's characterization of technological policymaking as occurring in distinct "arenas" or

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"estates," each subject to its own distinct incentives, influences, pressures, constraints, and goals ("Understanding Technological Development," in *Science, Technology and Human Values* 9 [Summer, 1984]: 33-46).

Particular strong points of Rushefsky's book include its mature treatment of all sides in what has become a shrill and contentious policy issue, and its frequent use of concrete cases to illustrate its points. Its title notwithstanding, it does not address all aspects of cancer policy (the politics of cancer research funding, for instance, or the politics of tobacco subsidies); nor does it pursue some questions as far as one might wish. One key question left unanswered, for instance, is whether ideology or science was the primary driving force in the policy changes Rushefsky describes. Were EPA officials (such as John Todhunter, for instance) simply political ideologues who used scientific assumptions selectively to justify their pre-conceived positions, or were they good scientists articulating changes in scientific understanding whose implications happened to be consonant with the Reagan administration's agenda? Both the reputations of the individuals and our understanding of subject turn on the answers to such questions, yet Rushefsky stops short of answering them.

Notwithstanding these minor criticisms, Rushefsky's book provides a solid and readable scholarly treatment of the regulation of environmental carcinogens and a valuable contribution to the literature.

RICHARD N. L. ANDREWS

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Disenchanted Realists: Political Science and the American Crisis, 1884-1984. By Raymond Seideman and Edward J. Harpham (New York: State University of New York, 1985. vii, 295p. \$39.50, cloth; \$12.95, paper).

The authors of this volume argue two basic theses: First, from the beginnings of U.S. political science there can be traced a lineage of practitioners who constitute a "third tradition." These individuals, all characterized as "liberals," believed that the problems of our U.S. society could be solved by "linking objective professional political study with political

reforms." Convinced that a scientific political science could lead to whatever changes were required to make the U.S. democratic system function effectively, they "blended scholarship and political advocacy, a science of politics with a science 'for' politics" (p. 2).

Second, the reformist proposals espoused by the third tradition (from Lester Ward in earlier years to Theodore Lowi and Walter Dean Burnham today) have been not only manifestly but inescapably unsuccessful. Consequently, if U.S. political science is meaningfully to address our current political, social, and economic ills, it must abandon the "centrist liberalism" of the third tradition and move drastically to the right or to the left.

These two propositions, it is important to note, have no inherent logical relationship. That is, the truth or falsity of one has no necessary implications for the truth or falsity of the other—a view, I suspect, with which the authors might not agree. In any event, I will deal with them in order.

According to Seideman and Harpham, there are two long-established traditions in U.S. political thought (a term sometimes employed as synonymous with *political science*)—the *institutionalistic* tradition and the *radical democratic* tradition. The institutionalists (they point to the "American Founders") are skeptical about human political capacities, inclined to put their faith in the Constitution, and utilize "a political vocabulary of system, mechanism, control, realism, skepticism and 'facts'" (p. 5). In contrast, the radical democrats (e.g., Tom Paine) trust popular virtue and sentiment and favor "continuous experimentation and impromptu forms of popular power associated with equalitarian democracy" (p. 6).

The newly discovered third tradition, however, seeks to go "beyond mechanics and spontaneity." It is less a synthesis of the other two, according to Theodore Lowi's Foreword, than "an organized effort virtually to deny that these are separate and inconsistent streams of political thought and values" (p. xiii).

What do they have in common, these eponymous "disenchanted realists" (a phrase that appears so rarely in the text that one senses the publisher's helpful hand) who constitute the third tradition? They are all "liberal" (a term never defined and used with remarkable elasticity); they all believed in a "science of poli-

tics" or a "scientific politics" (terms used interchangeably); they regard the state as essentially benign; they do not think that "citizens are by nature greedy, self-seeking and irredeemable"; they have great faith in the possibilities of civic education and, as previously mentioned, of political reform. "Relevance" appears as a specific requirement only in the discussion of postbehavioralism.

The book's opening chapter briefly describes the institutionalistic and radical democratic traditions and then, at somewhat greater length, delineates the beliefs common to the third tradition. Chapter 2 traces the emergence of a professional U.S. political science and the concomitant appearance of the third tradition in the persons of Lester Ward and Woodrow Wilson. Chapters 3-6 carry the story down to the present: chapter 3 focuses on Bentley and Beard, chapter 4 on Merriam and Lasswell, chapter 5 on V. O. Key and David Truman, and chapter 6 on Lowi and Burnham. The treatment is essentially the same in each case: the ideas of each of the aforementioned individuals are analyzed to show how they can be fitted, if occasionally in Procrustean fashion, into the third-tradition mold. What we have here, then, are eight intellectual biographies, running some 10-20 pages in length, interestingly written, and providing a useful, if perhaps sometimes arcane, reading and assessment of eight of our leading practitioners, past and present.

The key question, of course, is whether the authors make a convincing case for their thesis. There are, I must say, some difficulties. As indicated above, key terms are often undefined and loosely used. Not only are the first and second traditions treated in sketchy fashion but, more troublesome, the authors fail to identify a single political scientist associated with either of these traditions, despite their claim that "modern political science has been understood as a simple extension of one or the other of them" (p. 3). This omission makes any meaningful comparison among the three traditions extremely difficult.

That difficulty is further compounded by the professional and political commitments that, the authors contend, uniquely characterize the third tradition—namely, aspirations toward a more scientific politics; a positive attitude toward the state; and faith in human nature, education, and reform. These (and possibly

even "relevance") have been the values, I would suggest, of many, if not most, U.S. political scientists rather than of any select "subfield within a minority" (p. 191).

In short, I remain dubious that the eight political scientists singled out in the volume really constitute an intellectually homogeneous set of "disenchanted realists," or that they represent a truly separate third tradition in either the practice of U.S. political science or the history of U.S. political thought. Try as I might, I cannot escape the sense that there is an essentially contrived quality to the thesis. In fact, the authors themselves tell of the slow metamorphosis of a "semester-long reading project among three Cornell graduate students" into "a longish paper, a short monograph, and finally into three or four versions of the present work" (p. xix).

Chapter 7, "Conclusion: The End of the Third Tradition," advances two propositions. The first is that liberalism generally, and especially that variant exemplified by the third tradition, is hopelessly incapable either of correctly diagnosing, or of proposing useful solutions for, the ills of contemporary U.S. democratic capitalism. The liberals cannot do so because, given their inherent ideological limitations, they are unable to comprehend the "political proclivities of American capitalism," the gravity of the problems with which we must deal, or the ineluctable deficiencies of our system of governance.

The second proposition follows from the first. Since liberalism has come to the end of the road, U.S. political scientists *must* move to the right or to the left if they are to have any voice in the reconstruction of U.S. society. If and only if they do so can they hope to play a significant role in the debate between the "advocates of democratic socialism and state conservatism" that "may finally form the contours of future American politics" (p. 240).

Must we finally abjure "centrist liberalism"? Is there nowhere to go but to the statist-conservative Right or to the democratic-socialist Left? I do not know. But I do recall hearing, many years ago, the shattering news that God was dead and, in a probably unrelated communiqué, that we had come to the "end of ideology." Both pronouncements were, I think it fair to say, somewhat premature. So, too, it may be, with the alleged demise of liberalism.

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In their Acknowledgment, the authors give special thanks to Theodore Lowi without whose "charm, wit, tolerance, critical abilities and sheer enthusiasm . . . this book would never have been written." As indicated earlier, Professor Lowi has also contributed a Foreword in which, *inter alia*, he endorses the notion of a third tradition. He contends that "whether by coincidence, common causation or plan, modern social science and modern government have common origins in liberal thought" (p. xxix); and, coming to the immediate point, describes *Disenchanted Realists* as a "splendid and provocative book."

Although *splendid* is not a term that would have occurred to me, I do agree that the book is certainly provocative, both in its strengths and its weaknesses. The entire volume should be read by those concerned with the history of our profession; the final two chapters by those who, if less interested in where we have been, are curious as to where we now are—or might be going.

ALBERT SOMIT

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Citizen Demand-Making in the Urban Context. By Elaine B. Sharp (University: University of Alabama Press, 1986. xii, 211p. \$25.95).

Citizen contacts with municipal governments have both intrigued and puzzled political scientists in recent years. These contacts are intriguing as an apparently increasingly common form of political participation, but they are puzzling because they seldom follow the patterns observed on other forms of participation. Socioeconomic status, for example, cannot be counted on to have the same positive effect on contacting propensity that it has on other participation.

Prominent among those who have tried to solve the puzzle is Elaine Sharp of the University of Kansas. Now, as the culmination of a lengthy NSF-supported project, she has produced the best analysis to date of the causes and consequences of these contacts—or demands, as she terms them.

This is a carefully crafted, meticulous piece of research. One by one, Sharp analyzes each question about these contacts, translates the

question to operational terms, and presents and interprets original data she collected to answer the question. The manner of presentation is almost always clear and well written, thereby making complex arguments understandable.

Sharp is at her best when addressing the causes of these demands. She assesses the role of many possible causes, ranging from the standard variables of socioeconomic status, need, and awareness to the usually overlooked influences of centralized complaint units and neighborhood organizations. In the process, she suggests resolutions to many previously conflicting conclusions about the relative roles of these factors.

Perhaps the most interesting question Sharp attempts to answer is why political science should be concerned with these contacts at all. It could be argued, after all, that contacts focused on mostly minor service-delivery problems are politically trivial. In two concluding chapters, Sharp attempts to show why that is not the case.

She suggests, first, that these contacts can influence whether citizens stay in the city or move away. Evidence presented in the book indicates that dissatisfaction with the municipal response to contacts does significantly, if modestly, increase citizen inclinations to move out of the city.

Sharp is somewhat less persuasive in arguing, second, that these contacts may be important in a negative sense as a component of the supposed "demand overload" on urban governments. This speculation raises a number of interesting empirical questions. Is the increase in these contacts sufficient to constitute a dramatically increased demand? Has the increase translated to an increased demand on municipal resources? These questions are ignored in the book, however, in favor of theoretical speculation founded on a directed literature review. Although interesting, this speculation is less convincing than the book's data-based arguments.

That is a minor problem, however. The only major problem for the book may be finding an audience. Written in the manner of a sequence of journal articles, with each step in the research carefully explained and documented, the book represents a significant research contribution but not a contribution accessible to a broad audience, even within political science.

This is not the sort of book, for example, likely to be used in an undergraduate class, even as a supplementary text for advanced undergraduates. The book is important reading, on the other hand, for any faculty or graduate students who want to understand how citizens interact with local government.

JOHN CLAYTON THOMAS

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Back of the Yards: The Making of a Local Democracy. By Robert A. Slayton (Chicago: University of Chicago Press, 1986. xiv, 277p. \$22.00).

This book treats a largely immigrant, early industrial, working-class community that emerged in one of the world's most unpromising settings—the Back of the Yards. In this area, the workers in Chicago's meatpacking industry lived and worked from approximately the end of the Civil War until the 1950s when the industry began to move out of Chicago, culminating in the official closing of the Stockyards in the early 1970s. Meatpacking as a large-scale industry was one of the earliest and largest fully to rationalize the factory system. And within the industry, the Chicago plants were the earliest and largest to do so. By 1900, if not before, Armour and Company could boast, truthfully, that they used every part of the hog except the squeal.

But the minute division of labor that makes such rationalization possible (and profitable) extracted a high toll among those who labored there, particularly so where the labor force was relatively defenseless to begin with, as this one was. From Marx on, sociologists of quite different theoretical persuasions have documented the social pathologies attendant upon early, rapid, large-scale, industrialization. Of all these ills, the one that lies at the crux is social disorganization and anomie behavior, or, in the author's words, "the concept of community lost."

It is this theoretical convergence of both the Marxists and the non-Marxists and their agreement that the loss of community was and is the defining characteristic of a neighborhood like the Back of the Yards that supplies the author with his leitmotiv. He disagrees, and sets out to show "that communities did exist in urban

industrial immigrant districts, that bonds between people did form, and that institutions did persevere," "that residents of these inner city areas did form *communities* even by a rustic definition of that term" (emphasis original) (p. 8).

The book is, of course, a case study and the author is conventionally modest about the typicality of the Back of the Yards and consequently about the generalizability of his findings. He is too modest. The point to be asked about any empirical study is not, primarily, how the evidence was generated but rather whether the findings are theoretically interesting and relevant. At this level, there is no essential difference between a case study and any other mode of inquiry. And there can be little doubt that this study satisfies on that score. There are other advantages to the choice of this site as well. Many of the changes in which the author was interested have occurred within living memory and, accordingly, the text is enriched by a number of lengthy interviews (79, by my count). Because of the location and the time, there is a wealth of publicly available data that studies of early industrialization in other parts of the world have often had to do without. Finally, the Back of the Yards has been the object of scholarly scrutiny for a long time; most notably by the social scientists of the nearby (geographically if not socially) University of Chicago, who, as early as the turn of the century, were already treating the neighborhood as their own private laboratory.

The author makes his case that a community—or rather communities—in the full sociological sense of the concept of *community*, did emerge in these unlikely environs—and he makes it persuasively. Very early on, the immigrant workers recreated as best they could the social conditions and institutions they had left behind in Europe (the book concentrates on the middle period from roughly 1880 to 1920 when the immigration was overwhelmingly East European). Primarily these were based on church and family and therefore faithfully recreated as well the ethnic balkanization of Eastern Europe. Shared language and belief and very often even shared villages or districts of origin in the homeland facilitated the development of a sense of community in a strange and hostile environment. In addition, a network of other forms of association such as

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sport clubs or social clubs soon sprang into being. The churches were fertile sources of such activity. The author at one point counted 18 different associations with no direct religious purpose sponsored by one church. Neighborhood taverns and shops provided daily foci for socializing and socialization. In short, a rich mosaic of associational life that provided the social and psychological support for the people to assert their autonomy and establish control over crucial aspects of their lives soon emerged. Their struggles were not, of course, uniformly successful. In particular, the attempt to establish some degree of autonomy in a central aspect—the workplace—through unionization was, on balance, a failure until the late 1930s, in part because the packers manipulated the very ethnic divisions that in other ways served to facilitate the development of community.

There is, however, a parallel thesis to the one concerning the establishment of community. That is that the creation of community simultaneously involved the creation of democracy. Indeed the book is subtitled, *The Making of a Local Democracy*. And here caution is advised. For one thing, at the conceptual level community and democracy are by no means the same thing. Some—Burke, for example—have even seen them as antithetical. Yet the author at times confounds the two. We can agree with the author that the Back of the Yard-ers sought, and in no small measure achieved, stability, predictability, and autonomy in the sense of freedom from external interference in many areas of their daily lives. But nomic, as opposed to anomic, behavior or autonomy, as opposed to dependency, can be realized quite apart from democracy. It must be shown, not assumed, that the creation of community entails the creation of democracy as well.

That demonstration is particularly necessary here. The two major institutions responsible for the development of community—church and family—are late nineteenth century, East European, and working-class. Rightly or wrongly, such structures have most often been characterized as strongly authoritarian and fundamentally antidemocratic. Second, two of the major external actors the author cites as helping to foster democracy, the unions and, somewhat later, the local Democratic party machine were not and are not internally demo-

cratic. The author says that the union, for example, extended democracy, "by organizing industrially, by company, rather than by craft." But how, one asks, does this extend local democracy? And the local political machine may have imbued the residents with the sense that "downtown" can be manipulated, but clearly it did so in the framework of a patron-client relationship rather than democracy.

Finally, one must remember that all of this takes place within a larger picture. And that larger picture is the overwhelming dependence of the community on the meatpacking industry with all the misery and degradation that entailed together with the overall failure of the workers to establish order and autonomy in this central aspect of life; failure, that is, until 1939, nearly 80 years after the community was founded and less than 20 years before the industry started to disappear from Chicago. Summing up, the book more than amply shows the emergence of a community in a setting where one would think it unlikely. It does not successfully demonstrate the emergence of local democracy in an equally unlikely setting, in part because the author assumes what needs to be shown—that community and democracy go together.

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Public Opinion and Collective Action: The Boston School Desegregation Conflict. By D. Garth Taylor (Chicago: University of Chicago Press, 1986. xiii, 241. \$29.00).

Reading this book brought back a lot of memories, many of them unpleasant: violence in the streets and schools of Boston; tawdry attempts by Louise Day Hicks, "Pixie" Paladino and John Kerrigan to exploit the desegregation controversy to advance their political agendas and careers; timid leadership by Mayor Kevin White, concerned about his own political skin; ineffective leadership by Cardinal Medeiros as he attempted to rally his flock, in this most Catholic of the United States' great cities, behind peaceful support of desegregation. Garth Taylor, I should hasten to add, does not attempt to exploit the sensational aspects of the Boston school-desegrega-

tion controversy. This book is a careful and skillful piece of work, written in the straightforward style suitable for reporting the results of social-science research. For anyone interested in how a city, once the cradle of liberty, nearly became its coffin, this book is most instructive.

Taylor begins with a review of racial ideology and civil-rights progress in the United States. By the time of *Brown v. Board of Education*, he concludes, U.S. citizens were prepared to jettison racially based doctrines of inferiority, social inequality, and mandatory segregation. Over the next 10 years, Taylor argues, there was widespread acceptance of the right of black students to attend desegregated public schools. In this process, the focus of debate shifted from whether mandatory segregation was wrong to whether mandatory desegregation was right. People were prepared to abandon the former but not necessarily to accept the latter. Orientations toward racial injustice came to be governed by what Taylor calls the "Doctrine of Voluntary Compliance," a belief that the wrongs of mandatory segregation are remedied best by voluntary desegregation. Taylor traces the roots of this doctrine back to Reconstruction and demonstrates how it can still operate as a powerful deterrent to mandatory desegregation.

The analysis is based mainly on five waves of survey data from a panel of 631 white adult residents of six neighborhoods in the city of Boston. The interviews were conducted over a period of approximately two years, beginning several months before Judge Arthur Garrity's order to desegregate the schools, and ending after the implementation of the first phase of the plan.

Taylor shows that opposition to busing and willingness to protest against mandatory desegregation are not merely the product of racial prejudice and intolerance. Adherence to the doctrine of voluntary compliance provided a frame of reference for citizens to view the insistence on mandatory desegregation as contrary to U.S. principles of popular consent and thus as unfair and unjust and potentially harmful in terms of safety and educational quality. Equipped with these "rationalizations" rooted in a pervasive popular belief, citizens became prime targets for collective mobilization at the hands of antibusing leaders whose own political prosperity was linked to the stridency and

duration of public protest. In telling the story of how a city with liberal and abolitionist traditions came to symbolize bitter and protracted opposition to school desegregation, Taylor shows how all the pieces of the puzzle came together: widespread belief in the unfairness and potential harm of the desegregation orders; aggressive antibusing leadership; a strong and resourceful organizational base for protest and a belief in its efficacy; limited resources for support, enfeebling the initial efforts of those charged with enforcing compliance with Judge Garrity's orders.

Although published in 1986, this book, based on research conducted in the 1970s, belongs very much to that decade. Like much research of that time period, its main focus is on white resistance to desegregation, and Taylor does an able job of relating his work to the research of that period. Perhaps in recognition of the somewhat dated quality of the work, Taylor attempts to draw some contemporary significance from the Boston case. In the final few pages of the book he notes that the federal-court desegregation orders produced significant changes for the better in the Boston public schools, improving management and curriculum, raising average daily attendance, and boosting dramatically the proportion of high-school graduates going on to postsecondary education. A few years after the disruptions that accompanied the first phase of implementation, the situation changed dramatically. Hicks, Paladino, and Kerrigan all suffered electoral defeat, the Boston school board came under the control of blacks and moderate whites, and, in 1985, the federal district court closed the case. While Taylor (reminiscent of the poet John Donne) seems to feel that the old order of mandatory segregation is dead but that the new order of a desegregated society is yet powerless to be born, an opposite conclusion can be drawn. A new order of school desegregation has indeed been born in Boston, and its benefits are being reaped by those who chose not to flee in protest but to remain in voluntary compliance.

EVERETT F. CATALDO

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Book Reviews: American Politics

Reagan's America: Innocents at Home. By Garry Wills (Garden City, NY: Doubleday, 1987. 472p. \$19.95).

Most political scientists who study U.S. politics and virtually all who study the presidency are likely to have read at least parts of one or more of the previous books in which Garry Wills combines history, biography, literary imagination, facile prose and self-assured personal judgements in accounts of such U.S. presidents as Washington (*Cincinnatus* [Garden City, NY: Doubleday, 1984]), Jefferson (*Inventing America* [London: Athlone, 1980]), Madison (*Explaining America* [Garden City, NY: Doubleday, 1978]), Kennedy (*The Kennedy Imprisonment* [Boston: Little, Brown, 1982]) and Nixon (*Nixon Agonistes* [New York: New American Library, 1979]).

Wills's latest foray is only partly about the current chief executive. Rather, as the possessive in the title indicates, it also comments on aspects of U.S. politics, society, and history that nurtured Ronald Reagan—for example, the small-town Midwest, the ethos of the Disciples of Christ (Reagan's mother's religious denomination), life in the Depression, and the myths and mores of Hollywood films and the film colony. But the book also delves extensively into Reagan and his career, refracting both Reagan and his America through the imagery of Mark Twain (hence the variant on *Innocents Abroad* in Wills's subtitle).

Any reader of this book who has not read Wills's previous work and expects to encounter meat-and-potatoes journalism of the sort to be found, for example, in Lou Cannon's *Reagan*, will be disabused before finishing the first paragraph of *Reagan's America*. Wills pronounces that Reagan must be understood as a *synecdoche* (a figure of speech in which a whole is described by naming an instance of it, e.g., "He is a Rockefeller" for "He is a millionaire"). Indeed, the political scientist who is unfamiliar with the Willsian mode is likely to dismiss the book as hopeless before completing that paragraph with its succession of fuzzy assertions of paradox: "The geriatric 'juvenile lead,' even as President, Ronald Reagan is old and young—an actor, but with only one role. Because he acts himself, we know he is authentic. A professional, he is always the amateur. He is the great American synecdoche, not only

a part of our past but a part of our multiple pasts. That is what makes so many of the questions asked about him pointless. Is he bright, shallow, complex, simple, instinctively shrewd, plain dumb? He is all of these things and more" (p. 1).

Why then should a reviewer ignore the book-review editor's instruction not to submit a review "if, after examining the book, you conclude it is not appropriate for *APSR* treatment"? For this reader, the reason is that by the end of this sometimes maddeningly digressive book (and in spite of its reiteration of the familiar theme that Reagan is a font of rhetoric, with little if any impulse to confront his rhetoric with evidence), I had a visceral sense of the character and roots of the fortieth president that I heretofore lacked—a sense of Reagan as a man, a sense not easily specified but that goes beyond what I learned from Cannon's scrupulously detailed biography.

Some of this sense has a cognitive basis. Wills has unearthed new information about Reagan's hometown. He interviewed people who knew Reagan in his early years, mined the accounts of him in 1940s film magazines and Hollywood gossip columns, and reassessed his participation in the Screen Actor's Guild and the biographical record of this film-actor-turned-politician.

Much of what led me to continue reading, however, was the book's imagery and its author's undocumented interpretive flights. In the mode of a novelist or essayist, Wills stretches the reader's imagination. Although he would deride such social science usages as "heuristic," the fertility of Wills's conceits, along with the archness of his prose, provides this distinctly non political science-like analyst with his political-science following.

Moreover, some readers may have reason to believe that even when Wills's assertions are unsupported, they may be on the mark. Long ago I read and dismissed as clever fantasy Wills's account in *Nixon Agonistes* of a tough-minded, politically shrewd Dwight Eisenhower. Later I found myself opening the newly declassified personal papers in archives of the Eisenhower Library. What Wills put forward as unquestioned truth on the basis of no evidence was on the mark and he had been spared my own molelike burrowings through mountains of documents.

This book is vintage Wills. It is not as care-

fully assembled and closely presented as some of his earlier work. It is hard to say much new about Ronald Reagan at this point, and Wills's two purposes of discussing Reagan and discussing Reagan's America get tangled with one another. Still it is a safe prediction that just as many political scientists have found Wills's earlier books of interest, many will read or dip into this one. They will not find political

science but are likely to emerge with a more nuanced perspective on a president who needs to be deciphered in any effort to take stock of the continuities and variations in presidential leadership.

FRED I. GREENSTEIN

Princeton University

COMPARATIVE POLITICS

Capitalism and Public Policy in the UK. By Tom Burden and Mike Campbell (London: Croom Helm, 1985. vii, 269p. \$29.00).

In the authors' own words, this book "aims to provide an analysis of the nature, development and impact of public policy in the UK" (p. vii), and it attempts to do so from within an explicitly Marxist framework. Ambitiously, nine topics are covered, ranging from the more or less expected economic policy and "health and welfare" through the less studied "cultural policy" and "law and order," concluding with a look at "foreign and defence policy." Each of these chapters is clearly organized, beginning with a section outlining the relevant theoretical concerns, after which the course of British public policy in that area is outlined. *Policy* is generally taken to mean "legislation," with special reference to the post-World War II period. Finally, the chapters conclude with a brief section that attempts to link the pattern of acts back to the Marxian model. In very general terms, the argument presented is a familiar one: the state pursues policies (passes laws) that are in the interests of the capitalist class, and particularly the financial (city) interests of that class. The succession of acts, then, can be understood as a means by which such processes as "accumulation" and "legitimation" are made more secure for capital.

Despite the fact that there is little (if anything) that is new in this argument, such an approach does come as an antidote to the pluralist conceptions of the state, which often view the state as a neutral arbiter of interest-group demands. At the very least, a Marxist

approach gives pause for thought to the pluralist "state-as-referee" idea.

Valuable though this insight may be, however, its worth hinges on an ability to demonstrate evidence for the argument, a point especially critical when there are important problems of falsifiability, as is often the case with Marxist approaches to public policy. Unfortunately, Burden and Campbell, while able to present a redescription of many pieces of legislation in terms of their capitalist interest, do not tackle, but only court, these problems. The "capitalist interest" seems to cover many eventualities and aspects, and can be defined and redefined in such ways as to ensure that every government act, without exception, was in the interests of capital in at least one sense: if not accumulation, then legitimization; if not in the short term, then the long term; if not for capital as a whole, then for some "fractions" of capital. The latter device does open up some interesting room for discussion. After all, why should successive British governments be so slavishly devoted to financial interests at the expense of (job- and vote-providing) business interests? Rather than being a starting point for analysis, however, such questions are an end point: they are not pursued and are rarely in any form amenable to hypothesis testing.

At some points the authors do present opinions in what may be termed *testable* form. For example, "The worse the condition for profitable accumulation, the greater the 'needs' of capital, and the more coherent their articulation, then the less the strength of labour and the less is the need to buy consent" (p. 73).

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Sadly, these all too rare moments do not result in any attempts to test out the hypotheses. Here the book's content does not follow its systematic form, and in general there is no direct demonstration of the links between policy outcomes and capital, only the repeated assertion that such and such a policy is consistent with some flexible definition of the capitalist interest. More detailed and careful studies of the relationship between business and policy, such as those conducted by Moran, Grant, or Marsh tend to reveal that the situation is usually much more complicated and uncertain. Often there is little evidence to support the view that the strings of politicians are being directly pulled by businessmen.

All in all, readers will find very little here of immediate use in studying British public policy; more praiseworthy and useful studies do exist in which at least some of the same conclusions may be reached with a much more parsimonious theoretical underpinning. Having said that, however, the emphasis on the importance of capitalist interests and of a state structure that is not politically anemic are important points. At the very least, they demand some kind of response from those students of policy who rely on such diverse analytical foundations as electoral incentives, the importance of trade unions or the impact of national styles. Despite the fact that many political scientists would agree with Lindblom's assertion that there is a "privileged position" for business, all too few seem to wish to see how this works out in practice and in relation to other, competing, explanations.

SHAUN BOWLER

Washington University

National Politics and Community in Canada.

Edited by R. Kenneth Carty and W. Peter Ward (Vancouver: University of British Columbia Press, 1986. 200p. \$14.00).

Six historians and four political scientists combine in this collection of essays to argue for a reinterpretation of the evolution of Canadian politics and the Canadian political community—or, perhaps more precisely, to call for a return to an older tradition. Two themes dominate. The first is substantive. Canadian scholars, the editors argue, have

been too preoccupied with "the part and not the whole" (p. 1)—with the weakness of the national political community and its institutions, with regional and linguistic cleavages, and with "province building" and the decentralized character of Canadian federalism.

Such preoccupations, they argue, underestimate the "primordial" strength of the national political community and the role of institutions in fashioning and maintaining it out of "the pieces of Britain's North American Empire" (p. 1).

The methodological theme, therefore, is "state centered," emphasizing the role of political parties and their leaders in using the resources of the state, primarily those of patronage, to build networks of interest and loyalty: "The national community was explicitly political in early Canada, where party constituted the principal tool of pragmatic nation-builders" (p. 7).

This nation- and state-centered approach is indeed an important corrective to recent trends of interpretation in Canada. Several of the authors elaborate it persuasively—Gordon Stewart with an excellent analysis of the foundations of elite culture and style in pre-Confederation Canada; the editors in their analysis of the changing definitions of citizenship through manipulation of electoral laws; P. B. Waite in a lively description of the role of spoils and jobs in reconciling the reluctant entrants Nova Scotia and New Brunswick to Confederation; Margaret Prang in an insightful analysis of the development of a national "public culture" through the emergence of nationwide business and religious elites; David Smith on the nation building and integrative strategies of the Liberal party in alliance with the national bureaucracy; and John Courtney on the role of national party conventions.

But the nation-centered thesis turns out to be hard to sustain and is indeed contradicted in many essays. As Stewart concludes, Sir John A. Macdonald's "calculating, comprehensive and thoroughly partisan" use of patronage to strengthen the national government ultimately failed because it did not foresee that "the new provinces could act as counterweights to the seemingly limitless power of the party that controlled the Commons" and because Canada "was too extensive geographically, too diverse ethnically, too fragmented economically, and . . . had no national ruling class to sustain

Macdonald's vision of central power" (pp. 42-43). This theme recurs throughout the essays. Thus, David Smith traces a sharp decline in the integrative capacity of political parties in the face of province building, the rise of policy concerns in provincial jurisdiction, and the importance of intergovernmental relations. Christopher Armstrong elaborates on the last point with an analysis of the growth of federal-provincial conferences as regional brokerage mechanisms displacing parties. Donald Blake shows how sharply divided national and provincial party systems are in British Columbia, while calling for a more aggressive assertion of federal power into the provincial-policy arena. John English shows that one integrative mechanism—the role of "Quebec lieutenant" for an English-speaking prime minister—has rarely been successful. Prang concludes that the importance of provincial governments, increased ethnic diversity, and increasing Canadian linkages outside the country have eroded the networks that earlier contributed to the "nationalizing of sentiment." Cartt and Ward themselves conclude their analysis of franchise laws by noting the "conflicts about the essence of Canadianness that lie at the heart of the political system" and the "lack of agreement on what constitutes a Canadian" (p. 77).

Thus, the book may be read less as a celebration of the success of nation building and more as a commentary on its relative weakness. Perhaps the key is that state building on the basis of the efforts of pragmatic, vote-maximizing, elitist politicians deploying the patronage instruments of the modern state is insufficient. It requires other qualities as well—and more attention than these authors pay to underlying social, economic, and ideological forces. Nevertheless, collectively, this is a stimulating, well-written, and useful book.

RICHARD SIMEON

Queens College

Corporatism and Political Theory. By Alan Cawson (Oxford: Basil Blackwell, 1986. viii, 174p. \$45.00).

Twelve years after the publication of Schmitter's article on the "century of corporatism," his approach seems to have become

something like the "orthodox" conceptualization of *corporatism*. As a synthesis of the recent debate, Cawson's book is, before all, a systematic exposition of this orthodoxy and a defense against the counterattack of the "pluralists." Cawson points out that *corporatism* does not necessarily supplant the pluralist "paradigm" but rather supplements it. This involves a restrictive definition of *pluralism* as an ideal type with a limited scope of applicability.

The other challenge to the emerging "orthodoxy" of corporatist theory is, of course, the structuralist version of neo-Marxism. Here the central issue is the concept of the state. Cawson places the corporatist concept of the state clearly into the Weberian tradition. In this perspective, in fields of welfare-state policies where the power base of the bureaucracy proves not sufficiently effective, state agencies enter into exchange relationships with "organizations representing monopolistic functional interests."

Compared to earlier writings of the corporatist school, the major shift in emphasis in Cawson's synthesis emerges in his distinction of three levels—macro-, meso-, and micro—of corporatism, according to the "scope of interest organizations" involved. Macrocorporatism intermediates interests aggregated on a class basis in peak organizations, and the level of state organizations involved is the central government of the nation-state, which has broad economic management as its "policy focus." Under this rubric, Cawson reports some of the cross-national studies of the impact of corporatism on policy outcomes and discusses Austria as an "ideal type." He presumes "that macro-corporatism is characteristic only of a few small countries, and that it has flourished especially in the context of economic growth in the post-war period" (p. 104). This, however, does not warrant the conclusion that the concept of corporatism is only of limited interest. But the specific category of *macrocorporatism*, if I understand the author's intentions, is of little use as an analytical category.

For Cawson, the potential is greater for *meso-* and *microcorporatism*. The latter he defines as "collaborative policy-making between a single firm and a state agency" (p. 76), distinguished from "agency capture" through industrial interests by a greater degree

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of "autonomy" of the agency (p. 121). The subsumption of such collaboration under the concept of *corporatism* is justified by the argument that large firms "aggregate" interests as well as organizations do. But how can this be reconciled with Cawson's definition of *corporatism* in terms of "organizations representing monopolistic functional interests" (p. 38). And whether there is anything on theoretical grounds distinctive about this subcategory remains obscure.

Obviously, the author's main emphasis is on *mesocorporatism* where, as he points out, organizations represent "sectoral as opposed to class interests" (p. 72). Cawson does not single out any countries as being specifically "mesocorporatist"—although he indicates that Britain has meso-, but only weak macro-corporatism (p. 136). The organizations at the level of sector—"be it industrial sector, public sector, consumption sector or whatever" (p. 73)—have restricted domains. Again, the author's ideas about what is theoretically distinctive in mesocorporatism are somewhat elusive. The "levels" of corporatism appear to be characterized by the predominance of specific policies. What is discussed in relation to the mesolevel is social and welfare policy on the one hand (p. 115) and industrial policy on the other (p. 113).

The latter is said to be "sectoral by nature" and to arise "when macro-economic instruments . . . fail to discriminate sufficiently between the different needs of various branches of industry" (p. 113). Thus, we see here the emergence of industrial restructuring as "negotiated sectoral policy" (p. 114). An important point seems to be that, whereas class-based macrocorporatism tends to be based upon "growth coalitions" depending on favorable economic circumstances, mesocorporatism is the form of "state intervention in industrial restructuring under crisis conditions" (p. 87).

The author thus seems to suggest that "macrolevel" corporatism is, first, a "fair-weather phenomenon" and, second, operates mainly through macroeconomic policy, whereas mesocorporatism is a crisis phenomenon and emphasizes sectoral industrial restructuring. Now it is certainly true that in some of the earlier contributions to the debate, corporatism was indeed discussed on the national (macro-) level and mainly associated

to macroeconomic policies, in particular to incomes policy. (This was also the earlier perspective of this reviewer.) However, since then we have learned that in countries like Austria and Sweden, industrial policy and labor-market policy were central issues in the process of nationwide (and "class-based") "concertation" of peak organizations (and this already under conditions of economic growth). Hence, *macrocorporatism* should not be equated with negotiated "macroeconomic" policy. On the other hand, it is difficult to see that sectoral industrial policy is something specific to crisis. After all, the sectoral Economic Development Councils ("Little Neddies") in the UK had been established already in the sixties. The author wants to analyze "the impact of economic changes on corporatism" and to demonstrate that "what is involved is a shift in the level of corporatist practice, rather than an increase or decrease of corporatism as a whole" (p. 125). This may be an interesting hypothesis and a plausible one, at least as far as Britain is concerned. But it would be more convincing if the alleged correspondence were demonstrated with the aid of measurement (of "economic changes" or "shifts in level") and tested for cross-national validity.

GERHARD LEHMBRUCH

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The Faces of Justice and State Authority: A Comparative Approach to the Legal Process. By Mirjan R. Damaska (New Haven: Yale University Press, 1986. xl, 247p. \$26.00).

Legal systems are often classified under rubrics like *Anglo-American* and *Continental* or *adversarial* and *inquisitorial*. Mirjan R. Damaska, a professor of comparative law at Yale Law School, reveals the inadequacy of such labels for cross-cultural study of the legal process. He offers instead an analytic framework that not only illuminates the world's diverse judicial systems but, more important for political scientists, demonstrates the impact of political ideas and institutions on legal structures and practices.

Damaska examines the administration of justice in terms of both the structure and the function of government. His structural focus

produces two competing "ideals of officialdom." The *hierarchical* ideal, similar to the classic model of bureaucracy, reflects the centralized authority of Europe; whereas the *coordinate* ideal resembles the decentralized authority of the United States. These models help shape a nation's judicial process. In hierarchical regimes, the legal system entails specialized stages. Information is collected gradually and elaborate files are developed as cases ascend the procedural ladder. Authorities control proceedings strictly, allowing little role for litigants and their attorneys. Appellate review is routine before a decision is final. By contrast, legal proceedings in coordinate states are less integrated, testimony is oral rather than written, authorities delegate considerable procedural responsibility to actors outside the organization, and review from above is limited.

Governmental functions likewise affect the administration of justice. In *reactive* states, where government seeks only to maintain a stable order in which individuals are free to manage their own affairs, the legal system mainly resolves conflicts between competing parties. *Activist* states, on the other hand, limit individual freedom in order to mold society to a particular vision of the best order. Here justice facilitates implementation of state policies. While *conflict-solving* and *policy-implementing justice*, like *coordinate* and *hierarchical authority*, sound like sophisticated names for adversarial and inquisitorial proceedings, Damaska shows that they are independent of this traditional dichotomy because they influence both kinds of proceedings.

Combining these visions of authority and justice, the author surveys past and present legal systems to illustrate "the ways in which procedural features attributable to different goals of justice interact with features rooted in the character of officialdom" (p. 181). His conceptual scheme is particularly useful for understanding recent changes in the U.S. judicial process, such as the rise of public-interest litigation. "Much as the *functions* of the contemporary American state are a mixture of activist and reactive impulses," Damaska argues, "so the actual *structure* of the state apparatus—especially the administration of justice—is a hybrid of bureaucratic and pre-bureaucratic forms" (p. 234).

Nevertheless, he understates how bureaucratic our legal system is (pp. 44–46, 231–34). In terms of decision making as well as administration, the U.S. legal process has become a large-scale, complex organization displaying many of the hallmarks of Max Weber's model of bureaucracy: shared values, beliefs, and expectations; loyalty to the organization and its goals; consensual decision making by mutually dependent and supportive work groups; routinized operating procedures; and secrecy and hostility toward outsiders. A good example of this bureaucratization is the collaboration among police, prosecutors, defense attorneys, and judges in criminal cases.

Although the book makes surprisingly little use of political-science research, given its recognition of the importance of politics in understanding law, it is nonetheless a novel exploration of the connection between justice and state authority. It places much of what we already know about the administration of justice in a fresher perspective while identifying patterns and suggesting meanings not appreciated before. Damaska's elegantly written and flawlessly organized work is therefore a valuable contribution to comparative analyses of the legal process, showing that study of the relationship between law and politics is far from exhausted.

GERARD J. FITZPATRICK

Ursinus College

Political Socialization in the Arab States.

Edited by Tawfic E. Farah and Yasumasa Kuroda (Boulder, CO: Lynne Rienner, 1987. xiv, 215p. \$25.00).

Jo-Ann Hart's valuable introduction to this collection of survey essays ends with Roberta Sigel's self-criticism that *political socialization* "is a misnomer for what we study because we study what children have learnt . . . not how they have learnt it" (p. 15). The great majority of these essays have not fallen into that trap. This leaves the reader with both an interesting and relevant source, because in the Middle East the function of political-socialization agencies is more that of system creation than of system maintenance. However, in the Middle East, system creation and political legitimacy are inherently linked to the ongoing

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effort to create a common communal association and national identity. These communal associations have taken two "modern" forms since the collapse of the Ottoman Empire, those of state and Arab nationalism. "Pan-Arabism," as much a socioeconomic rallying point for the disenfranchised under the old regimes as it was an antiimperialist and anti-Israeli nationalist movement, threatened "parochial" state nationalism under much of Egyptian president Nasser's rule but faltered from the mid-1960s onward until it was debilitated by the debacle of the 1967 war and Nasser's death in 1970. The Arab state, sometimes no more than a dynastically and or bureaucratically led collection of religious and ethnic groups, families, and tribes, survived much of the Arab system, but since 1979 it has come under a new ideological assault from Shiite Islamic fundamentalism.

Identity, personal or national, is a dynamic process and the history of a people's self-identification is the history of both their objective situation and their self-conception within that situation. The best articles in this collection attempt to identify the most relevant socializing agents in Arab society and, at the same time, place the respondents, families, schools, peers, and national institutions within the region's political context. These include two focused on Kuwait: one by Tawfiq Farah and the second coauthored by Farah and Faisal S. A. Al-Salem. These examine the issue of efficacy and succeed in decoupling the theoretical link between interpersonal trust and trust in an individual's government and political system. Trust in a system, they persuasively argue, is not a subjective or psychologically induced trait but emerges, when it does, from experience and successful interaction with the system. While traditional social units, such as the family, frequently provide the required access to the material and status benefits that the government provides in Kuwait, the result is that for male Kuwaiti citizens of all classes, the system is responsive due to the small size of the population and the wealth and the welfare-nature of the system.

This "instrumental" approach to political socialization is also found in Mark Tessler and Linda Hawkins's study of Tunisia. Tessler and Hawkins find some of Joel Migdal's earlier conclusions (*Peasants, Politics, and Revolution* [Princeton: Princeton University Press, 1974],

12, 20) an aid in understanding the oft-found contradictions in surveys regarding modernization. One is that "individuals often change some patterns and commitments, but not others" and the other is that "self-interest largely determines whether an individual accepts or rejects new cultural patterns to which he is exposed" (p. 108). The results of the Tunisian survey lead the authors to the conclusion that "If acculturative experiences [including education] are unaccompanied by increased socio-economic status, they do not lead to variations in psychological or political outlook" (pp. 117-18). However, these findings tend to differ from the results of the studies in the Gulf States in one important respect. While the respondents in Tunisia who experienced greater socioeconomic mobility changed their political attitudes in the direction of greater "participant citizenship," the surveys of the Gulf States found that the family and religion—and not the state—remain the prime foci of loyalty even for upwardly mobile males.

This forces Ahmed Dhaher to conclude from his survey in "Culture and Politics in the Arab Gulf States" that while the material and ecological setting has been rapidly changing, the Arab mind has not "kept pace": "The students in the sample have come to expect modernity in their materialistic needs while maintaining a traditional mental outlook, apparently unaware of the contradiction" (p. 76). However, young Gulf women and a majority of respondents of both genders from Bahrain tended to be much more highly politicized in general and more closely identified with the "liberation of Palestine" than the aggregate, leading Dhaher and Maria Al-Salem (in "Women in the Gulf") to speculate that those individuals who see themselves as most repressed in their own social setting have had their consciousness raised to the point of at least verbalizing a commitment to the liberation of other Arabs.

The two surveys on Palestinian identity and political loyalty indicate that traditional social structures have the potential of serving as the vehicle for modern political mobilization and activity. Tawfiq Farah's "Learning to Support the PLO" and Rosemary Sayigh's "Sources of Palestinian Nationalism" should be read together. While very different in approach and representative sample, each tends to find the family as the major socializing agent for

Palestinian consciousness and identity among the young, rather than the PLO-run schools or the respondents' peer groups. One might theorize that due to the stateless nature of the Palestinian people, Yasir Arafat (the personification of the national expression), the family, and the "homeland" have become linked in the Palestinian psyche.

One overall criticism of this collection is that many if not most of these surveys took place at the earliest stages of, or prior to, the 1979 revolution in Iran. This event was riddled with nationalist, religious, and socioeconomic dimensions and has greatly influenced the altogether unresolved issues of identity and legitimacy throughout significant parts of the Arab world. An updated series of surveys can easily rectify this shortcoming.

STEWART REISER

Harvard University

Decolonization and the State in Kenya. By David F. Gordon. Special Studies on Africa (Boulder, CO: Westview, 1986. 266p. \$21.00).

In strictly economic terms Kenya has to date been among the most successful African countries during the first postcolonial generation. At the same time, after having survived one of the more traumatic colonial experiences among African former colonies, Kenya has been among the most open to foreign and domestic private investment of any country on the continent. The conventional wisdom among "mainstream" economists has been to see Kenya's economic progress as a direct consequence of its policies and to say to other economically struggling African countries, "Go thou and do likewise." Political economists of a more radical persuasion have seen Kenya's fate as the result of the changing character of international capitalism, disagreeing on whether this has (Colin Leys, *Underdevelopment in Kenya* [Berkeley: University of California Press, 1975]) or has not (Nicola Swainson, *Development of Corporate Capitalism in Kenya* [Berkeley: University of California Press, 1980]) produced dependency. They agreed in challenging earlier analyses emphasizing the salience of major local political developments and responses to them (e.g.,

the Mau Mau Emergency) in shaping Kenya's future course (e.g., Carl Rosberg and John Nottingham, *Myth of "Mau Mau"* [New York: Praeger, 1966]; George Bennett, *Kenya, a Political History* [London: Oxford University Press, 1963]; and George Bennett and Carl Rosberg, *Kenyatta Election* [New York: Oxford University Press, 1961]). Those centering on the conjunction of land-transfer schemes and political decolonization differ over whether local European-settler economic interests did (John Harbeson, *Nation-Building in Kenya* [Evanston, IL: Northwestern University Press, 1973]) or did not (Gary Wasserman, *Politics of Decolonization* [New York: Cambridge University Press, 1976]) dominate the transfer of power and significantly shape Kenya's future. Underlying these debates is the question of how much relative autonomy the colonial regime enjoyed and which constituencies were most effective in limiting that autonomy in their own interests.

David Gordon's *Decolonization and the State in Kenya* is a refreshing new contribution to this ongoing debate. His thorough and balanced review of the rise and demise of the Kenya colonial order facilitates, if it does not resolve, the foregoing debates. Gordon's argument is that the colonial "state," if that's what it was, was too "diverse and ambiguous [a] collection of parts" to have dominated, or been dominated by, any single interest or underlying economic trends or to have dominated the new regime after its formal demise. There is ample room, he believes, for the play and interplay of political strategy in policy formation and execution.

Gordon builds a strong case for the importance of the political that should be taken to heart by economic and structural determinists in our midst. Yet the "indigenization of control," both economic and political, does not by itself, in my view, disprove the Kenya-as-neocolony case. The problem centers on the question of "control," the many complexities of which it is beyond the scope of Gordon's book to explore fully. Neocolonialism is not incompatible with such processes, nor is it incapable of reappearing in new forms as societies appear to change. At the same time, Gordon's long historical view of the interplay of political economic developments is important. That same perspective applied to the first quarter century of Kenya's independence will

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show, I think, that neocolonialism is not forever present, a point that Kenyans have begun and will continue to demonstrate.

JOHN W. HARBESON

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Forming Economic Policy: The Case of Energy in Canada and Mexico. By Pen Hampson (New York: St. Martin's Press, 1986. x, 161p. \$27.50).

Hampson uses energy policy in Canada and Mexico to demonstrate how and when governments act in the long-term national interest, a behavior contrary to the pluralist view that elites respond to "micropolitical" pressures. According to the "statist" model, perceptions of crisis mobilize leaders to create policies meant to preserve stability and legitimacy at the risk of alienating important societal groups.

The price and availability of energy was perceived widely as a crisis in the 1970s. Canada and Mexico joined other governments in seeking policies to cope with problems of security and equity. Both faced the trade-offs between blessings and costs of expanding domestic production in response to rising world oil prices. They shared the opportunities and problems of land borders with the United States and its oil deficits. Was there "politics as usual" in these two countries? Did the state act autonomously to formulate policies to reverse the damaging effects of past concessions to micropolitical pressures?

In Mexico, the problem with oil lay in the rapid acceleration of production that was meant to capitalize on the rising world price of oil. While success in oil production and exports fostered economic growth, there were the destabilizing effects of drawing scarce capital, increasing the foreign-debt service, milking resources from other sectors, and fueling inflation. President Lopez Portillo capped PEMEX production in 1979 in order to reverse these trends. The effect of this policy was short-lived, as world oil prices fell in the 1980s.

Unlike Mexico, Canada was incapable of being a net exporter of oil. Conflict centered on eastern Canada's vulnerability to rising imported-oil prices and western Canada's frustration over being denied the producer's right

to sell at market prices. In the National Energy Program (NEP) of 1980, the Liberals ended an oil-price freeze, forcing higher gasoline prices on eastern consumers, and also imposed a wellhead production tax to capture revenues for a strapped federal government. The "made-in-Canada" price fell victim in the 1980s to the downward reversal in oil prices, making a judgment on the NEP difficult.

Acute crises resulted, the author argues, from concessions to parochial interests. In Mexico, decisions to expand production and shelter prices were meant to hold consensus among the dominant elements—labor, business, agrarian—of the revolutionary coalition. In Canada, the Liberals wanted to avoid alienating pivotal constituencies in central Canada by controlling oil and gasoline prices.

These two cases show, Hampson argues, that pluralist and statist theories are actually complementary. Parochial interests usually prevail, but under conditions of widely perceived crisis, elites rise above the pressures to secure the national interest. The author acknowledges a weakness of this approach. Retrospective analysis allows the observer to define a crisis and describe cause-and-effect circumstances.

The paradigm also does not allow the reader to independently categorize policy that is merely the constellation of private, parochial interests and policy that meets an unequivocal definition of the national interest. In the case of the NEP, some critics have argued that its publicized features of security, fairness, and efficiency were transparent as energy policy and that it was little more than a confirmation of the Liberals' view of the virtue of economic centralization. "Canadianization" of the oil industry was intended to help secure the Liberals' electoral base in central Canada. In Mexico as well, the same arguments about national salvation made in defense of Lopez Portillo's decision to cap PEMEX production could as easily be applied to the previous decision to mobilize production—the action that supposedly produced the crisis.

The statist model does not, in practice, provide a useful distinction between the objectives of the "state" as the embodiment of sovereign interests and those of the government as the resultant vector of the society's dominant interests. In Mexico, what distinguishes the preservation of the interests of the state from

the interests of the revolutionary coalition? In Canada, to what extent was the NEP the ideological vision of senior civil servants, and to what extent pure power politics?

Notwithstanding these reservations, Hampson's book is a good effort that calls attention to the need for more studies to explore the convergence of statist and pluralist interpretations.

LAUREN S. MCKINSEY

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Guyana: Politics and Development in an Emergent Socialist State. By Kemp Ronald Hope (New York: Mosaic, 1985. 136p. \$19.95, cloth; \$12.95, paper).

Trinidad and Tobago: Democracy and Development in the Caribbean. By Scott B. MacDonald (New York: Praeger, 1986. ix, 231p. \$31.95).

Revolutionary Grenada: A Study in Political Economy. By Frederic L. Pryor (New York: Praeger, 1986. xx, 395p. \$45.00).

Militarization in the Non-Hispanic Caribbean. Edited by Alma H. Young and Dion E. Phillips (Boulder, CO: Lynne Rienner, 1986. ix, 178p. \$20.00).

The case studies of Guyana, Trinidad and Tobago, and Grenada by Kemp Hope, Scott MacDonald, and Frederic Pryor, respectively, and the contributions in the volume on militarization edited by Alma Young and Dion Phillips provide a variety of assessments on the nature of the problems faced by the non-Hispanic Caribbean since the attainment of independence and how different governments have responded to them. In one form or another, the four books focus upon the forces, both external and internal, that affect the chances for countries in the region to preserve and maintain democratic practices while at the same time formulating and implementing effective programs of economic development.

There is a general consensus by all the authors as to a growing militarization in the region as a whole and in the individual countries in response to domestic political concerns for order, stability, and regime survival and as a result of the intensification of international Cold War contentions in the Caribbean and Central America. There is general agreement

that militarization can have a profound negative effect upon the chances for sustaining democratic practices or of achieving self-sustaining economic development.

From Hope's case studies of Guyana and Pryor's of Grenada, the adoption of "Third World socialism" appears to bring with it the certainty of economic crisis and stagnation as well as kill any chances for the preservation and maintenance of democratic political practices. Pryor does not see these outcomes as inherent in socialism per se. Rather, the explanation has to do with how this ideology has become fundamentally and aberrantly altered in its transposition to the Third World. In his fascinating and highly convincing analysis, he concludes, with reference to his own set of economic, political, and socio-cultural criteria, that, like Grenada, most Third World countries identified as Socialist would rank very low on a Socialist scale. The exceptions to these criteria are in their adoption of the vanguard party, in their international support for Socialist nations, and in their ideological rhetoric. These three characteristics, he seems to be saying, constitute the entirety of the phenomenon of Third World socialism.

In his case study of Guyana, Kemp Hope provides an example of how these three elements can easily be employed to ensure and underwrite regime survival. There, the notion of the vanguard party was used to secure the centralization and personalization of power, to underwrite the conversion of the state administration into an institution of patronage, and to justify the development of a system of coercion and control. In addition, both ideological rhetoric and a foreign policy based on international support for Socialist nations were opportunistically employed to elicit economic assistance and strategic political support from the Eastern bloc and from progressive Third World nations.

What occurred in Guyana constitutes the essence of what Pryor calls "foreign-aid socialism" characterized by a complete dependence upon foreign grants and concessionary loans. The intention of those adopting this form of socialism is to employ such loans and grants for promoting economic development and supporting efforts at structural change.

The case of Grenada highlights the problems that inhere in such a strategy. Pryor shows

how the Grenadian leadership became preoccupied with establishing their "Socialist credentials" in the international arena while lacking a fundamental and sophisticated understanding of socialism itself. This invariably leads to the failure of developmental planning, an outcome also observed in Guyana by Hope, who attributes it to ideological dogmatism in the Guyanese government's approach to development. For Pryor, however, such failure has to do with the inability of governments adopting foreign-aid socialism to devise a consistent and integrated development program. This is because of their absolute reliance upon a foreign-assistance package, the contents of which are entirely determined by what foreign donors are willing and able to provide and which constantly change over time. To this is added the problem, endemic to all Third World countries, of the severe shortage of skilled personnel with the requisite levels of training and administrative acumen to implement a program of Socialist economic development. In Grenada, the inevitable developmental failures were hidden by an effective propaganda campaign directed at the domestic population, and more successfully at the international, particularly Socialist, community.

Pryor rejects almost entirely any suggestion of foreign meddling in the affairs of Grenada as an explanation for the political and economic crises of the Peoples Revolutionary Government (PRG). By contrast, the contributors to the volume on militarization, with the exception of the Seduc-Dahlberg article on Surinam, emphasize the realities of U.S. regional hegemony and the intolerance, particularly of its present administration, for any progressive regimes in the region. Such an emphasis is also evident in Hope's case study of Guyana. He suggests that the failure of the Guyanese regime to recognize this reality has distanced it from the "new Caribbean leadership." It is this very failure that explains the economic and political crises affecting that country.

In the volume edited by Young and Phillips, Boodhoo sees the escalation of militarization during the PRG regime in Grenada as the continuation of a trend toward state violence that began in the late 1940s. The dramatic military buildup after the regime came to power had, as its initial cause, the undoubted fear of U.S. military intervention or U.S. covert support for Eric Gairy, the ousted former prime

minister. It was this fear that drove the PRG to make military agreements with the Soviet Union, Cuba, and North Korea thus driving the country much more closely into the arms of the Communist bloc.

The growing militarization in the region is related, by all the contributors to the volume, to the erosion of democratic institutions. The contributions by Danns on Guyana and Seduc-Dahlberg on Surinam both deal with the use of the military as the primary prop to increasingly unpopular regimes. In the case of Grenada, according to Boodhoo, the PRG's program of militarization and its failure to hold elections contributed to its eventual collapse. In the introductory article by Phillips and Young, in the latter's article on Belize, and in the overview article by Watson, the source of the threat to democracy is clear: the efforts by the United States to assert or reassert its hegemony in the region. To this is added the fear of mass radicalization by the current crop of Caribbean leaders in the face of an escalating economic crisis. The renewed willingness of the United States to prop up pro-U.S. regimes and to provide them with military assistance and the threat to what Watson sees as petit bourgeois domination are acting in tandem, according to him, to "Central Americanize" the Caribbean. By this he means the proclivity in that region for right-wing, U.S.-sponsored, authoritarian regimes. A variation on this theme is provided in Alma Young's contribution on Belize. There, growing militarization has been sponsored by the United States in view of what it sees as the strategic importance of the country in the politically volatile Central American region.

What are the necessary prerequisites for democracy in the non-Hispanic Caribbean? Scott B. MacDonald attempts to provide an answer by examining the "successful experiment" of democracy in Trinidad and Tobago. This he attributes to the emergence of a dominant and politically unified middle class, which provided the basis for the establishment of a system of state-directed capitalism. MacDonald sees episodes of lower-class mobilization as anathema to democracy if only because they lead to racial fragmentation and are rooted in radical ideology. He traces middle-class moderation to "acceptance of European social attitudes" and, more recently, to the importation of middle-class ideas as a result of

the military presence of the United States in the country between 1941 and 1960. MacDonald thinks that the two challenges to Trinidad's democracy are lower-class radical mobilization and the breakdown of middle-class consensus. These two combined in the early seventies to produce an authoritarian tendency that was turned back in the wake of the post-1973 oil boom that laid the framework for the strengthening and consolidation of "democratic capitalism." The question that needs to be answered is why did middle-class domination result in democratization in Trinidad and Tobago when, from the accounts of all the authors considered, it appears to be the crux of the problem of militarization, underdevelopment, and growing authoritarianism throughout the region?

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Black Politics and Urban Crisis in Britain. By Brian D. Jacobs (Cambridge: Cambridge University Press, 1986. viii, 227p. \$44.50).

The title of this book promises a comprehensive analysis of the role of Asian and Caribbean people in political action and protest in British cities. Jacobs dismisses on various grounds most extant writing on the political participation in Britain of people of Asian and Caribbean origin. He sets himself the task of mapping the pattern of political activity of, the parameters of political debate amongst, and the impact on public policy (particularly urban questions) of, this section of the British population (p. 6). His central claim is that the leaders of Asian and Caribbean communities have been slowly integrated into various forms of cooperation with local and national governments as the latter have attempted to execute policies intended to reverse the disadvantaged position of Caribbean and Asian people, particularly in Britain's urban areas.

Jacobs makes a virtue of his descriptive intentions but thereby lapses into empiricism. Uncritically, he utilizes the "race relations" problematic and abstracts "black politics" from its class context, this being legitimated post hoc by his claim that he is concerned with policy rather than theoretical issues (e.g., p. 5). It is mistaken to limit the conception of

political activity of Asian and Caribbean people to include only forms of activity within or exclusively on behalf of the Asian and Caribbean communities, although this conception follows from regarding these populations as shaped by, and only concerned with, "race."

Jacobs is therefore silent about Asian and Caribbean electoral behavior, trade union membership and participation, involvement in industrial disputes, and political-party membership and participation. An integrated analysis of these forms of political activity would have allowed him not only to realize his stated objectives but also to comprehend theoretically the process of political integration he describes. There is no reason to doubt that a process of political incorporation intended to achieve, or resulting in, the neutralization of dissent is occurring in Britain, but its scale and significance require a more thorough analysis of Caribbean and Asian political activity than Jacobs offers.

There is an additional empirical limitation. Having argued that Asian and Caribbean political organization has been localized, the only example he cites is that of Wolverhampton, where his focus is almost exclusively upon the Community Relations Council (CRC). Because the formation of CRCs led to the simultaneous creation and incorporation of Asian and Caribbean leaderships, this focus certainly sustains Jacobs's thesis. But I could accept it more easily if Jacobs had demonstrated that this organization was the primary focus for Asian and Caribbean political activity in Wolverhampton (something that could only be done by means of a more systematic and extensive survey of the political activity of Asian and Caribbean people in Wolverhampton, and if a similar pattern were revealed in other towns and cities. Case studies of single cities can be extremely useful, but in the absence of supporting evidence or of an argument and evidence that they can be considered to be typical or representative, one must hesitate over the generalization of the findings to other contexts. Elsewhere in the book, key assertions are not supported with evidence from Wolverhampton or from any other city. For example, we are told that young Caribbean and Asian people have begun to use state and private assistance to establish themselves as part of the growing petite bourgeoisie, but we are offered no evidence or source (p. 167).

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Jacobs's contextualization of his analysis is weak, particularly his historical overview of British state policy regarding immigration. He describes state practice as pragmatic (p. 22) but can only sustain this interpretation by ignoring detailed evidence to the contrary. For example, he ignores the fact that the Labour government of 1945-51 recruited non-British refugees to resolve labor shortages while simultaneously but secretly considering methods by which to prevent British subjects from entering Britain because they were "colored." He ignores the method by which a Labor government withdrew in 1968 the right of British passport holders to enter and settle in Britain if they were "colored" and the formalization of that device by a Conservative government in the Immigration Act of 1971, an act that increased by several million the number of "white" people with the right to enter and settle in Britain. There is a consistency in the actions of the British state on immigration that supports the assertion that it has systematically institutionalized racism.

These weaknesses are not offset by the positive features of the book. It usefully describes central-government initiatives and the emergence of private-company strategies to deal with urban decline and political protest during the 1980s. And the hastily added Postscript describing the political disturbances of 1985 suggests that these various initiatives have not resolved the major structural determinants of political protest and violence. It is also useful to have the recent debate about the role and effect of the Commission for Racial Equality summarized and clearly set out. But the author has not realized his objectives, and in the absence of a more thorough survey of Asian and Caribbean political activism, it is difficult to evaluate the extent and significance of the process of political integration that Jacobs describes.

ROBERT MILES

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Legal Traditions and Systems: An International Handbook. Edited by Alan N. Katz (New York: Greenwood, 1986. viii, 450p. \$55.00).

This is a large and physically impressive volume that treats a limited set of subjects for a large, implicitly worldwide sample of legal systems. It consists of an introductory essay by the editor and 18 substantive chapters (seven contributed by the editor), each dealing with the legal traditions and systems of either a single nation or a group of nations. The very brief (five-page) editor's introduction lays out the outline followed by the authors of the substantive chapters. Each opens with a discussion of the "historical development" and organization of the legal system. Each then turns to the "personnel of the law," especially the "recruitment and training of those entering the profession" and mobility within the profession and its relation to judicial selection and promotion. Finally, the outline calls for each chapter to discuss "the public's perception of the legal system and its personnel and the role played by the courts in the legal system studied" (pp. 2-4).

It is difficult to know what to make of this outline. Aside from an attempt to link the first two elements of the outline to a definition of legal tradition offered by Merryman and Clark, the editor makes no effort to develop a rationale for its use. Perhaps it is seen as an obvious or self-evident organizing tool for the production of "handbook fare." But the potential difficulties that lie in its lack of development are clearly seen in the editor's two-page discussion both of the evidentiary problems accompanying efforts to assess "the public's perceptions of the legal system and its personnel and the role played by the courts" and of the meanings attached to the "role played by the courts." At a minimum, assessment of the latter is said to involve assessment of the judicial independence and exercise of judicial review by the courts studied. The discussion that ends up being mandated by these outline topics is truly global and is inevitably muddled by use of available, selectively chosen evidence documenting the courts' "roles."

Each of the substantive chapters conforms fairly well to the editor's outline, especially given the broad and ambiguous tasks it mandates. In some cases, this leads to a very dry,

formal-legal discussion with no great depth as the chapter authors' attempts to fill in the outline fall back on the evidence of constitutional and statutory provisions in the absence of better quality evidence.

Although there is no explicit statement of the book's intended geographical coverage, the chapters included deal with the vast majority of the world's legal systems in some way or another. Nine chapters deal with the legal traditions and systems of individual nations: the Federal Republic of Germany, France, Italy, Japan, the People's Republic of China, the Soviet Union, Spain, the United Kingdom, and the United States. Four chapters treat small groups of nations that are united by some fairly closely shared historical or cultural backgrounds: the Benelux nations; Canada, Australia, and New Zealand; India, Pakistan, and Bangladesh; and Scandinavia.

The remaining chapters report on the rest of the world considered as more or less standard geographical-cultural divisions: Africa (actually black, sub-Saharan Africa), Eastern Europe, Latin America, and the Middle East. These regions contain from some (Eastern Europe) to considerable (Southeast Asia) variety in political systems, national histories, cultures, languages, religions, and so on.

The "great-powers" bias of the book's composition is striking to any student of contemporary world politics. One might easily accept the presence of separate chapters on the legal systems of China, the Soviet Union, and the United States, three of the world's most populous and powerful nations. But it is less easy to defend including separate chapters on the Federal Republic of Germany, France, Italy, and Spain while lumping India with its undemocratic neighbors, treating Brazil as merely one of six examples of Latin American legal systems, and paying no separate systematic attention to Africa's largest nation, Nigeria. The point is not that the chapters on the Western European nations are uninformative or poorly done; it is that even the larger and potentially more powerful nations in Third World continents get relatively short shrift in the design of *Legal Traditions and Systems*.

Part of the reason for this great-power bias is a short supply of adequate sources for many Third World legal and judicial systems. But that can be only part of the problem: sources

on the Indian legal system and courts are voluminous, for example. To fulfill their assignments within the apparent space limitations, authors of the regional chapters are forced to make some difficult coverage choices. Some, Harvey Feinberg in his chapter on Africa, for example, choose to pitch their discussions at a very high level, seeking broad generalizations and using concrete examples to illustrate their points. Others, Lee Epstein, Karen O'Connor, and Diana Grub in their chapter on the Middle East, for example, choose to concentrate on specific nations (Egypt and Israel, in this case) as regional examples of apparent or demonstrated relevance.

Overall, this work will be of only limited value to political scientists. Its focus on legal traditions and systems moves it away from the mainstream concerns of public law and judicial politics, along with judicial policy-making and judicial behavior. Its coverage, though broad, is not broad enough to make it a comprehensive reference tool. But it is broad enough to make much of the discussion formal and shallow. It is in discussing the roles of the courts that the handbook chapters most frequently touch closely on matters of central concern to political scientists. It is also in these discussions that the gap is greatest between what the authors say and what they can say with confidence because it is based on firm evidence.

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Politics, Economics, and Welfare Reform. By Leslie Lenkowsky (New York: University Press of America, 1986. vii, 207p. \$12.75).

This book gives an account of the effort to implement versions of negative income tax in Britain and the United States. "The story of FAP, FIS, and the tax-credit plan," we are told, "suggests that whatever the future brings in Britain and the United States will emerge from the interplay of historical, political, economic, and cultural factors in each nation" (p. 188). Dr. Lenkowsky clearly does not like to go "out on a limb." And, barring the intervention of Shirley Maclaine's extraterrestrials, the future is unlikely to let him down. Generally, this

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book is more compelling in its description than in its analysis.

This comparison of attempts to implement negative income tax is presented as an example of "surprising convergence," as a method for explaining the genesis and failure of policy proposals, and as a means of arriving at general conclusions about the two political systems. The book's contributions lie elsewhere, however. They may be found in the discussion of the idea itself as a serious alternative to the present structure of social policy.

Dr. Lenkowsky claims that "negative income tax was an idea that could appeal to liberals and to conservatives, to those who liked the 'welfare state' and to those who valued individualism" (p. 174). The most important reason for the "surprising convergence," he argues, was the central role played by experts in advancing the idea. As a general concept, it is true that negative income tax is ideologically neutral. But as soon as it is given a specific structure of tax rates and benefit rates it reflects the values of policy makers. Targeted towards poverty, negative income tax offers individually tailored help for the "truly needy" that substitutes cash for the complicated network of existing policies, without diminishing work incentives or damaging the operation of the market. These are the advantages Milton Friedman mentions, and from this perspective it is not really surprising that such a policy, consonant with a particular market-oriented vision of the welfare state, should appeal to both Richard Nixon and Edward Heath. (Perhaps more surprising is that the more self-consciously Friedmanite Margaret Thatcher and Ronald Reagan, to an extent, should have neglected the idea.) It had become a right-wing idea, associated with right-wing experts, and so appealed to right-wing governments. Actually, even as a general concept, there is an ideological content to negative income tax insofar as it is viewed as a complete solution to welfare problems, without regard for health policy or full-employment policy, for example.

Aside from his argument about negative income tax being a rational and attractive idea propagated by experts, Lenkowsky suggests that both countries share similar welfare states, even in health policy (p. 171), and that these similarities in policies and values led Britain and the United States toward the negative

income tax" (p. 175). I am not aware of any comparative work on the welfare state that would support this claim of similarity.

From this comparison of welfare states we move to the equally shaky ground of a comparison of whole political systems. British government deals badly with complex issues, due to the "often unrecognized restraints of Parliamentary government" (p. 177). Briefly, a Prime Minister, "before making substantial changes," needs a consensual manner and the cooperation of ministers, backbenchers, civil servants, the opposition, and relevant interest groups. Lenkowsky should tell this to Mrs. Thatcher.

The book is better when describing the progress of the policies. While saying that the negative income tax failed because it was too sophisticated, Lenkowsky also provides evidence that the idea had its difficulties. In his account of the work of the Senate Finance Committee, he describes how Senator Williams pointed out the "notch effects," where in some circumstances a rise in earnings would result in a loss of income as a result of the proposed policy. In addition to similar objections, in Britain there was also the problem of the low take-up rate. Those eligible for the Family Income Supplement did not always receive it. Lenkowsky apparently relies on government figures in discussing this problem. Curiously, while Peter Townsend appears in the impressive interview list at the end of the book, his *Poverty in the United Kingdom* (Berkeley: University of California Press, 1979), which comes up with significantly higher figures, is not cited. Also omitted from this book is an index.

It does have on its cover, however, very favorable comments from two participants in the policy-making described, Daniel P. Moynihan and George P. Shultz. Moynihan says that Lenkowsky "has mastered that most elusive art of comparative politics." His art has eluded this reviewer.

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Sozialdemokratische Verfassungstheorie in der Weimarer Republik. By Wolfgang Luthardt (Opladen: Westdeutscher Verlag, 1986. viii, 194p. 32 marks).

Luthardt's short book on social democratic theories of the Weimar constitution epitomizes a decade during which younger German social scientists have rediscovered and reassessed the achievements of some previously neglected German exiles from Nazism—a number of Social Democratic thinkers trained in the law, including Hugo Sinzheimer, Herman Heller, Hans Kelsen, Ernst Fraenkel, Franz L. Neumann, and Otto Kirchheimer. Many U.S. political scientists also will find a lot to think about in Luthardt's analysis and assessment of these sophisticated attempts to interrelate a sociologically realistic democratic constitutionalism with a design for radical social change based on labor and proceeding through law. The most recent turn in much contemporary discussion, moreover, means that U.S. political scientists will not be discouraged by the fact that this political theorizing has a marked juristic component. The political sources, uses, and limitations of law have become central themes in present-day inquiries into the supposed "crisis" of the welfare state, as they were in Weimar.

Luthardt states the central question confronting almost all of his protagonists as one of how Socialists, who consider a progressive dismantling of capitalism a necessary condition for the maintenance and extension of democracy, can subordinate their conduct to a legality defined by a parliamentary democratic constitution that guarantees their opponents ample chances for making political use of their massive social power. This formulation connects with the predominant theme in the self-criticism by Neumann and others after 1933, that the Social Democratic strategy was vitiated by excessive "legalism." According to Luthardt, the response towards which his theorists tended is best summarized in the conception of "collective democracy" adumbrated by Ernst Fraenkel in 1929, at the time of the last Socialist-led coalition regime and before the impact of the Great Depression.

Collective democracy is a theory of the Weimar constitution that stresses the intrinsic value of its democratic political forms as an alternative to absolutist violence, that affirms

the indispensability of compromise, and that locates the dynamics for progressive social change in the legislative and social development legitimated (and in part initiated) by the constitutional provision for parliamentary sovereignty and social rights. The three labor lawyers, especially—Sinzheimer, Neumann, and Fraenkel—elaborated the third of these points. For them the institution of a "parity" constitution, parallel to the political one, was of the highest importance. This was to be a constitution that would give new contents and new impulses to legislative politics, as witnessed by the transformation of property law through labor law (especially through the operations of collective agreements) and the transformation of administrative and judicial agencies through the incorporation of organizationally grounded social judgments and negotiations, counteracting statism and class justice.

Unlike many critics, Luthardt does not belittle the theory of collective democracy when he turns to an analysis of the "confining conditions" that obviated its designs in Weimar, especially under the pressure of economic disaster. After 1930, he points out, the Social Democratic conception was rendered ineffectual. The parliamentary legislative state was confronted by a parallel and superior legitimacy grounded in statist and plebiscitarian elements in the constitution, a legitimacy that the courts used to devise a "legality" fatally limiting the political and social relevance of participation by the labor movement. The non-socialist partners in the political and social processes of compromise abandoned the earlier common ground. The incipient social constitution effectively collapsed. When the democratic labor movement opted for "legality" between 1930 and 1933, according to Luthardt, the substance of legality no longer corresponded with the constitutional design upon which these thinkers had not unreasonably pinned their hopes. All of them recognized this (although Luthardt singles out Kirchheimer as the most astute), but none of them formulated alternatives beyond rhetorical statements of defiance or tragic acceptance of what they could not change. Luthardt's somewhat eccentric inclusion of Kelsen in the study, despite his distance from most of the elements of "collective democracy," clearly signals Luthardt's own resolu-

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tion of the question of what is to be put first if the constitutive question of democratic socialism cannot be successfully answered in a socialist way.

Luthardt does not evade the question whether the failure of the Social Democrats' project in Weimar makes their conceptions more than a deterrent object lesson. Neumann and Kirchheimer thought so themselves. But Luthardt draws on the past generation of remarkable historical scholarship to present a diagnosis of that failure that is far more nuanced than the shocked self-judgments of the defeated participants or the facile stereotypes that grow out of them. The result is a more qualified and open response to the structural and strategic thinking of the reformist Left. He thinks that Weimar may still have something to teach us. Nothing can turn the history of Weimar social-democratic speculations into a success story. But it can be productively demythologized and rendered useful for discriminating comparative study and reflection by political scientists. Luthardt's book is a very useful contribution to the latter undertaking.

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Supreme Courts and Judicial Law-Making: Constitutional Tribunals and Constitutional Review. By Edward McWhinney (Dordrecht, The Netherlands: Martinus Nijhoff, 1986. xvi; 305p. \$67.00).

Edward McWhinney, prolific writer on world judicial systems, has written another book on the role of courts in the policy-making process. Here, in the context of several national legal systems, he looks at the variables related to the role of *judicial review*, which he defines as "Court practice in applying constitutional norms to scrutinise executive and legislative actions" (p. xiii). Because of the influence of the U.S. judicial system, this is actually a study of the dissemination of the U.S. principle of judicial review into other legal systems of the world.

McWhinney attributes the rise of modern-day judicial review in large part to the prestige and influence of the United States in the post-

war world. He notes that the example provided by U.S. independence from an imperial power also made the United States a likely model for judicial review. Moreover, for former Axis nations, adopting a new constitutional system was seen as a way of embracing democracy and shortening the Allied occupation time.

Supreme courts around the world, namely, the Indian Supreme Court, the United States Supreme Court, the West German Federal Constitutional Court, the French *Conseil constitutionnel*, the Japanese Supreme Court, the Canadian Supreme Court, and, to a more limited extent, the International Court of Justice at the Hague, are the focus of analysis in this book. The underlying purpose is to show how supreme judicial bodies play out the legal-realist drama of judges "making," rather than "finding," law and to expose the "legal myth" (the opposite of the realist approach) in an international context.

The book seems to be divisible into two parts: the first part, consisting of the first four chapters, presents disparate information about the role and selection of chief judges, court organization and structure, appointment by chief executives, terms in office, removal of judges, and measures of descriptive characteristics of justices who serve on these courts. The major problem with the analysis in this beginning section is an insufficient attempt to relate this enormous array of factual information to the theme of the book; that is, to examine how these variables impact on, and are affected by, the role of the selected supreme courts in constitutional review.

Especially disappointing in dealing with this mix of countries is that there is not enough recognition of the differential effects of civil-law and common-law systems on judicial review. The author begins chapter 2 by announcing that he has identified three principles that separate these two types of legal systems, but he does not adequately discuss what would appear to be a major question to legal scholars, namely, whether judicial review appears in different guises because of these two legal traditions. After all, judicial review is based on a principle of judicial supremacy over legislative and executive actions; this is a practice certainly more familiar to common-law states. Surely it is worthy of more notice that such courts as the *Conseil constitutionnel* and the

Federal Constitutional Court, arising out of civil-law traditions, perform roles similar to courts in countries with common-law backgrounds.

The last part of the book, however, dealing with judicial interpretation of constitutional charters, makes it all worthwhile. These chapters offer an excellent discussion of courts in the context of national political and legal arrangements and present comparisons of dimensions of judicial policy-making by identifying types of national legal systems on an activism-restraint policy-making continuum. The major issue of constitutional interpretation, according to McWhinney, is the behavior of supreme courts in adopting a preference for "strict and complete legalism" as opposed to the imposition of judicial value judgments (p. 92). I agree with him that it is necessary to question whether legalism is possible or even desirable in decision making on "the great political and social issues of the day" (p. 92).

McWhinney differentiates primarily between constitutions patterned after a U.S. model, ones with a "lapidarian text, short and succinct" and constitutions with a "detailed social blueprint" (p. 89) commonly found today in Eastern European countries. The discussion from chapter 5 to the end of the book is devoted to explication of the legal-realist premise that judges play active roles in policy-making when they interpret the constitutions of their respective states on such major questions as social policy, federal arrangements, institutional (political) questions, and economic policy. This analysis is especially valuable as a means of comparing the United States with other countries of the world—something that U.S. legal scholars are not often enough given opportunities to do.

McWhinney concludes by observing that he has found that "the same problems tend to recur in all societies, and that the judicially found solutions are often remarkably similar in spite of the seeming disparateness of the constitutional charters involved" (p. 279). All in all, this book plays an important role in bringing us much closer to an understanding of this basic truth.

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Syria under Assad. Edited by Moshe Ma'oz and Avner Yaniv. (New York: St. Martin's, 1986. 273p. \$35.00).

Syrian Intervention in Lebanon. By Naomi Joy Weinberger. (New York: Oxford University Press, 1986. 367p. \$29.95).

Contemporary Syria poses a series of paradoxes to outside observers. Close relatives and comrades of the current president, Hafiz al-Asad, monopolize the most powerful offices within the state and party apparatus, yet the government's broadly populist policies have generated "support from large sectors of the Syrian population that have benefited from the regime or that share the Ba'ath concepts" (Ma'oz and Yaniv, *Syria under Assad*, 34). Efforts to expand the role of private enterprise in the country's commerce and industry produced substantial levels of economic growth during the 1970s but at the cost of rising political discontent and increasing indebtedness both to Western financial and trading concerns and to the Arab oil-producing states (pp. 44-65). The al-Asad regime has devoted greater amounts of scarce resources to the country's armed forces over the last decade or so, while allowing social-welfare programs upon which much of its popular support depends to languish (pp. 72-76). Syria's leadership has, since 1970, adopted a consistent policy of reducing the influence of the Ba'th party over such key institutions as the armed forces and state administration but legitimizes its actions in terms of promoting Ba'thi socialism and prohibits rival organizations from playing a substantial role in domestic politics (*Syria under Assad*, 29; *Syria Intervention in Lebanon*, 74-77).

These dynamics have a direct impact on Syria's foreign affairs. Ba'thi policies designed to promote socialism at home continue to prevent Syria and Turkey from establishing diplomatic relations that are anything but "markedly chilly" (*Syria under Assad*, 94-97). Domestic opposition made it impossible for the al-Asad regime to implement programs intended to stabilize Lebanon during the years from 1977 to 1981 (pp. 184-85). Rivalry between the more radical Ba'th associated with Salah Jadid and pragmatists allied to al-Asad laid the groundwork for Syria's abandoning al-Fath and creating competing Palestinian organizations such as al-Sa'iqah and the

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Palestine Liberation Front in the mid-1960s (p. 194). And shifts within the Ba'thi regime largely determined the character of relations between Damascus and Moscow over the last two decades (pp. 227, 230).

But no other event demonstrates the connection between the country's domestic politics and its foreign policy more clearly than the initial phases of Syria's intervention in the Lebanese civil war. As Naomi Weinberger shows, "Syria's military elite" faced growing sectarian and "socioeconomic" discontent in the years after 1973 (pp. 77-79). Whatever stability was present within Syrian society was increasingly a consequence of the regime's use of armed coercion to dominate such challengers as the Muslim Brotherhood (p. 81; cf. *Syria under Assad*, 31-34). Thus, when the situation in Lebanon began to deteriorate in the winter of 1975-76—and persistent disunity within the Arab world precluded effective opposition to Syrian action (*Syrian Intervention in Lebanon*, 265-68)—the al-Asad regime dispatched first the Syrian-sponsored Yarmouk brigade of the Palestine Liberation Army and then regular units of the Syrian armed forces across the border into Lebanon (pp. 180-83, 213-14, 225). In retrospect, it is not clear that this move was worth the cost: Syria was for the most part unable to bring order among Lebanon's warring militias; refugees from the fighting imposed a serious burden upon Syria's already shaky economy (p. 234); and support for the Maronite Christian Kata'ib generated open discontent among Syria's Sunni Muslim community (pp. 236-37). All of these factors prevented the al-Asad regime from achieving its underlying objective in Lebanon—that of restoring some degree of orderliness to Lebanese affairs, if not of recreating the pre-1975 status quo.

Weinberger herself rejects this line of argument, claiming instead that Syria's intervention in Lebanon was related to "multiple miscalculations" on the part of the al-Asad regime (pp. 16, 326-27, 337). These misperceptions were largely predicated upon the regime's vocal support for the Palestinians residing in Lebanon in general and for the mainstream Palestine Liberation Organization in particular (pp. 112-15 and chap. 5). In her view, defending the Palestinians from the Kata'ib and other Lebanese forces, while preventing Yasir Arafat from consolidating a tactical alliance with

Kamal Jumblat's Lebanese National Movement, could have provided Syria with a principled basis for claiming a leadership position in inter-Arab affairs (pp. 14-16, 114, 241). This perspective is congruent with those of Yair Hirschfeld, who argues that the alliance between Damascus and postrevolutionary Tehran serves as a way of enhancing Syrian strength and prestige relative to Jordan and the Arab Gulf states (*Syria under Assad*, 117-21), and Amazia Baram, who explains trends in Syrian-Iraqi relations in terms of successive ideological shifts in each country's wing of the Ba'th (pp. 128-38).

In the end, this kind of explanation remains both incomplete and unconvincing since it deals only with what Weinberger calls the "motivations" for Syrian foreign policy rather than with the conditions that enable or prevent a regime from carrying out its preferred programs. Although *Syrian Intervention in Lebanon* provides a generally lucid overview of the opening phases of Syria's involvement in the Lebanese conflict, as well as of the pitfalls into which the al-Asad regime has stumbled in the years after 1976, the volume provides no theoretical explanation for why the regime was able to act on its presumed motivations. (On pp. 196-98, Weinberger suggests that Syria completely reoriented its policy toward the conflict when President al-Asad changed his mind about backing Jumblat's forces.) In the absence of a well-developed explanation for Syrian policy that connects internal dynamics to external actions, the structural perspective adopted by Avner Yaniv in his analysis of Syrian-Israeli interaction during the period from 1948 to 1982 (*Syria under Assad*, 157-78) and by Robert Freedman in his essay on Soviet-Syrian relations in 1982-83 (pp. 224-43), as well as by Weinberger in her discussion of Israeli and Soviet-U.S. responses to the Syrian move into Lebanon (chaps. 10 and 11), appears to be the most promising means in the existing literature of unraveling what Ma'oz and Yaniv call "the Syrian puzzle."

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Policy Making in a Three Party System: Committees, Coalitions, and Parliament. By Ian Marsh. (New York: Methuen, 1986. 264p. \$65.00).

The rise of the Liberal-Social Democratic Party (SDP) alliance in the early 1980s has fueled vigorous debate among observers of the British political system about the prospect of a "hung parliament," perhaps marking the end of a very long era of two-party adversarial politics. In February 1987 the Greenwich by-election in southeast London resulted in a spectacular success for the SDP at the expense of both the other main political parties. Clearly, as Ian Marsh says in his Introduction, "A House of Commons in which no single party commands a majority of members can no longer be considered fanciful" (p. 1). But how in practice would such a parliament operate? What impact would a no-majority situation have on the way in which the processes of government and policy-making are conducted? Would the discussion of policy become more open, or would powerful political parties, forced into formal coalitions or less formal pacts, be even more inclined than they are now to hammer out their compromises privately in smoke-filled rooms? Could a hung House of Commons break free of the shackles of executive domination and carve out for itself a more significant and positive role in the ordering of policy priorities and the mounting of initiatives? Would the fragile principles of individual and collective ministerial responsibility, and the bruised conventions of civil-service neutrality—not to mention the mechanistic functioning of constitutional monarchy—survive such a change?

Inevitably, much of the discussion on this subject has been highly speculative, and some of the conclusions reached have wildly exaggerated the likely short-term impact of such a result. Hung parliaments could take many different forms. The outturn of events would depend, among other things, not only upon the party political arithmetic but also, crucially, upon how long the situation lasts. After all, the most probable consequence of a hung election result is the prospect of a rerun in a few months' time, as happened in February and October 1974; but the persistence of a hung parliament for a decade or so, and a lasting im-

provement in the alliance's capacity to translate its votes into seats (buttressed perhaps by the introduction of proportional representation), would almost certainly necessitate a major reappraisal of the way in which British government is conducted.

So far as the House of Commons itself is concerned, it may be tempting to speculate, as Ian Marsh does here, about the potential role of investigative select committees in such circumstances. Westminster's committees inevitably tend to operate on the sidelines of a government-dominated parliamentary system, being concerned with scrutinizing and informing rather than with active decision making, but they have long been seen by optimistic parliamentary reformers as holding the key to redressing a perceived imbalance of power between executive and legislature. The system was significantly changed in 1979 with the establishment of 14 departmentally related select committees empowered to investigate public policy, administration, and finance across the whole range of government responsibility. Might not the new committees, relatively tranquil islands of bipartisan inquiry in a storm-blown ocean of adversarial politics and perhaps possessing the untapped potential to acquire, as Marsh suggests, "independent powers of political initiative," find a special niche in a hung parliament as, "an essential buttress to effective government?" (p. 2).

Marsh's book has two distinct aspects. The first is the product of impressively thorough empirical research. The second is based almost entirely on wishful thinking, only tenuously reinforced by the author's research findings. In the research-based part of the exercise, the author traces the development of the present committee system. He examines with commendable thoroughness the scope and impact of committee inquiries and reports in the fields of economic policy and budgeting, and in relation to "current issues" and strategic policy-making, concentrating on the session of 1982-83. Description of his findings is clear, and there are abundant and helpful summaries in tabular form. His conclusions about the impact of committees are, for the most part, careful and well balanced, though he sometimes exaggerates the significance of committee activity, as when he claims that "there is evidence that committees have crafted a role for themselves in each phase of the budget

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process" (p. 61): that role, in so far as it exists at all, is episodic and highly marginal.

The author has digested a large amount of paper and talked to a very large number of people involved, in one way or another, in committee activity. There is a separate chapter on select committees and interest groups, based largely on the results of a wide-ranging questionnaire survey, supplemented by interviews. The author concludes that committees themselves have made extensive if patchy use of interest-group evidence but have done little actively to cultivate their "outreach" to such groups; the latter, for their part, remain uncertain about the role of committees but generally welcome their "visibility" and their "fairness." There is, the author believes, considerable undeveloped potential for enhancing the scope of interaction between committees and groups.

So far, so good, but the more general, speculative conclusions of the book require the reader to make an imaginative leap from the real world into make-believe. Marsh's working premise is derived from Samuel Beer's indictment of "pluralist stagnation" (*Britain against Itself* [New York: Norton, 1982]). He argues in his introductory chapter that the "drift and immobilism" of current policy making (he does not give a clear impression of what exactly he has in mind here) could best be remedied by integrating "recalcitrant pressure groups" more effectively and comprehensively into the policy-making process via the select-committee system.

This is a reasonable enough starting point. The trouble is that the author fails to sustain his thesis on the evidence derived from his research and has to fall back on assertion and guesswork. No doubt there is unrealized potential for committees to act more extensively as a forum for the articulation and mediation of group demands, but the recent record of committees provides no indication at all of the likelihood of this happening. Arguably, if it did happen, it *might* be a good thing (though one doubts if it would be a panacea for pluralist stagnation), but this is a matter of both opinion and prediction. And the author himself acknowledges that the views of the Conservative and Labour parties are essentially hostile to such a development. Even the alliance, notwithstanding SDP leader David Owen's general sympathy towards enlarging the role of committees, has not so far begun to

go down the road signposted in this book. And whether a hung parliament, of whatever kind, would advance the author's cause takes us even further into the realm of fantasy. It is a pity that the book becomes so wildly speculative, because it contains some interesting things.

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Liverpool on the Brink. By Michael Parkinson (Newbury, England: Policy Journals, 1986. 184p. £22.50 cloth; £9.50 paper).

On 12 March 1987, the Judicial Committee of the House of Lords ruled against 47 Liverpool city councillors and thereby disqualified them from office for five years and left them with total debts of over £500 thousand. This decision marked the end of a four-year fight between the Labour party in Liverpool and Mrs. Thatcher's administration. Michael Parkinson's book tells the story of this fascinating struggle.

The volume provides a detailed account of how the political drama unfolded up to autumn 1985 and, although the book suffers from an absence of references and also lacks an index, Parkinson ably highlights the fiscal, political, organizational, economic, and ideological factors in the potent mix that led inexorably to confrontation with the central government—and to the verge of municipal bankruptcy.

As Parkinson clearly shows, Liverpool Council was *underspending* during the period 1974-83, when no party had a majority and the Liberals held the balance of power. They underfunded services and used reserves to avoid raising the rates; this caused dire consequences after the election of the Thatcher administration, as the artificially low level of expenditure became the basis of financial cuts. "The lost decade" was characterized by confusion and a persistent failure to tackle the city's financial shortfall. The political uncertainty led to bureaucratic inertia. Inefficient council services were not reorganized, and municipal housing was not adequately repaired or replaced.

Within the Liverpool Labour party, where years of patronage and complacency had taken

their toll, the old guard was ousted during the 1970s by new-breed activists, many of whom supported the views of the newspaper, the *Militant*. Under their influence and as a result of a shift in union power, municipal socialism became Labour's central concern. Between 1979 and 1984, 40 thousand jobs disappeared and "Liverpool became known as the Bermuda Triangle of British capitalism" (p. 12). By 1985 the rate of unemployment was 27% and in these circumstances the local Labour party was determined to safeguard council jobs. The Conservative government was equally committed to reining in what it saw as profligate local-government spending.

Parkinson argues that a clash between two ideological responses to urban decline was at the heart of the conflict between Liverpool and the Thatcher government. In addition, the national leadership of the Labour party became increasingly embarrassed by the Liverpool councillors' strategy.

Liverpool on the Brink tells the story of how one council challenged the British government and won a round but lost the fight. Parkinson convincingly shows that the local Labour leadership was politically conceited and tactically inept. Nevertheless, although a combination of factors brought Liverpool to the brink, a principal cause was a blundering and obstinate central government. Although the center has control over legislation and financial grants, local authorities have resources of their own, such as the ability to raise finance, control over the implementation of policies, legitimacy arising from local elections, and access to local expertise and information. Consequently, relations between central and local government in Britain have been generally characterized by bargaining and accommodation.

From 1979 onwards, however, the position changed. Mrs. Thatcher's government arrived with scarcely concealed contempt for politics by negotiation and consensus and with a determination to impose its policies on local government. This approach predictably led to considerable opposition. Much of the resistance has been covert. Resentful local authorities manipulated their finances and procrastinated in response to central directives. In Liverpool's case, however, the opposition was stridently defiant.

According to Parkinson (p. 36), the Liverpool political and fiscal crisis was like a Greek

tragedy in which the ending was determined as the play opened. But what ending has been reached? Liverpool City Council's debts are currently estimated at £850 million. Unemployment and social deprivation are widespread. Serious public disorder may recur. And the people of Liverpool may once more vote councillors into office who, like their disqualified predecessors, are prepared to defy the British government.

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Israel at the Polls, 1981: A Study of the Knesset Elections. Edited by Howard R. Penniman and Daniel J. Elazar (Bloomington: Indiana University Press, 1986. 280p. \$35.00).

For some years the American Enterprise Institute, under the editorship of Howard Penniman, has been sponsoring election studies of selected countries, with each successive election becoming the subject of a volume of collected articles. Of these volumes, the editions on Israel have complemented another series that the political scientist Asher Arian has been editing in Israel since the 1960s. Together, Penniman, Arian, and their contributing authors have given English-language readers excellent access to a political system in which all that can happen in parliamentary politics often does happen.

One drawback of the Penniman-Elazar volume on the 1981 Israeli election is that it was published five years after the event. Major changes have taken place in Israeli politics since then, including the 1983 retirement of Menachem Begin after the Lebanon War of 1982. Events such as these are covered only in a concluding article by Daniel Elazar, but even here the concluding year is 1983. Not mentioned are the 1984 election and the creation of Israel's first government based on a grand coalition of Labor and the Likud.

The value of this book therefore lies less in its description of one election and more in its treatment of long-term political trends. Valuable too are the political-cultural insights into a changing Israel provided by well-informed academic observers.

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Among the trends discussed in depth is the consolidation undergone by the Labor and the Likud alliances, or blocs of parties, so that each is becoming a party rather than a bloc. Israel has acquired a quasi two-party system, in which the two major parties are equally capable of contesting for control of the government. The smaller parties have steadily declined as survivable entities. A case in point is the decline of the National Religious Party (NRP) from its former prominence as an important coalition partner and representative of religious opinion.

Changes in the party system are linked to trends within the political culture. There has been a blurring of the distinction between secular and religious politics in Israel as a new "civil religion" emerges blending rabbinical Judaism with the events of Israeli national existence (for more on this, see Charles S. Liebman and Eliezer Don-Yehiya, *Civil Religion in Israel* [Berkeley: University of California Press, 1983]). Generational changes within political elites and among the electorate have been rendering older ideological labels ever less meaningful despite continuation of the ideological style in Israeli political discourse. Events and elections have legitimized some leaders and factions and delegitimized others. The Israeli Arab minority has come into the mainstream of Jewish party politics rather than remain an alienated group of protest voters. Prohibitions on Israeli and U.S. interventions in each others' political systems have declined in effectiveness, and the relationship between the Israeli and U.S. Jewish communities has experienced strains.

Many of the articles in the Penniman-Elazar volume that cover these trends are valuable encapsulations of ongoing research undertaken either by the authors themselves or by others. Torgovnik and Arian, to cite just two of the contributors to this book, summarize their own research pointing to the steady long-term rise in support for the Likud, and the decline of the Labor Party's ideological distinctiveness (see Arian, *Politics in Israel* [Chatham, NJ: Chatham House, 1985]). Labor has become a party that tries to appeal to as many segments of the electorate as possible. But it is unable to secure loyalty among large numbers of younger Israelis unimpressed by the party's heroic past or among many voters from the "Second Israel" (Sephardic and Oriental Jews).

Shmuel Shandler, another contributor to this book, offers a conceptual historical treatment of the religious parties, thus continuing the sociological style of analyzing party behavior pioneered in Israel by Yonathan Shapiro (*The Formative Years of the Israeli Labor Party* [Beverly Hills: Sage, 1976]) and Horowitz and Lissak (*The Origins of the Israeli Polity* [Chicago: University of Chicago Press, 1978]). His eye-opening article delineates the process whereby ideological blurring among many parties has occurred. That process started with Labor's abandonment of its "consociational" relationship with the NRP after Ben Gurion's departure from Mapai.

So long as Mapai avoided mergers with other socialist parties and relied on the NRP for support in coalition making, the NRP elite respected the Mapai's dominance in domestic and foreign affairs. But the new Labor alignment's reduced need for partnership with the NRP set the stage for younger NRP leaders to take positions on formerly secular matters. This they would do in the wake of the 1967 war and the capture of what many Israelis now call "Judea and Samaria." Yet the NRP suffered because the emergence of the Likud bloc gave religious voters a new way to express themselves. Even regular NRP voters felt safe in turning to a Likud that posed no threat to the place of religion in Israeli life. Those same voters were now apprehensive of a secular Labor alignment no longer made cautious on religious issues by a need to preserve its ties with the NRP.

Each article in the Penniman-Elazar book shows that care was taken to place the 1981 election within historical-developmental contexts of this kind. The most impressive articles in terms of identifying linkages of subtlety and elegance are those of Elazar, Brichta, Reich, Shandler, and Arian. Elazar writes from his understanding of Israeli and Jewish culture; and Brichta offers a theoretical framework within which Israeli nomination methods can still be called oligarchical despite major efforts at reform. Arian draws from his immense longitudinal store of survey and voting data to reach conclusions explained with sarcastic wit.

Unusual perspectives on Israeli elections are provided by Samuel Krislov in describing the U.S. role and by Judith Elizur in detailing the role of the Israeli news media.

The same is true of Efraim Torgovnik who

writes of the Labor party with the knowledge of an insider and with much delicious detail about who-did-what-to-whom. Indeed, an element that could be even more emphasized in a serious study like this one is the sheer fun of watching the antics of Israeli *askan tziburim* (public persons, i.e., politicians). To see the deal making, the betrayals, the names people call each other, the public reconciliations, the decisions achieved by bluster and brinksmanship, the marriages of high principle and low machine manipulation is to capture much of the true flavor of Israeli political life. I hope more of that flavor appears in any 1984 Israel election volumes issued by these or other editors.

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Government and Politics of Singapore. Edited by Jon S.T. Quah, Chan Heng Chee, and Seah Chee Meow (Singapore: Oxford University Press, 1985. xviii, 324p. \$10.85).

This book, completed in 1980, was a project supported by the Southeast Asian Studies Program of the Ford Foundation. The eight authors, including Professors Quah, Chan, and Seah, who served as editors, are Singaporeans teaching at the National University of Singapore. Dr. Quah coordinated the project and also contributed two chapters.

The collection of 12 original essays is designed as a textbook for Singapore University students. It analyzes a country whose prime minister, shortly before independence, warned that an independent Singapore was an economic, geographic, and political absurdity. The two bibliographies, one at the end of the Introduction and one at the end of the book, are well chosen and extensive. Further detailed references are found in the endnotes to each chapter.

The Introduction sympathetically distinguishes the inside and outside researcher and alerts us to the interest of outsiders in Singapore during the 1960s and early 1970s, years of considerable foreign research on Southeast Asia. Unfortunately, an era of cooperative research between insiders and outsiders has yet to emerge. They have data and perspectives that complement each other, and

collaboration should augment our knowledge of and perspectives on Singapore and the Third World substantially.

This work is divided into three parts. Part 1 deals with political, economic, and social-cultural conditioners. Part 2, the longest, deals with the contemporary political system. Part 3 analyzes four policy areas—education, public housing, public transportation, and foreign policy. The chapters are not especially critical in their approach, but neither do they avoid pointing out political shortfalls and problems.

Part 1 offers a useful balance between detail and analysis. The British left behind a modern political infrastructure. A specious anti-colonialism is not part of Singapore's political culture. Seven years after the book was completed, a key issue, identified in chapter 1, remains high on the political agenda—the effort of the ruling People's Action party (PAP) to find a midpoint "between liberalism and benevolent paternalism." There are inevitable gaps. Operation Cold Storage in February 1963 critically wounded the principal opposition party, the Barisna Sosialis, and facilitated the PAP government's efforts to acquire viability and legitimacy. More time should have been allocated to a discussion of government debt as well as income inequality, both mentioned but not analyzed in chapter 2. What is the Singapore identity? Chapter 3's conclusion that "the levels of national identity in Singapore are very high" may be premature. National identity takes decades to evolve. Singapore is a young country. Still to be researched is how and in what ways it is nurturing popular self-identity. Overall, however, part 1 is a very useful heuristic overview.

Part 2, on contemporary politics, focuses on institutions. This is effective in relatively stable, new political systems where most political behavior funnels through existing institutions. An institutional approach frequently is the best entrée to identify and study the processes of influence and decision making as well as to understand how politics actually works. This is an excellent way to look at functions—because functions are essential in defining and describing an institution. Institutional analysis often makes it possible to focus attention immediately on what is of primary political importance. Part 2 evaluates parliament, the civil service, statutory boards, political parties, and parapolitical institutions

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such as the People's Association, Citizens Consultative Committees, and Residents Committees. Chapters on interest groups, the executive elites, and the judicial system were not included. The latter would be especially useful now, particularly in view of the wide publicity given to former Member of Parliament (MP) Jeyaratnam's legal problems.

Singapore illustrates one form of cabinet government. The cabinet dominates, and rule by administrative technocrats is unbroken since the mid-1960s. One opposition MP now sits in Parliament. Parliament attracted increasing public attention when two MPs were in Parliament. Now that Mr. Opposition, J. B. Jeyaratnam, who served from 1981 until November 1986, has served one month in jail and is excluded from Parliament for five years, parliamentary sessions will be less spirited. Parliament's policy-input functions will remain generally minimal, as they have since the 1960s. Chapter 4 notes a few issues where the government saw no easy or logical solution, and debate was bolder. A few earlier policies, which were made to seem obvious to Singaporeans and non-Singaporeans, are partially unraveling. This might encourage hard-hitting questions by more backbenchers, especially as the second-generation leaders assume increasing responsibility. The single opposition member has many valid questions to raise, though no structured alternative program has been developed by any of the 15 registered opposition parties. Perhaps, as in the British Parliament, the Singapore Parliament will remain primarily a tester and maker of persons, not of policy. The civil-service chapter provides a good overview. Future research should include recruitment, institutional identity, administrative initiative and policy formulation, and the Management Services Division.

The diminishing role of the People's Action party began in the early 1960s. It is not, though, a cultural holdover and still undertakes relevant functions like short-term electioneering. More pervasive problems likely to increase demands for change—a younger, more educated middle class desiring a more open society and inevitable periods of economic stress—may revive functions of grass-roots recruitment, two-way communication flow, and a renewed role for party mobilizers and communicators as well as technocrats. Tensions continue between the

need to marshall and coordinate human resources for purposes of economic development and an orderly society, and a growing public desire for individual expression and political pluralism.

Part 3 focuses on four issue areas of the late 1970s. Public housing is solved for all practical purposes. Public transportation alternatives have been considered, but the mass rapid-transit plan selected is on track. The foreign-policy chapter sets out the relevant foreign-policy patterns as they evolved since 1965 and provides an excellent basis for understanding foreign relations in the 1980s. The education analysis is candid in identifying and summarizing conflicts. This latter issue remains the least amenable to a permanent solution. Not included is a chapter on the economy, perhaps because it was not seen as an issue in 1980 or anticipated for the immediate future—which was wrong on both counts.

This book is a successful collection of essays and provides more data and analysis of a developing political system than can be found respecting many other Third World countries. Asianists as well as those interested in Third World political development look forward to a second edition and hope it will be available in paperback for use in upper-division college courses.

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Democracy and Pluralism in Africa. Edited by Dov Ronen (Boulder, CO: Lynne Rienner, 1986. xl, 220p. \$26.50).

Probably no scholar of African politics would question the value to participants of a small, intimate seminar on topics such as democracy and pluralism in Africa, especially one sponsored by the United States Information Agency (p. xl) with funds to include a large number of African contributors. The value of the printed proceedings of such a conference is more problematic.

This book does not address the question implicit in its title. It is not a collection of essays on the relationship between pluralism and democracy in Africa. Indeed, there is no consensus among the 18 contributors about what the two terms mean, nor does the editor seem

to have seen fit to organize discussion around any particular theoretical starting point, though he himself, in the closing chapter, defines the two terms as sociocultural heterogeneity (*pluralism*) and self-rule and self-determination (*democracy*) (pp. 194, 202). Had these two definitions constituted the theoretical starting point of the book, one might at least take pleasure in reading a serious debate. Instead, one encounters a hodgepodge of definitions and issues, most of them inadequately addressed in articles of 10 pages or less. Pathe Diagne, for example, defines *pluralism* as almost all differences among African societies, including religious, artistic, and ethical differences. Richard Sklar, in an otherwise interesting short—though perhaps prematurely optimistic—piece on the rule of law in Zimbabwe, defines *pluralism* as divided power (p. 141), perhaps more appropriately viewed as a component part of democracy. Pauline H. Baker uses the term *democracy* interchangeably with *political stability* (p. 53). The term *constitutionalism* is used rather weakly by John W. Harbeson, who appears to think that it means merely the presence of a written constitution rather than the actual subjection of government to the rule of law.

Interesting disagreements among the authors go unnoticed. John A. A. Ayoade argues that in Africa, one cannot "opt for . . . aloofness because the wielders of power invade every citizen's privacy" (p. 29), yet Ilunga Kabongo maintains that African political systems are not dictatorships but rather "inefficient polit[ies] where democracy is negatively expressed" and "the peasants always have the exit option" (p. 37). Which, one wonders, is the more accurate picture?

Two pieces address the ideologically obligatory thesis of African "tradition" and how Africa's inherent democracy, which I would prefer to call *gerontocracy*, can or cannot be translated into modern practice. David N. Magang discusses Botswana's successful democratic system but appears not to notice his own point that Botswana is in fact a homogeneous, not a pluralistic, society (p. 106). Lansine Kaba discusses power and democracy in the Songhay empire of the fifteenth to sixteenth centuries, with a brief conclusion that "a fair and long-term effective political system requires autonomous institutions" (p. 100). But the myth of African traditional democracy is not

addressed head-on, nor are the real elements of caste, slavery, and gender discrimination in precolonial societies discussed, although Colin Legum briefly notes, in a refreshingly frank overview, that "there are examples of both authoritarian and democratic systems in pre-modern times" (p. 179).

In general, the participants in the seminar that resulted in this book do not appear to have reconsidered their original contributions in the light of subsequent discussion. While some pieces, such as W. A. E. Skurnik's article on press freedom, stand well alone, the volume as a whole does not cohere. Some articles are on democracy, some are on pluralism, very few discuss the relationship of the two, and no two use the same definitions. This volume is useful as an introduction to the thinking of some prominent African(ist) scholars, but it is not what the title leads the reader to hope for.

RHODA E. HOWARD

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Voters Begin to Choose: From Closed Class to Open Elections. By Richard Rose and Ian McAllister (Beverly Hills: Sage, 1986. vii, 178p. \$40.00, cloth; \$14.95, paper).

The sense of change and uncertainty in the British party system and electoral behavior is conveyed by the title of this book. The long established two-party, two-class framework accounts for a smaller part of electoral behavior. This important study draws largely on the monthly Gallup surveys and successive British General Election surveys, including the most recent for 1983. The authors argue that the widely accepted model of British voting behavior is one in which voting is *determined* by society, in this case by social class. *Indeterminate voting* is when short-term influences are more important than enduring factors like class and party identification. Party competition can be *minimal* or it can be *multiparty*. The authors combine these characteristics of electors and parties to give four types of electoral competition. Britain has moved from the *closed-class* model, in which two parties compete for a socially determined electorate to *open competition*, that is, an indeterminate electorate and many parties.

Rose and McAllister reject any single-factor

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explanation of voting, though they make use of class, party identification, and issues to provide an umbrella. Social class has become looser, the manual working class is steadily declining in numbers and in share of the population, and the Labour party is getting a steadily smaller share of a steadily declining constituency. Each social class now splits across the three parties, with the major divisions within the working class. Labour gets just over half of the unskilled working class, and the Conservatives 42% of the skilled working class and 56% of the middle class. Whichever definition of class is used, it shows a consistently weak relationship with voting.

Party identification is also declining in importance. Between 1964 and 1983 the proportion of Conservative identifiers fell from 40% to 36% while Labour identifiers fell from 43% to 31%. The weakening of the traditional moorings of the vote, class and partisanship, has increased the scope for issues. But, in view of the general agreement on many issues across voters of all three parties, Rose and McAllister reject issues as an explanation. An explanation resting upon political principles is also rejected, in view of the lack of clear party divisions on such principles as welfare, traditional morality, and racialism. The authors find little support for the neo-Marxian explanations of cleavages of public versus private consumption and production.

If so many of our traditional approaches are found wanting, what does influence voting? Rose and McAllister provide a "class-plus" explanation. They use factor analysis to set out the contribution different variables make to voting. Not surprisingly they find that each factor makes some contribution, though it is interesting to see the relative importance of political principles. What also emerges as important are class-related or economic-interest factors such as housing and trade-union membership. Within any social class homeowners and council tenants are 20% more likely to vote Conservative or Labour respectively, compared to persons without these attributes. The most solid group of Conservative voters are the nonunion owner occupiers, 62% of whom voted Conservative in 1983. The most solid Labour voters are working-class council house tenants, 61% of whom voted Labour. But the first group is 2½ times bigger than the latter and the ratio has been worsening over

time. In 1964 the former outnumbered the latter 34% to 22%; in 1983 they outnumbered them 40% to 17%. The ratio is likely to have widened even more by the time of the next election. In many ways, the alliance is now the more "typical" party in terms of its support. Its vote is minimally influenced by social structure, and on many issues alliance voters' position lies between that of voters for the other parties. Rose and McAllister wonder if we are all alliance today?

Rose and McAllister do well to remind us that class, though declining in importance, never has had the significance in Britain that the number of commentators once attributed to it. For the twentieth century and even since 1945—and the emergence of the Labour party as a majority government for the first time—a substantial part of the working class always voted Conservative. Without that "deviant" minority Britain would not have had a competitive party system and there would not have been Conservative domination. An interesting question is, What is the Labour party, the party of the working class, to do without a majority working class?

It is interesting to compare *Voters Begin To Choose* with two other analyses of the 1983 election. *How Britain Votes* (Oxford: Pergamon, 1985) by Anthony Heath, Roger Jowell, and John Curtice is the successor to the Butler and Stokes landmark study, *Political Change in Britain* (New York: St. Martin's, 1969). It argues that social class, though declining in significance, is still important. But the authors have provided a trichotomous division of the electorate into the middle, intermediate, and working class. Their own approach to social class has resulted in a reduction of so-called manual workers into a "purer" type and, not surprisingly, this type is more inclined to vote Labour than is found with other definitions of *working class*. Even then, however, they find that only 49% of the working class voted Labour in 1949. *British Democracy at the Crossroads* (Boston: Allen & Unwin, 1985) by Patrick Dunleavy and Christopher Husbands is a highly original study that draws on many concepts and insights from U.S. social science. Their own emphasis is on a so-called "radical" approach to electoral behavior in which they stress the importance of consumption cleavages. A welcome feature of *Voters Begin to Choose* is that it is written so

clearly, avoids jargon, and also steers clear of tiresome arguments with proponents of so many other approaches.

DENNIS KAVANAGH

University of Nottingham

Political Liberalization in Brazil: Dynamics, Dilemmas, and Future Prospects. Edited by Wayne A. Selcher (Boulder, CO: Westview, 1986. x, 272p. \$26.50).

After 1974, Brazil's military regime underwent a gradual process of political liberalization. The process began with President Ernesto Geisel's controlled relaxation of authoritarian rule and continued through the transferal of the reins of power by his successor, General João Figueiredo, to the civilian winner of an indirect presidential election in March 1985. *Abertura* (opening) has yet, at this writing, to develop into a fully representative democracy. This prolonged democratization poses a special scholarly challenge. Analysts of liberalization must not only chronicle unfolding events, but they must also place them in a proper theoretical context. This collection of six essays by North American authors succeeds admirably in the former, but less well in the latter, effort. Overall, it makes a valuable contribution to a growing literature.

Three of the essays present detailed accounts of 1977-85 politics, the period in which the regime's highly controlled *distensão* (decompression) strategy gave way to pressures from below and without, resulting in an *abertura* with its own momentum. Enrique Baloyra's essay focuses on the contradiction between the military as a professional institution and as an incumbent regime. Baloyra places inordinate importance on tensions between the presidential advisory group and the military high command as the crucial determinant of both Geisel's and Figueiredo's strategies. This misplaced emphasis on internece military struggles rather than on the interplay of political, social, and economic factors may stem, in part, from the author's heavy reliance on English-language sources, especially *Latin America Report*, often regarded as a font of unsubstantiated political gossip. Baloyra contributes, nonetheless, a number of astute observations. Most intriguing is his com-

parison of Mexico and Brazil. Mexico's military leaders created a strongly associated, but distinctly civilian, party apparatus that could engage directly in politics without losing its institutional identity. The Brazilian military administrations were never as successful in using their own party, Partido Democrático Social (PDS), to fight their political battles. By entering the political realm directly, the military thus compromised its own institutional professionalism.

The essays by Wayne Selcher and David Fleischer cover much the same ground as Baloyra. Selcher gives a lucid and well-informed account of the 1979-85 period, assigning more responsibility for Figueiredo's mismanagement to the onslaught of economic recession, heightened internal social and political demands, and increasing international pressure for austerity measures to cope with the foreign debt. *Abertura* was pushed beyond a purely institutional reform to address larger social-reform issues, and Figueiredo was forced to try to balance authoritarian and democratic forces in an economic crisis within a socio-economically polarized society. Fleischer focuses more narrowly on the role of the congress and political parties. By describing the casuistry utilized by the government to assure PDS domination in successive municipal, state, and federal elections as well as the presidential succession fight, Fleischer provides a uniquely microscopic view of political procedural mechanisms.

Robert Packenham traces the changing nature of political discourse among Brazilian academics and intellectuals from 1964 to 1985, a discourse that shifted from Marxist domination in the repressive 1960s and 1970s to a more pragmatic post*abertura* liberalism. Much of the essay fixes on the transformation of the work of Fernando Henrique Cardoso, a Brazilian sociologist-turned-politician with whom Packenham has long jostled. Packenham's arguments are interesting in their exploration of the somewhat neglected topic of Brazilian intellectual history. To integrate them more completely into this volume, however, the connection between intellectuals and events occurring in the larger polity itself needed to be more clearly established.

Ronald Schneider attempts to predict Brazil's political future by calculating the changing shares of power held by various

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political actors. These calculations, based on Schneider's 30 years of study of Brazil, are purely subjective. Schneider is to be commended for his effort to measure specifically what most authors assess with broad brush, but his methodology is problematic. Not only are the 29 categories of political actors not mutually exclusive, but one cannot assume that at any given time all actors are fighting over the same issue. The political arena is multidimensional, and political actors may be interested in any number of issues with varying degrees of intensity.

By far the best contribution to this volume is Donald Share and Scott Mainwaring's comparative analysis of political liberalization in Brazil and Spain. The essay argues that both cases embody "transitions through transaction" in which the regime itself initiates the transition, establishes some limits to political changes, remains a significant electoral force during the transition, and negotiates directly with the democratic opposition. Unlike Portugal's transition through regime breakdown (1974) or Peru's transition by extrication (1980), Brazil and Spain both democratized through carefully paced, staggered reforms. The regime excluded selected opposition groups, ruled out both extensive socioeconomic change and punitive measures against former regime members, marginalized extreme groups, and sought to maintain sufficient popular support so that it could compete electorally. The analogy between Spain and Brazil is occasionally overdrawn, but the authors present a well-organized analysis and provide a useful theoretical prism through which to view liberalization.

Overall, this book offers, for the first time in English, a thorough account of political liberalization in Brazil. Several of the essays tread the same ground, however, and one wishes the editor had included an economic or sociological perspective to counter the heavy emphasis on political factors. With the exception of the Share and Mainwaring essay, which makes a more general contribution to theories of comparative politics, the articles are primarily of interest to those who follow contemporary Brazilian politics, both in and out of the scholarly community.

BARBARA NUNBERG

World Bank

Government Violence and Repression: An Agenda for Research. Edited by Michael Stohl and George A. Lopez (New York: Greenwood, 1986. viii, 278p. \$35.00).

Political scientists will not understand Third World politics until they can explain governmental violence and repression. Miles Wolpin notes, "In short, an absolute majority of the population in the developing countries survived under regimes characterized by torture, execution, disappearances, and brutal prison treatment of those suspected of opposing the government" (p. 100). How can one talk about interest articulation or aggregation where every opposition voice is smashed and made speechless? Michael Stohl and George Lopez, first in *The State as Terrorist: The Dynamics of Governmental Violence and Repression* (Westport, CT: Greenwood, 1984) and now in the present volume, have brought together an excellent group of writers to begin to fill the lacuna left by Western social scientists' poor perception of what has been going on in the Third World (and also in the First and Second Worlds).

The previous blindness to what should have been obvious to everyone had its reasons, and one was the lack of conceptual tools. Given that the existing conceptual tools failed to take into account governmental violence and repression, how should one start to develop the proper equipment for studying the subject? Some things can be done by just working on the concept in question: defining it, suggesting how to measure it, estimating values and running correlations, postulating hypotheses, and so on, and this volume does a good job here. In the long run, however, we will have to frame a picture of the social and political world that integrates force, coercion, and terror into a wider view of how politics works. While the book makes a few suggestions, most notably in essays by the editors, it still leaves much for future writers.

Several essays use the notion of utility maximization in a decision model, which provides insight but holds dangers and blind spots as well. The insight is that governments are trying to accomplish something when they kill people. The bias is that it makes the decision to terrorize appear too rational, ignoring entirely the very interesting psychological work from Erich Fromm and T. W. Adorno to Milton

Rokeach and now Robert Jay Lifton. It also makes states seem too much as unitary actors, which, if it were the case, would leave no occasion for repression. Hiding in the shadow of this kind of analysis is the metaphor of a polity as an organic body that must rid itself of disease or cancer, such as is exposed in Lopez's discussion of the national-security doctrine. Social utility cannot exist as it is impossible to aggregate individual utilities, and it could not be arrived at if it did exist, given the logical problems of collective action and the paradox of voting. Utility maximization as a rationale for governmental decisions masks their partisan and self-serving character. None of the authors mentions that the most brutal governments are also exceedingly corrupt.

These essays straddle another conceptual problem—the level of analysis. Most of the writing assumes that since the state carries out the violence, the cause of the violence should be at the level of the state or its constituent parts. For instance, Lopez writes about the national-security doctrine in Latin America with no reference to the predominant role of the United States in military training on the continent since 1948. On the other hand, Wolpin in an exhaustive analysis of variance confirms Ruth Sivard's conclusion: "All of the worst offenders are clients of one or the other superpower" (p. 120), and Stohl, in the most interesting conceptual development of the book, points out that much international-relations theory and military strategy portrays the international world in such a way that one should expect intense conflict within countries in the Third World. One of Gurr's 14 hypotheses reflects the same consideration: "Regimes involved in proxy big power conflicts are likely to use the most extreme forms of violence against challengers, including state terrorism" (p. 60).

One has to turn to two other essays to see the correct implication of this tie between international relations and state violence. Grant Wardlaw shows how terrorism breeds counterterrorism if democracies are not alert, which is just what the terrorists want. Robert Friedlander points out that the only real alternative to international terror is that "implausible dream," international law.

JOHN F. McCAMANT

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Religion and Modernization in Southeast Asia.

By Fred R. von der Mehden (Syracuse, NY: Syracuse University Press, 1986. viii, 240p. \$29.95, cloth; \$14.95, paper).

In this volume Professor von der Mehden returns to some of the topics discussed in his *Religion and Nationalism in Southeast Asia* (Madison: University of Wisconsin, 1963), but he has broadened the inquiry to grapple with the entire range of modernization issues. He is primarily concerned with Theravada Buddhism in Burma and Thailand, Islam in Malaysia and Indonesia, and Roman Catholicism in the Philippines, although mindful of the situation and role of minorities in all these countries. In the early chapters, the author emphasizes the lack of scholarly consensus on modernization theory, but whatever the conceptual and methodological problems, he regards some notion of modernization as indispensable. The core elements of modernization in his definition are "technological development and the maintenance of a modern nation-state" (p. 48).

In this balanced and judiciously argued study, the author asserts that Western scholars since 1945, like some of their nineteenth-century predecessors, have permitted secularist bias to lead them to the too sweeping conclusion that religion is a massive obstacle to modernization in Southeast Asia. The indictment of religion charges that it instills otherworldliness or fatalism, diverts economic resources from development, supports a non-productive clergy, stirs up communal conflict, and in general upholds traditionalism and opposes change. While the book contains evidence to give partial support to all of these charges, the author's aim is to show that the "massive obstacle" generalization is seriously flawed. The undifferentiated dismissal of, or attack on, religion obscures too much.

The author is surely on the right track in insisting that the big question be divided into manageable parts. And throughout the book he demonstrates exemplary restraint in providing answers; in many cases the empirical evidence is ambiguous or simply lacking. Methodological problems abound, particularly in trying to link formal religious tenets with the behavior of adherents of that religion. Even when dealing with more concrete contemporary religious institutions—the hierarchy,

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clergy, and lay organizations—the author is most impressed by the lack of a clear direction. "In sum, although it is difficult to generalize about the role of religious institutions in the process of modernization, recent years have seen expanded efforts toward modernization by both lay organizations and clergy. Technological change remains only a marginal interest to the vast majority of the religious in Southeast Asia, but changes are underway" (p. 98). The reader might wish for a stronger conclusion, but these two sentences accurately summarize the facts presented in the chapter.

The author weighs the pros and cons of religious expenditures as they affect modernization and finds that they are fairly well balanced. The Western critique of popular religious beliefs, he asserts, has frequently missed the point: "At many levels religious belief has been a force in delaying change, but very often as an indistinguishable part of a more complicated process" (p. 201). The author deals with the political manipulation of religious symbols to forge national unity or to foster modernization, and finds that these efforts have frequently been ineffectual and counterproductive. While the influence of religion has been weakened by the forces of modernization, there is no inherent and inevitable conflict, and religion continues to provide identity and security for many educated youth in Southeast Asia.

Von der Mehden has made an important point in this book. The notion of religion as "massive obstacle" has been on the wane for at least a decade and a half but this is the first effort to subject it to systematic analysis with regard to Southeast Asia.

One is left, however, with the sense of an

important question not addressed. Surely Buddhism, Islam, and Catholicism are radically different religious systems. One would expect them to have significantly different socioeconomic-political effects. Yet there is little indication in this book that modernization is likely to fare better interacting with one religion than with another, nor is there any systematic analysis of the differing points of tension in the three faiths. The special character of Southeast Asian Islam may have something to do with the disinclination to make comparative statements. With syncretism in Indonesia and communal identity in plural Malaysia, generalizations about Islam do not work nearly as well in Southeast Asia as, say, in Pakistan. Nevertheless, useful comparative statements can be made.

In terms of political modernization, Buddhism, without a divine law to regulate society and with little inclination toward the formation of political ideologies, presents few problems, whereas the notion of man-made laws in a modern legislative state will continue to encounter serious opposition in Muslim societies. Origins sometimes explain little, but it is of continuing relevance that the Buddha was a prince who fled from political power in the pursuit of individual salvation, while the revelation given the Muhammad concerned the building of a total divinely ordered state and society. This is not to suggest that these differences and manifest tendencies are necessarily the most important facts, but they are a part of the overall picture.

DONALD E. SMITH

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INTERNATIONAL RELATIONS

South Asian Insecurity and the Great Powers.

By Barry Buzan and Gowher Rizvi, with Rosemary Foot, Nancy Jetly, B. A. Robertson, and Anita Inder Singh (New York: St. Martin's, 1986. xii, 257 p. \$32.50).

We political scientists spend too much time reinventing the wheel. In this case the wheel is

called a *security complex* (p. 8). It bears a striking resemblance to groupings of states that have otherwise been called *subordinate systems*, *subsystems*, or *regional subsystems* since Leonard Binder wrote a pioneering article, "The Middle East as a Subordinate International System" (*World Politics* (NJ) 10 [1958]: 408-29). Subsequently, Michael

Brecher applied a similar approach to the object of study now of concern to Buzan et al. ("International Relations and Asian Studies," *World Politics* (NJ) 15[1963]: 213-35). Taking the perspective a step further, Louis J. Cantori and Steven L. Spiegel published in 1970 a comparative study of subordinate systems, *The International Politics of Regions* (Englewood Cliffs, NJ: Prentice-Hall).

To be fair, Buzan does cite Brecher and Cantori and Spiegel in his introductory chapter ("A Framework for Regional Security Analysis") but implicitly dismisses them as inadequate or at least unworthy of replication. By identifying Cantori and Spiegel with "the perspective of integration" (p. 6), Buzan reveals a lack of knowledge not only of the body of Cantori's and Spiegel's regional-systems work but a lack of appreciation of the distinctiveness of the regional-systems approach from integrationist, transactional, and other approaches to regional study. That Cantori and Spiegel distinguish their approach from the integration perspective could not be more definitively stated than in the title and content of their article, "The Analysis of Regional International Politics: The Integration versus the Empirical Systems Approach" (*International Organization* 27 [Autumn 1973]: 465-94). The regional-systems analyst is concerned with disintegrative as well as integrative variables, with conflict as well as cooperation.

In short, there is a body of regional-systems literature built upon and around the tradition of Binder, Brecher, and Cantori and Spiegel of which Buzan et al. were either unaware or which they chose to ignore in their joint work on South Asia. Concepts from that body of work, much of it security oriented, could have served them well. For example, the Cantori-Spiegel notion of *core* (*International Politics of Regions*, 20) could have been used with much more flexibility than Buzan's *security complex*. A regional subsystem can have by definition (and in "reality," as Buzan properly requires) more than one core, whereas Buzan's analogous nomenclature for a multicore region is that it is a *complex of complexes* (p. 11) or a *supercomplex* (p. 12). The Middle East, with Gulf, Arab-Israeli, and Maghreb cores or security complexes is an example. Furthermore, the Cantori-Spiegel concept of *intrusive system* (*International Politics of Regions*, 25) is

much more elegant and much less confusing than Buzan's *superregional level* of analysis (p. 15), which, he admits, causes him problems in deciding what to do with China: "A case could be made for including China on the global level. . . . The odd position of China means that some confusion will result no matter which level it is included within" (p. 17). It is not confusing to include China in the intrusive system, as Cantori and Spiegel specifically recommend.

While Buzan's introduction is therefore of little use to the international-relations theorist because it neither builds upon long-established conceptual foundations nor, in my view, offers a better alternative approach to regional-security studies, Buzan and Rizvi do deserve credit for assembling a collection of pieces of considerable interest to South Asia security specialists. The substantive chapters (2-8) are generally up-to-date, well presented, and evocative of fresh insights into the multilevel security relations in which South Asian states are involved.

Furthermore, the final chapter by the two principal authors, "The Future of the South Asian Security Complex," does develop useful standards for judging prospects for future system transformations in the area. Buzan and Rizvi convincingly argue that even a change in external power alignments (though unlikely) "in which the Soviet Union is aligned with Pakistan, and China and the U.S. are aligned with India [following, for example, a flip-flop similar to the one that occurred on the Horn of Africa]. . . . from the perspective of our models . . . would not disturb the continuity of the status quo within South Asia. . . . Nothing could illustrate better than this speculation the impact and importance of the local and regional patterns of security relations in the larger picture of international relations as a whole" (p. 239). Nothing indeed. And that is perhaps the greatest overall virtue of this book. It serves to remind globalists and would-be globalists that by ignoring or downplaying regional dynamics they miss much of the essence of world politics.

WILLIAM L. DOWDY

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Book Reviews: International Relations

In Whose Interest? International Banking and American Foreign Policy. By Benjamin J. Cohen (New Haven: Yale University Press, 1986, xi, 347p. \$19.95).

There has been a substantial resurgence of interest in political economy in recent years. This has been particularly noticeable in the area of international economic relations, where economic issues (initially relegated by political scientists to the realm of low politics) are increasingly recognized as politically salient. At the same time, economists are increasingly willing to acknowledge the political constraints that influence economic policy-making, as well as the important interdependencies among economic, security, foreign policy, and political considerations.

Benjamin J. Cohen, the William Clayton professor of international economic affairs at the Fletcher School of Law and Diplomacy at Tufts University, has been one of the leading contributors to the "new" field of international political economy. *In Whose Interest?* is of the same high quality as his previous important books, *The Question of Imperialism* (New York: Basic Books, 1973) and *Organizing the World's Money* (New York: Basic Books, 1977). Here he treats the complex and increasingly important nexus of issues involved in international banking and U.S. foreign policy. After a useful and aptly titled introduction, "High Finance/High Politics," Cohen presents a fascinating set of chapters on the historical evolution of international banking and an important series of case studies of U.S.-policy episodes involving the Arab money weapon, the freezing of the Iranian financial assets in the United States during the hostage crisis, the Polish debt question, and the current international debt crisis. In his last chapters, he turns to policy recommendations for dealing with this debt crisis and for improving decision-making processes.

Cohen writes from a sensible, middle-of-the-road perspective. He sees most issues in shades of gray; channels of influence are multidirectional and complex. Banks are neither the all-powerful villains of popular Marxist literature nor the ideal performers of efficient-markets theory. Banks, he argues, have fallen prey to disaster myopia with the consequent overlending that is one, but far from the only, important reason for the current international debt

crisis. Government is viewed sympathetically, with officials generally striving to do good, but the efficiency of policy initiatives is often undermined by poor planning or bureaucratic infighting, or both.

Despite the sensibleness and lack of drama in Cohen's perspective and recommendations, the reader is caught up and carried along by the writer's skills. A cynic might belittle his suggestions, such as a better information-gathering and planning system and the creation of a structured dialogue between the government and banks, but as one who spent a number of years in government service, I can attest that these are messages well worth repeating. What could be a list of platitudes in the hands of some become meaningful lessons through Cohen's enlightened use of historical examples.

Can we hope to improve our decision-making processes systematically and significantly? Having seen numerous reforms of the formal economic-policy coordination mechanisms within government undertaken with few noticeable results achieved, I have become skeptical of the role institutional innovation can play in this area. Perhaps more important are efforts to encourage longer-range thinking and sensitivity to the complexity of issues, but even here the role of institutional innovation is limited. In the 1970s I directed Treasury's international research and planning staff, which was created for just such purposes. It was actively used by one secretary, largely ignored by a second, and abolished by a third. Thus I am inclined to feel that what is most important is the attitude of senior officials toward the need for good policy analysis. Making Cohen's book mandatory reading for all policy officials would be a good beginning toward improving our policy-making process.

THOMAS D. WILLETT

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Foreign Aid, War, and Economic Development: South Vietnam, 1955-1975. By Douglas C. Dacy (Cambridge: Cambridge University Press, 1986. xix, 300p. \$39.50).

Many political scientists see the Vietnam War as the beginning of the end of the United

States' primacy, as the first symptom of its later spastic efforts to regain hegemony by relying on a foreign policy designed almost exclusively around the use of force. But it was more than that (if indeed that is what it was). It was also a test of theories and hopes about the potential of foreign aid as an instrument for the promotion of political and economic development in friendly countries, as an indirect contribution toward the creation of an unthreatening and stable world order. Most books about Vietnam center about the first set of issues; Dacy's book is a thorough and convincing exploration of the second.

There are special circumstances in the Vietnam experience that have discouraged economists and political scientists from drawing generalized conclusions about development. The huge overlay of U.S. foreign aid dominating the economy, the crescendo of rising guerrilla activity culminating in outright war, the hesitant leadership facing apparently insatiable demands for reform, and the absence of the aura of legitimacy, all so overwhelmed the processes of development that the lessons are hard to see. But other nations have faced similar problems, in various combinations. Windfalls like mineral discovery or sudden rise in oil prices, ethnic and ideological unrest, inexperienced governments, and unhistorical claims to power are commonly found equivalents to the special circumstances of Vietnam. Dacy shows how the Vietnamese effort is informative both about the processes of development and the potential application of foreign aid on their behalf.

This carefully documented economic history makes it clear that Vietnamese economic progress during the early years of independence (1955-59) was better than generally reported, though far from sufficient, and that it was political failure that robbed the population of what fruits it produced. The best that foreign aid could do as military costs rose was to check inflation and to encourage an occasional economic reform in the direction of liberalization. One of the by-products of foreign aid—the black market—contributed to both outcomes. A surprising finding is that the major spurt of economic development seemed to have occurred in the wake of the unsuccessful Tet offensive, whose consequences stimulated and liberalized both the economy and polity for a time until the withdrawal of U.S. troops

and the full-scale invasion from the North took their toll.

Economic-development experience in the context of international and domestic political uncertainty is a neglected area of empirical research. It is fortunate that it has been so well carried out here. Major economic decisions, such as the tactics of devaluation during a period of inflation, tax measures designed for redistributive purposes in the context of rapid financial turnovers, the fixing of interest rates to encourage savings, the use of parallel foreign-exchange structures for different imports, the consequences of commodity subsidies, and even the consequences of land reform, are all carefully explored. There is an ingenious use of data to estimate such features of the Vietnam experience as comparisons of growth rates across sectors, the effects of the Commercial Import Program on industrial development, the economics of increased "dependency" associated with different aid levels, the monetization of the economy as an indicator of development, changes in farmers' income associated with land reform, and contrasts between rural and urban income changes resulting from different policies. The data expose some absurdities in economic convention, such as the interpretation of foreign aid as "savings" and the theory that control over foreign-exchange levels could offset the inflationary effect of the very presence of U.S. troops. Perhaps the last persuasive argument appears in the final chapter of the book, in which Dacy tries to show how a military threat can contribute to development if a country is receiving foreign aid. This demonstration consists of a pairing of Israel, Taiwan, Korea, and Vietnam with Latin American countries that were "nonthreatened" and "moderately aided," to show that Vietnam was the odd man out in not developing more impressively. But the conclusion, that the relative failure came about because neither the government nor the donor really placed development as a major priority, is fully justified by other data presented in the book.

The reader will find much to learn from the Vietnamese economic experience, and though the achievements of the foreign-aid program are fully documented here, no one will be likely to conclude that the U.S. presence in the later years of independence was a manifestation of superpower, or even great-power,

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wisdom. This is not a revisionist tract, only a reexamination of an experience from which U.S. citizens have learned all too little.

On the dust jacket, the publisher expresses the modest hope that this book could be used as supplementary material for courses on economic development at both undergraduate and graduate levels. That is a reasonable hope. Courses that make use of this rich case study will be the better for it, as indeed will foreign-policy makers.

JOHN D. MONTGOMERY

Harvard University

Perceived Images: U.S. and Soviet Assumptions and Perceptions in Disarmament. By Daniel Frei (Totowa, NJ: Rowman & Allanheld; 1986. xvii, 323p. \$26.50).

Daniel Frei correctly asserts that we need a careful, document-based description of Soviet and U.S. perceptions of one another and of the conceptions on which their approaches to security are based. This volume does not even begin to satisfy this need. It is little more than a string of quotations from official documents cited out of context to present the erroneous impression that both superpowers are monolithic in their respective points of view.

Frei bases his "analysis" on official documents by arguing—really proclaiming—that these are indicative of what policymakers in Moscow and Washington really believe. A careful scholar who chose his documents wisely and interpreted them in context might make a good case that they offered insight into policymakers' minds on a carefully defined range of issues. Frei is utterly insensitive to any of these methodological concerns. Take the documents he uses. For the United States, these are the annual reports of the secretary of defense, U.S. military-posture statements, and press releases from the State Department's Bureau of Public Affairs. These are the least likely sources to reveal thinking of officials. They are self-conscious public-relations gestures designed to sell military appropriations to the Congress or justify specific administration foreign policies to elite opinion in this and other countries. Any correspondence between documents of this kind and what policymakers really believe is purely coincidental. Their use

is doubly inexcusable in an open society like the United States, where much better documents are available for analysis.

Documents must be interpreted within their proper context. Pronouncements, especially public ones, are always designed to influence particular audiences. Very little can be inferred from such a statement unless one knows who made it, the audience it was aimed at, and the political consideration at the time, internal and external, that influenced its substance, tone, and timing. Students of Soviet and U.S. foreign policy routinely engage in this kind of analysis. Because public documents are all that is generally available in analyzing Soviet policies and decision, Soviet scholars have been forced to develop particularly sophisticated methods for making inferences from their data. Frei makes no attempt to do this or to build on the work of others who have. Instead, he treats all statements as equal in value and equally indicative of what their authors believe. This is either naive or irresponsible.

Finally, there is the question of consensus. Frei portrays both superpowers as unitary in their thinking. This stands in sharp contradiction to the position of numerous highly regarded studies of Soviet and U.S. strategic and foreign policy. They indicate that strategic policies, the view each superpower has of the other, and their respective attitudes toward nuclear war and arms control have evolved considerably over the years. These studies also indicate that there are often substantial areas of disagreement and controversy within the Soviet and U.S. governments at any particular time. The best and most useful analyses are those that document these controversies and use them to shed light both on the substance of policy and the process by which it is made in the two political systems.

Perceived Images is a sham. It pretends to be something that it clearly is not. Surely, the United Nations Institute for Disarmament, which sponsored the study, ought to be able to do better than this.

RICHARD NED LEBOW
Cornell University

Development Strategies Reconsidered. Edited by John P. Lewis and Valeriana Kallab (New Brunswick, NJ: Transaction Books, 1986. viii, 190p. \$19.95, cloth; \$12.95, paper).

The conventional wisdom in the major international development agencies today is that government is largely responsible for the Third World's disappointing economic performance and that markets are the remedy for most of these problems. Even within the mainstream, however, there remains a healthy degree of skepticism about these neoclassical views, so the Overseas Development Council (ODC) in 1985 brought together several experts to review the current state of development strategy. Eight papers emerging from that conference are reproduced here, the fifth volume of ODC's series on United States-Third World policy perspectives. While none denies the importance of markets, each points out limitations in the laissez-faire paradigm for promoting development and reminds us that governmental and political factors are also critical.

Coeditor John Lewis begins the collection with an overview of the ongoing debate. His spirited defense of foreign aid, which will be familiar to those acquainted with Lewis's work, is guardedly optimistic about planned promotion of economic growth and social equity. In the next essay, Irma Adelman focuses explicitly on the equity issue, and calls for specific programs to expand access to land and education as the first step in development. Equally important, in her view, Third World states need to adopt economic policies that have a high labor content, which means basing development on agriculture or, alternatively, on exports.

John Mellor picks up the agricultural theme in the next paper, renewing his classic argument about how the rural sector can support an employment-based strategy of growth. Government has a major contribution to make by providing public goods such as research, education, and infrastructure. Interestingly, Mellor sees mutual enrichment, not conflict, between an agriculturally oriented strategy and one led by exports.

A noted proponent of export-led development, Jagdish Bhagwati, offers the next paper. Of all the contributors, Bhagwati is most sym-

pathetic to the neoclassical orthodoxy, though he sees virtue in public-private partnerships because they can help assure a stable policy environment. The thrust of his paper, however, is to refute arguments that export-oriented strategies have become less feasible under current world market conditions. Most telling is his observation that outward-looking countries have tended to adjust better than inward-looking ones to the economic shocks of the 1980s, a point reiterated in Leopoldo Solis and Aurelio Montemayor's essay. Colin Bradford, however, warns in his paper against simplistically applying the export-led model, which he shows to be based partly on a misreading of East Asia's experience. He points out that in countries such as Taiwan and South Korea, government intervention and import substitution actually have contributed to the industrialization drive.

In the next essay, Alex Duncan turns to the issue of how donors can help the poorer countries adapt more readily to changes in the global environment, which he feels necessitates greater national capacity for policy analysis and project implementation. According to him, economic assistance should be devoted more directly to improving management and institutions, to enhancing human resources, and to increasing the supply of technology.

The final contribution to this collection is by Atul Kohli, the sole political scientist represented. He disputes the prevailing opinion that authoritarian government is a necessary condition for economic development, pointing to the favorable performance of several democratic developing countries. Kohli is pessimistic about the spread of democracy in the short run; however, the major barrier, in his view, being the fragmentation and competition of Third World elites and not, as the neoclassical paradigm often postulates, the mobilization of the masses.

All the essays collected here are cogent, lively, and informative. Nonspecialists, for whom this volume is particularly intended, will find it a useful introduction to the current debate about development strategy.

ARTHUR A. GOLDSMITH

University of Massachusetts, Boston

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Essays in International Law in Honour of Judge Manfred Lachs: Études de Droit International en L'Honneur de Juge Manfred Lachs. Edited by Jerzy Makarczyk (The Hague: Martinus Nijhoff, 1984, 737p. \$108.00).

Manfred Lachs has had a 40-year career in international law, as diplomat, scholar, teacher, and, most recently, judge on the International Court of Justice (ICJ). His interests include disarmament (as architect of the Nuclear Free Zone in Central Europe in 1958); peaceful use of outer space (as chairperson of the UN Legal Subcommittee); legal control over local government; and international dispute settlement under the ICJ. The 50 essays in this volume (22 of them in French) reflect this diversity of both subject matter and authors' backgrounds (writers, scholars, practitioners, judges, and jurists from North America, Mexico, France, Great Britain, Poland, Finland, and the USSR, to mention a few).

Several articles should be of special interest to U.S. professors of international law. Jennings ("Teachings and Teaching in International Law"), following Lachs's footsteps, emphasizes that international law cannot be abstracted from diplomatic history or international political-economic events (p. 129). Similarly, McWhinney ("The Time Dimension in International Law, Historical Relativism and Intertemporal Law") discusses the conflict between the old and new in legal doctrine, the status quo versus the "newer political forces oriented to changing law in accord with changing society" (p. 180), and the ICJ decisions that have embraced Intertemporal Law, including *Namibia Advisory Opinion* (1971) and *Western Sahara Advisory Opinion* (1975). For ideological contrast, students might consult Tunkin of Moscow University ("Contemporary International Law—A New Historical Type of International Law") for a thorough but unoriginal exposition of historical types of international law, from slave-owning societies, to feudal and bourgeois law, to contemporary law defined in terms of the transition from capitalist to communist law.

In other articles, the specific role of the ICJ and international organizations is explored. For example, Elias ("How the International Court of Justice Deals with Requests for Advisory Opinions") dissects the relevant legal

and political criteria for the organs requesting advisory opinions, with special attention to the *Reparations* case. On a more explicitly political level, Franck ("Finding a Voice: How the Secretary General Makes Himself Heard in the Councils of the Nations") explores cases of the secretary's intervention and refusal and the legal precedent and procedures for such choices.

Finally, two substantive issues of most concern to Lachs are evaluated. Castaneda ("Negotiations on the Exclusive Economic Zone at the Third United Nations Conference on the Law of the Sea") offers a fascinating inside account of the negotiating process. And Blix ("Arms Control Treaties Aimed at Reducing the Military Impact on the Environment") emphasizes the Environmental Modification Convention, 1977, discussing the applicability of various provisions.

Numerous articles are of sufficient merit to the international-law community that library purchase is encouraged. Unfortunately, there is little concerted effort to integrate the material by broad themes. The Prott article ("The Judicial Philosophy of Manfred Lachs") might have provided some of the needed integration, especially had it been placed at the beginning or the end. Readers of books containing such diverse articles deserve to have the basic themes elucidated and my reading of the book suggests that this would not have been an unsurmountable task for the editor.

KAREN MINGST

University of Kentucky

Nuclear Ethics. By Joseph S. Nye, Jr. (New York: Free Press, 1986. xiii, 162p. \$14.95).

In this small book the author wrestles thoughtfully with one of the major moral problems of our age, maybe the moral problem of our age: is nuclear deterrence proper?

He is uncomfortable with the "self-indulgent" moral absolutists who see nuclear deterrence as unquestionably wicked and so move quickly to some sort of unilateralist or abolitionist position with, for Nye, too little concern for the dangers or practical realities. But he is also unpersuaded by the too cynical exponents of an amoral *realpolitik*, those "realist" strategists who want to minimize

moral concerns in foreign-policy making and so tend to sacrifice at the outset the Western values they claim they have adopted nuclear deterrence to protect.

Nye urges a middle course. Policy should be influenced by moral principles, policymakers by moral philosophers. The teachings of both the Kantians, with their rule-oriented perspectives, and the utilitarians or consequentialists, with their act-oriented perspectives, should be consulted. Leaders, strategists, and citizens should weigh all three essential dimensions of sound moral reasoning: ends (or motives), means, and consequences (likely results). Nye's recipe and the maxims he derives are solidly in the "just-war" tradition.

The only motive, or *end*, that can warrant possessing nuclear weapons is the "just cause" of self-defense via nuclear deterrence. But self-defense is given a pretty elastic definition; Washington is entitled, Nye argues, to use nuclear threats to defend not only the mere physical survival of the United States and its citizens but also its allies and friends, its "culture" and democratic institutions, and the current global geopolitical equilibrium (pp. 42-49). Such a broad definition of *self-defense* might license nuclear threats to protect either those far-flung "vital interests" so often invoked or even the overseas commercial investments and operations so important to the affluent U.S. lifestyle. Moreover, with such a definition, Nye or a follower of his maxims might even approve nuclear threats with compellent rather than deterrent intentions, threats designed to compel or persuade Moscow to do something (exit Africa, say) rather than to deter Moscow from aggression.

The key criteria to help people decide about the propriety of *means* in this arena are the criteria of proportionality and discrimination. Nye argues that nuclear deterrence can meet these tests. Nuclear weapons could conceivably be used in some limited ways such that their use would not be judged as disproportionate by many, though Nye is probably too sanguine about the possibility of controlling escalation up the ladder to clearly disproportionate levels. And if we concede that in the twentieth century the discrimination (or non-combatant immunity) test has already been relaxed to require only the minimization, not the complete avoidance, of civilian casualties, then some usages of nuclear weapons (at sea or

against remote military bases) could comply with the discrimination criterion almost as well as many standard (and apparently widely acceptable) usages of highly lethal conventional weaponry.

Consideration of the *consequences* of reliance on nuclear deterrence involves speculations about whether it really deserves the credit it usually gets for having prevented Soviet aggression that would otherwise by now have occurred, more speculations about risks and possibilities for the future, and then yet more speculations about possible psychic and political and economic effects of reliance on nuclear deterrence. Nye points up the hazards of all this speculating. But given his earlier insistence that we "start with a strong presumption in favor of rules and place a substantial burden of proof upon those who wish to turn too quickly to consequentialist arguments" (p. 23), one is left to wonder if he would really accept all these mushy speculations as "proof" tending to support reliance upon nuclear deterrence on consequentialist grounds.

Nye concludes with the usual "owlish" arms controller's suggestions for achieving "balanced deterrence" in order to reduce the risks of nuclear war during the next decade or two or three. And he urges the standard political and diplomatic approaches for improving U.S.-USSR relations in order to enable a gradual reduction of our reliance on the primitive and still morally troubling instrument of threatening catastrophe.

JAMES A. STEGENGA

Purdue University

The New Order of the Oceans. Edited by Giulio Pontecorvo (New York: Columbia University Press, 1986. 277p. \$30.00).

Covering some 70 percent of Earth's surface, the world's ocean areas are of significance in terms of food, energy, and mineral resources, as a site for waste disposal, a medium for the transit of the vast bulk of international trade, and as an arena for military operations. In recognition of the importance of ocean areas, three major multilateral diplomatic conferences under the auspices of the United Nations have been held since 1958 to determine

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the future legal and political order of ocean space. In light of changing technological and political circumstances, the Third United Nations Conference on the Law of the Sea (UNCLOS III) met for a decade and finally adopted a new comprehensive Law of the Sea Convention in 1982. Treating a vast array of ocean rights and duties, this treaty is of substantial complexity and embodies a mixture of both existing and new rules of international ocean law. From the perspectives of allocation of ocean rights and duties and the development of the international legal system, UNCLOS III ranks as one of the major events since World War II. In terms of broader aspects of international relations, it marks perhaps the first major diplomatic conference at which Third World states played a full and independent role in the shaping of a significant part of the international legal system.

The New Order of the Oceans, edited by Professor Giulio Pontecorvo of Columbia University, provides a selection of essays by a distinguished set of experts and diplomats on the resources of the ocean, ocean science, the development of the new ocean regime from the perspectives of Africa, Latin America, Southeast Asia, and the United States, and a speculative essay on the unfinished business of UNCLOS III. Though little of the substantive material in this book is new to specialists in the field, this volume does provide a good review of the major issues and problem areas treated at UNCLOS III. All of the essays are well written and informative; some are outstanding in their insights.

The issues surrounding deep-seabed mining ultimately led the United States government to refuse to sign the 1982 Law of the Sea Treaty. Accordingly, it is fascinating to read the essay by Marne Dubs, former industry executive and expert in ocean mining, who shatters a variety of myths associated with such mining, myths which contributed to rigid negotiating positions on the part of both the United States and a number of developing states. Surveying the current situation, he finds that the United States lead in ocean mining has withered away and that, diplomatically, the United States has been outmaneuvered by Japan and France.

Professor Thomas Clingan of the University of Miami Law School and vice-chairman of the United States delegation to UNCLOS III, provides a thoughtful and revealing assessment of

U.S. policy. Even in the early days of the conference, U.S. policy was driven not simply by navigational issues but also by concern with the resources of the continental shelf and the exclusive economic zone. Issues relating to deep-seabed mining were not seen to be of major economic or strategic importance and, Clingan asserts, were manipulated skillfully to draw attention away from matters perceived as having more fundamental significance such as those concerning navigation rights, continental shelf, and the exclusive economic zone. With the advent of the Reagan administration, seabed mining took on a new importance, however. Ambassador Djalal of Indonesia, in his contribution to this volume, sees this change as exemplifying this Reagan administration's "insatiable appetite" for having things its way at UNCLOS III.

In the closing essay of this book, Professor Jonathan Charney of Vanderbilt University Law School examines the important theme that a considerable amount of unfinished business remains in the wake of UNCLOS III and that the evolution of the law of the sea is a continuing process. While the drama attendant on UNCLOS III may be lacking in the postconference world, important developments are ongoing. The law of the sea remains a significant issue in contemporary international relations and *The New Order of the Oceans* contributes to our understanding of ocean law and politics.

LAWRENCE JUDA

University of Rhode Island

U.S. Trade Policies in a Changing World Economy. Edited by Robert M. Stern (Cambridge: MIT Press, 1987. 437p. \$25.00).

This book is a nonmathematical survey of the contemporary trade theory and a sample of the efforts of some economists to explicate the politics of trade policy in the United States. The survey of trade theory (notably Deardorff and Stern's essay) will be valuable to political scientists seeking a general knowledge of this area of economics, particularly since several of

the chapters deal also with the latest area of innovation in trade theory—the application of the theory of industrial organization and market structures to international trade. This latter development (known as the "strategic-industry" approach to trade theory and policy) is of special importance to political scientists because it makes relevant the behavior of firms and industries in imperfectly competitive markets, where each actor must predict the actions of others. Traditional trade theory assumed perfect competition, narrowing the scope for domestic politics to the very broad income-distribution effects predicted by the Stolper-Samuelson theorem (as in the Magee and Young essay).

The economists' explanations of political behavior are less satisfactory since, with one exception (the chapter by Magee and Young), they succumb to the temptation to treat political factors as exogenous constraints rather than objects of explanation. Magee and Young note that the pure theory of trade would predict that Democrats (representing labor-intensive, import-competing interests) should be more protectionist than Republicans and suggest that the reason this is not always so is that Democrats reduce protectionist pressure with policies that increase inflation and reduce unemployment. The authors of other chapters simply take political objectives as given and ask whether trade barriers are the most efficient method of attaining these goals. Thus, the book not only provides a good background in trade theory but is also useful in pointing out to political scientists areas of trade-policy analysis where they may have some comparative advantage over the economists (e.g., in explaining why governments may choose to build up defense industries with trade barriers rather than subsidies).

In explaining U.S. trade policy, the essays span at least four levels of analysis. Corden, Jackson, and Cooper all focus on the role of, and need for, international institutional constraints on sovereignty. Jackson laments the erosion of the General Agreement on Tariffs and Trade (GATT), which he blames partly on confusion (people do not understand that the terms *most-favored nation*, *nondiscrimination*, and *GATT* are not coterminous) and recommends the strengthening of both the procedures and structures of GATT. Corden's analysis is more abstract and discusses the need

for a global social compact that will meet both efficiency and distributional goals, protect people from the folly of their governments, and promote general harmony.

Several of the essays emphasize the state as a rational actor in the international economy. Srinivasan discusses the integration of defense objectives into trade policy, mostly by attempting to specify the domestic welfare trade-offs in linking the two. He also suggests that the use of trade barriers for military goals is usually unsuccessful, and that trade promotes peaceful political relations. Other essays adopt the rational-actor approach in recommending bargaining tactics (e.g., Dixit's emphasis on the need for the United States to be committed to a tit-for-tat policy of retaliation—though he believes the United States is not institutionally equipped to pursue such a tactic).

Krugman's essay on strategic-industry policy is also in the rational-actor mode, since it assumes a goal of national income maximization and leads to an emphasis on a state's making credible international commitments to increase its own oligopolists' share of global profits. However, the strategic-industry approach is so sensitive to critical assumptions about, for example, numbers of domestic firms, foreign firms' reaction functions, and the internationalization of capital markets, that Krugman concludes there is unlikely to be any gain from attempting to implement a strategic-industry trade policy. Nonetheless, the industrial-organization approach to trade does suggest the importance of domestic interests in explaining trade policy. Dornbusch and Frankel underscore the importance of this aspect of trade policy in an essay concluding that exchange-rate movements have had much less effect on trade policy than have structural changes in industrial organization (e.g., productivity, sectoral interdependencies). None of the essays make use of the large literature on rent-seeking models of interest-group behavior to explain the cross-sectional structure of protection.

All of the papers are worthwhile reading. If there is any general fault, it is that the book exemplifies a typical conundrum for economists: the difficulty of maintaining the abstract rigor of trade theory and at the same time explaining the political realities of trade policy. Political scientists ought to be able to help here, and this book will give them numerous insights

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into areas where such help may be usefully rendered.

JOHN A. C. CONYBEARE

University of Iowa

International Trade and the Tokyo Round Negotiation. Gilbert R. Winham (Princeton: Princeton University Press, 1986. xi, 449p. \$45.00, cloth; \$13.50, paper).

This is an important book on an important subject. The past 10 years have seen a flood of theories that purport to explain the behavior of governments in issues of foreign economic policy, theories based on the simple calculus of organized group interests, on the role of the *hegemon*, and on the conditioning power of economic culture. It is refreshing to look closely at the complex processes of a major economic negotiation, constantly looking for evidence of the utility of the various theories.

Predictably, nothing seems to work in quite the way the theories project. Those who prefer to think that national positions in international economic negotiations are the simple sum of organized group interests may take comfort in the fact that pressure groups indeed played a substantial role in the Tokyo Round. But both the context and the weight of that role varied from one country to the next, depending in considerable part on the nature of the processes available to pressure groups to express themselves. In some cases, the pressure-group representatives came to identify themselves with the larger purposes of the negotiation; in others, they accepted their lack of influence with resignation; in still others, they reacted with intransigence.

Consistent with the approach of most negotiation theorists, Winham looks on international negotiations such as the Tokyo Round as representing for each participating country an exercise in two simultaneous negotiations, one an internal negotiation within the country's domestic establishment, the other an external negotiation with other countries. A highlight of his book—a unique effort, as far as I am aware, in the literature of international economic negotiation—is his contrast of the internal negotiating structures of the United States, the European Community, Japan, and Canada. The U.S. structure drew primarily on

representatives from the special-interest groups, the EC structure primarily on officials representing national viewpoints, the Japanese structure primarily on representatives of the various government bureaucracies, the Canadian structure primarily on provincial and bureaucratic representatives. Thinking of the nation-state simply as a rational unitary actor or as a faithful representative of its organized interests proves not very helpful in predicting its behavior.

Winham produces a provocative generalization to explain the U.S. Congress's overwhelming support of the Tokyo Round, a generalization worth the price of the book. The 1974 Trade Act, which provided the statutory basis for U.S. participation in the Tokyo Round, contained two innovations of profound importance. First, the act modified the basic constitutional relationship of the Congress to the executive branch in trade matters by assuring the executive in advance that the Congress would consider the negotiation's results on a "fast track," that is, under a special set of rules that guaranteed a simple up-or-down decision within 60 days. Second, the act required the executive to involve the representatives of the organized pressure groups deeply in the negotiation, thereby extracting the Congress itself from its uncomfortable position as middleman between the special interests and the executive.

I shall resist the usual temptation of the reviewer to introduce his own hypotheses in explanation of the success of the Tokyo Round; it is sufficient to note that Winham's view may not prove to be the last word. But his explanation is seminal for anyone concerned with understanding foreign economic policy-making in the United States.

In the end, the book leaves unanswered what may be the most important question of all. If in 1979 governments were able to achieve the remarkable agreement represented by the Tokyo Round, why did the agreement prove so empty in its subsequent impact? Why, indeed, did international trade relations run swiftly downhill in the years immediately following? In his first and final chapters, Winham deals somberly with the disappointing years following the Tokyo Round and the disconcerting prospects for the future. But his ruminations on that subject leave the critical question unanswered.

Still, Winham's careful research has carried

us a step nearer to understanding the processes involved in international economic policy-making. His work is especially important in putting some naive theories of economic policy-making in a more realistic perspective.

RAYMOND VERNON

Harvard University

The Dynamics of Deterrence. Frank Zagare
(Chicago: University of Chicago Press,
1987. xiii, 194p. \$22.00).

For some time now the theory of nuclear deterrence has stood on shaky foundations. Political philosophers, and strategic theorists, as well as U.S. and French clerics have recognized the paradoxical nature of the deterrent threat in the nuclear age: the seeming irrationality of response once deterrence fails, and the dilemma of disassociating the threat from the use of nuclear weapons. Given such contradictions, Frank Zagare observes, "If deterrence were a building, it would probably be condemned" (p. 1). His goal in this book, therefore, is to provide a sturdier framework on which to place the strategic doctrines of the superpowers. Unfortunately, the author erects only a flimsy frame of revealed preferences.

Zagare argues that deterrence works when it has the characteristics of a prisoner's dilemma (PD) game, where the actors prefer war to unilateral disadvantage (the action that is being deterred). Such a formulation is immediately counterintuitive because the Nash equilibrium of the classic PD game is mutual defection or, in this case, war. If deterrence really resembles such a game, then one should expect deterrence to fail and, in a nuclear world, mutual destruction to result. Zagare argues that the inadequacy of this formulation lies not with seeing deterrence as a PD game but rather results from the limitations of classic game theory where actors choose strategies simultaneously. By simply stipulating that actors can move sequentially and in response to an opponent's strategy, mutual cooperation actually emerges as a "non-myopic equilibrium" in a PD game. This can be seen by following the reasoning of players who start from mutual cooperation in prisoner's dilemma: if one defects to get his or her best outcome, then the opponent will also defect

leading to mutual combat, which is a worse outcome than the initial status quo. Zagare's "theory of moves" presumes that both actors have the foresight to recognize that unilateral defection will lead to a worse outcome than one's starting position.

The remaining problem for a theory of deterrence in the nuclear age lies in whether or not the leaders of the United States and the Soviet Union really prefer mutual destruction to some form of capitulation. The author argues that the revealed preferences of the superpowers support such an interpretation; historical examples such as Masada and the Alamo are evidence that actors often reveal a preference for some value above life itself; moreover, such preferences are rational. As Zagare states, "Statesmen seem to prefer, or at least are perceived to prefer, [in accord with the slogan] 'better dead than red' or alternatively, 'better a grave than a capitalist wage slave'" (p. 174; my emphasis).

Herein lie two fundamental problems with *The Dynamics of Deterrence*. First, for rationality to be useful as a concept at all, we must be able to stipulate its limits and to recognize that some actions are irrational. Usually we do so by relating the efficacy of an action to the goal that one is trying to attain. In the cases of the Texans at the Alamo and the Jews at Masada, martyrdom is rational when it keeps a larger dream alive, or when losing a battle can lead to winning a war. Likewise, fighting on is rational if combatants believe that they will be killed when captured. Such martyrdom should be seen as irrational, however, as a counterthreat carried out when deterrence has failed. I am willing to grant rationality to the slaughtered at Masada and the Alamo on the principle of charity but not on the basis of them carrying out a suicidal threat when deterrence failed. There were not battles undertaken as a counterresponse when a preferred status quo was upset but battles in a larger war to upset a status quo predicated on the domination of some of the participants. These examples are entirely irrelevant to a discussion of nuclear deterrence and fail to resolve the paradox of counterresponse as irrational in a nuclear age.

Second, while nonnuclear deterrence may resemble a prisoner's dilemma game, such preferences can never be taken for granted in nuclear deterrence. The scale of mutual com-

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bat is so destructive, that there will always be doubt as to whether one's opponent really prefers that to anything else as an outcome. The question of course will remain, "What purpose could justify the risk of mutual destruction?" Deterrence is always about deterring something. It does not exist independently of a specific action to deter. When coupled with a specific action, such as an invasion of Western Europe, a theory of deterrence can have meaning. In connection with such general threats as Communist domination or a red menace, deterrence becomes so elastic as to be worse than useless. To see the world in such stark false oppositions as "better dead than red" may very well create the conflict one is trying to avoid.

Zagare may be right that deterrence as a theory is successful when it resembles a

prisoner's dilemma game. It may be that we have not had a major war between the superpowers since 1945 because the leaders of the Soviet Union and the United States rationally prefer massive death for both of its peoples to some abstract principle or specific encroachment by its antagonist. Alternatively, it may be that deterrence works best when opponents view the other as slightly irrational, that is, as willing to bring ruin on an unprecedented scale for everyone in order to punish a transgression that has already occurred. It is the peculiar dilemma of citizens living in an age of mutually threatened genocide not to know whether to prefer our leaders rational or not.

STEPHEN JOHN STEDMAN

Washington University

POLITICAL ECONOMY

The Evolution and Management of State-Owned Enterprises. By Yair Aharoni (Cambridge, MA: Ballinger, 1986. xvi, 411p. \$29.25).

Despite the recent episodes of privatization in some countries, state-owned enterprises continue to play important roles in the world's economies. Their performance therefore is of practical concern to the citizens in many countries. Theoretically, state ownership remains a meaningful alternative to private—especially private corporate—ownership. As such it is of central interest to political economists, particularly to students of democratic capitalism. Aharoni seeks to inform citizens and policy-makers about the roles state enterprises play and to help them design better institutions through which to manage state enterprises. He also seeks to develop a theory of state enterprise in democracy (p. 39).

The book's value lies in the quantity and range of information it contains. Aharoni catalogs a large amount of material having to do with numerous aspects of state enterprise within and across countries. Some of the information he presents is dated. For instance, his

account of the Austrian experience contains little information about the financial difficulties that that country's state steel combine has encountered or about the political reforms that have been adopted recently in an attempt to ameliorate those difficulties (pp. 84-87). Also, most political scientists will want more information about citizens' attitudes toward state enterprises (e.g., survey results) as well as about the views and activities of political parties and interest groups. As a summary of so many aspects of, and national experiences with, state enterprise, however, Aharoni's catalog is relatively current and comprehensive.

Aharoni's discussion rests on some solid analytic footings. For example, his evaluation of state-enterprise performance is rooted in an appreciation of the similarities and differences between public and private firms and also of the character of the industries in which public firms usually operate. As a result, he avoids the mistakes of applying the same performance criteria to both kinds of firms and of assuming that private managers always perform efficiently. In addition, throughout much of his book Aharoni is sensitive to the fact that

government is not a monolithic agent with well-formed (social) preferences. He recognizes that government is a hierarchy of competing interest groups and coalitions (p. 376). He therefore avoids the mistake of assuming that efficiency is the sole criterion by which state-enterprise performance should be evaluated.

Aharoni makes some lasting contributions to the study of state enterprise. He dispels many of the myths about, and refutes, some common, monocausal explanations for state enterprise. He identifies gaps in our knowledge about state enterprise. He illuminates logical tensions between such things as public accountability and the pursuit of long-run efficiency. And he produces collections of (cross-national) generalizations about the formulation of state-enterprise objectives and the determinants of state-enterprise performance (pp. 158-60, 215-16).

In some respects, Aharoni's analysis lacks rigor, and the value of some of his contribution is somewhat limited. For example, Aharoni argues that state enterprise is subject to both more and less public scrutiny than private enterprise (cf. pp. 56, 388 with 41, 215, 373). The conditions under which state-enterprise operations are more "visible" than the operations of private enterprise and hence subject to more government intervention need to be more carefully delineated and analyzed. Also, Aharoni's review of existing empirical studies of state enterprise is not synthetic. He makes little effort to determine the extent to which the conflicting results of extant studies of state-enterprise performance are outgrowths of the use of alternative data sets or statistical techniques, let alone to carry out improved empirical analyses or new, rigorous tests of the generalizations he himself proposes.

Most serious for political scientists is the weak analysis of democratic controls over state enterprise. Aharoni emphasizes citizens' rights, willingness, and abilities to hold state enterprises accountable for their performance (e.g., pp. 56, 194, 251). At the same time, he takes as axiomatic politicians' abilities to use state enterprise to serve their selfish, short-run electoral interests, that is, to exploit citizens' ignorance about the workings and welfare consequences of state enterprise (pp. 37, 40, 67, 237, 388, 403). This is contradictory. We need a more rigorous, in-depth analysis of citizens' roles as collective owners of state enterprises

and of elections' roles as mechanisms for executing collective ownership rights. In addition, Aharoni assumes that interest-group politics is a constant source of inefficiency (pp. 149, 406). He does not systematically analyze the relative virtues of promoting state-enterprise efficiency through democratic-corporatist, as opposed to pluralist, forms of interest intermediation or, more generally, consider the possibility that interest-group negotiations reveal coalitions' (citizens') tolerance of inefficiency for the sake of some other goal like distributional equity; this in spite of the frequent references he makes to the experiences of countries with democratic-corporatist political institutions and, again, his expressed commitment to public accountability (p. 251). Rather, in the end, Aharoni favors turning over state enterprise to a set of "good" state managers. He advocates insulating state-enterprise management from electoral and parliamentary politics (chap. 11). Aharoni, like Shonfield and others before him, proposes, as a "model" for state enterprise, technocracy, not democracy as he seemed to promise in the first chapter. Of course, this is a contribution. The point is that the theory of state enterprise in democracy needs to be more fully developed and evaluated before we conclude that a technocratic approach to state enterprise is preferable to a democratic approach.

In sum, for those political scientists who are interested in the phenomenon of state enterprise, Aharoni's book is a useful catalog of information. Indeed, because of the solid analytic footings on which much of the discussion rests, most readers will find the book to be one of the best catalogs of information we now have on this subject. Those political scientists who are interested in building theories of state enterprise, especially theories of state enterprise in democracy, will find it to be useful as well but more for the theoretical challenges it poses than for the analytic insights and findings that it presents.

JOHN R. FREEMAN

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Superfairness. By William Baumol (Cambridge: MIT Press, 1986. xi, 266p. \$20.00).

"Probably the most persistent reason for noneconomists' resistance to our most cherished recommendations on micropolicy," observes Baumol, "is our determined disregard of their implications for distributional equity" (p. 1). Concepts and analytical tools have recently been developed, however, that permit a new understanding of distributional questions. Baumol's aim is to present this literature and to show its usefulness in the evaluation of concrete policy issues. Although he partially achieves his first objective, he falls far short of achieving the second.

In 1967, Foley proposed that a distribution of goods among agents with equal claims be declared equitable if no agent prefers someone else's bundle to his own. This is the notion of an *envy-free* (EF) allocation. If there is only one good, then *equal division* (ED) is the only EF allocation. Then ED is also *Pareto efficient* (PE). Otherwise, ED is not in general PE, but allocations that are both EF and PE usually exist. Indeed the Walrasian allocations obtained by taking ED as initial position enjoy both properties.

The introduction of the concept of an EF allocation constituted an important development in welfare economics for the following reasons. First, it is ordinal; economists' reluctance to use the cardinal concept of utility is the principal reason for their extensive reliance on the notion of Pareto efficiency, which essentially places no restriction on the distribution of welfare. Second, it provides a significant reduction of the set of PE allocations; in fact, in many economies with a large number of agents, only the Walrasian allocations from ED, which are generally few, are both EF and PE. Third and more importantly, it is a very natural concept of equity in situations where agents can meaningfully exchange positions, and its recommendations are in accordance with intuition. (To illustrate, an allocation at which one agent receives more of all goods than some other agent is obviously not EF.) Also, it does not rely on ad hoc notions such as, for instance, that of implicit incomes; the suggestion often made by economists to select allocations at which implicit incomes are equal across agents, that is, to allocate resources by

operating the Walrasian correspondence from ED, is unnatural since that correspondence has no more normative appeal than a number of other correspondences commonly discussed, when operated from ED.

It was quickly discovered, however, that EF and PE allocations may fail to exist in production economies (because of possible differences in agents' productive abilities). This disappointing result led to the development of an important literature devoted to the formulation and the evaluation of other fairness concepts. Unfortunately, none of them enjoy all of the appealing properties of Foley's original concept.

Baumol does not attempt to review the whole fairness literature, but his exposition of the basic concepts (chaps. 2 and 3) is clear and well illustrated with Edgeworth boxes, a tool ideal for this purpose. He describes the intricate structure of the set of EF allocations in the two-commodity, two-person case, relates the EF concept to the criterion of Pareto domination of ED (an allocation may be EF without Pareto-dominating ED; an allocation in the core from ED may not be EF). He also discusses the problem of evaluating the equity of a trade vector from a given initial position (it is natural to say that a trade vector is envy free if no agent prefers someone else's component to his own), and he notes certain important difficulties that arise when the concepts of EF allocation and EF trade are combined (for example, an EF and Pareto-improving trade from an EF allocation may lead to an allocation with envy). This sort of result has serious implications for the step-by-step implementation of distributional objectives. In this regard, his own attempt at devising a procedure that would yield a single-valued selection from the set of EF allocations via certain sequences of EF trades is instructive, in spite of it being largely unsuccessful. Finally, the author presents, although in much less detail, various generalizations of the EF concept to production economies.

On the other hand, the author's efforts to show the relevance of the fairness concepts to policy issues are unconvincing. His analysis of rationing schemes (chap. 4) yields no useful results, and his treatment of arbitration procedures (chap. 11) is very limited. (The latter would have been considerably helped by a more systematic use of the Edgeworth box

representation, a technique also perfectly suited to this topic.)

In spite of these serious flaws, however, the chapters discussed thus far are well unified by a common conceptual apparatus. They could have constituted the core of a worthwhile monograph. Unfortunately, the other chapters (a large portion of which are reprints from the author's previous works) devoted to policy issues such as conservation (chap. 5), industry pricing (chaps. 6 and 7), peak and off-peak pricing (chap. 8), and income equalization (chap. 9), are very disappointing (I will not discuss chap. 10, concerned with certain methodological issues pertaining to "economic illusion": this chapter, also very weak, stands somewhat apart). The analysis is conducted mainly in terms of the standard concepts of Pareto efficiency and Pareto domination. Chapter 7 does contain a fairly comprehensive, although superficial, review of various normative theories of cost allocation schemes, but an application of the concepts and methods of the first chapters is seriously attempted only in a section concerning utility pricing. However, apart from the fact that nothing specific is really said about utilities, the use of the EF concept to analyze a situation in which the two agents are a representative consumer and a representative firm and the two goods are a physical resource and a price is artificial. In fact, it illustrates the basic difficulty (discussed in chap. 11) with the application of the fairness concepts; their usefulness seems to be limited to situations in which all agents play fundamentally symmetric roles.

WILLIAM THOMSON

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The Reason of Rules: Constitutional Political Economy. By Geoffrey Brennan and James Buchanan (Cambridge: Cambridge University Press, 1985. xiv, 153p. \$34.50).

Constitutionalism is back. We see it return in a variety of forms. One version is the constitutional political economy developed by the "Virginia school" of public choice. The methodology of such a constitutional political economy is the subject of the present book. But there are many other types of constitutionalism now in the works, including forms of a

constitutionalist political economy very different from the Virginia one. These alternatives include applications of interpretive methods to the most fundamental and constitutive political questions as well as the more conventionally scientific study of how impartial standards can restrain collective decisions. There are hermeneutic, neo-Straussian, neo-Parsonian and causal-behavioral constitutionalisms, as well as the public-choice version under review here.

Why is constitutionalism back? To a degree, no doubt, it is simply a fad, fueled perhaps by the approach of the 200th anniversary of the great constitutions (the U.S., 1787; the Polish, 1791; the French, 1793). But it is surely more than a fad. It is a response to the current political situation, especially the crisis of the "democratic welfare state." This motive is quite clear in the book under review. But it is also a response to the crisis of political, social, and economic theory, an effort to redirect it to more fundamental questions and to questions of greater normative significance.

There is no question that the Virginia school of public choice is now and will continue to be an important player in this revival. The works of Buchanan, Brennan, Vanberg, and others define a distinct constitutionalist alternative. Anyone who takes the new constitutionalist renaissance seriously will have to study them carefully. Brennan and Buchanan's *The Reason of Rules* can best be evaluated in this context. It is important as a report of the current stage of development of the "Virginia" alternative. But it is unlikely, in my judgment, to be a more definitive statement. The book has many marks, in fact, of a transitional work. It is short and sketchy. Most of its arguments are not worked out in great detail, many are short summaries of more elaborate presentations elsewhere, without any attempt to achieve a "grand synthesis." The tone of the book is highly tentative, it is full of *maybes* and *mights*. The overall organization is rather weak; there are at least two dramatic and unexplained shifts in argument. Finally and most importantly, Brennan and Buchanan's book defends a position that, by the end, the authors themselves acknowledge to be self-contradictory.

The Reason of Rules is engaged in two important intellectual battles. It defends a morally skeptical, contractarian position ac-

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cording to which politics is the pursuit of self-interest analogous to exchange in the market. It attacks the view that politics involves the search for right answers, that it is more like the work of scientists or members of a jury. But the book is simultaneously engaged in another kind of dispute. It sets out a case for the constitutionalist position, for the importance of choosing relatively rigid rules that will provide constraints for day-to-day decision making. It bases this case on the empirical hypothesis that people are more "future oriented" in their private decisions than in their public ones. Hence, without constitutional rules collective decisions will be shortsighted relative to individual decisions. Among possible consequences of this pattern Brennan and Buchanan discuss excessive taxes, growing public debt, and inflation. In the end, these two positions, the morally skeptical and the constitutionalist, come into conflict. If politics is only the rational pursuit of self-interest, then the constitutionalist argument cannot succeed politically, because it cannot succeed (as the authors note) by appealing exclusively to self-interest. One of the two positions must give way. Let me offer a suggestion: in order to maintain their "public-choice" position, Brennan and Buchanan need only assume that "politics as exchange" is ordinarily dominant over "politics as science"; they do not have to assume the latter does not exist. If we allow both kinds of politics in our theory, we can be more optimistic about the future of constitutionalism and about the future of democracy as well. But we also have a new task: to understand that aspect of politics—however weak—that is best seen as the search for a right answer. It is an aspect of politics that seems to me far less well understood than "politics as exchange." And it requires something very unlike economic theory to understand it.

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Electoral Laws and Their Political Consequences. Edited by Bernard Grofman and Arend Lijphart (New York: Agathon, 1986. xiv, 335p. \$38.00, cloth; \$16.50, paper)

In an era of renewed legal and scholarly interest in the subject of democratic representa-

tion, Bernard Grofman and Arend Lijphart have collected 19 articles—18 previously unpublished—in a useful volume on the impact of electoral laws. The volume, which includes a very good bibliography and index, is divided into four parts: the first part (chaps. 1-5) examines the impact of election format on political competition; the second (chaps. 6-10) reviews systems of proportional representation (PR) and semiproportional representation; the third (chaps. 11-16) considers various aspects of plurality systems; and the fourth (chaps. 17-19) looks at redistricting.

The approach of the book is broadly comparative in nature, with 11 of the 19 essays dealing in whole or in part with electoral practices outside the United States. The first three chapters examine the validity of Maurice Duverger's "laws" on the relationship between electoral format and party competition. William H. Riker (chap. 1) and Giovanni Sartori (chap. 2) agree that Duverger's original formulations are "ambiguous" (pp. 20, 44), but Sartori disputes Riker's reformulation of the principal law. Sartori lays out a careful analysis of the conditions that give rise to various party systems and concludes with two "tendency laws" that are, in Sartori's words, "just about all that Duverger said": "Plurality formulas facilitate . . . a two party format and, conversely, obstruct multipartyism" and "PR formulas facilitate multipartyism and are, conversely, hardly conducive to two-partyism" (p. 64). Duverger himself (chap. 3) reviews party competition in Western European countries in the four decades since he first proposed his "sociological laws" (p. 70).

A second group of the "comparative" chapters (4, 6-7, 9-11, and 18-19) deals primarily with various aspects of PR abroad; these chapters cover such matters as national and state representational formulas in Australia; semiproportional representation (limited voting) in Japan and Spain; and special provisions for ethnic representation in Belgium, Cyprus, and several other countries. Lijphart (in chap. 10) cogently examines the different forms of PR, explaining the formulas and ranking the various approaches in terms of the correspondence of party representation to party vote. In a related article, Peter Taylor et al. review the "geography of representation."

For the most part, the "comparative" chapters offer a very readable, and sometimes eye-opening, introduction to foreign election

systems. An example is Richard S. Katz's "Intraparty Preference Voting." While pointing to the direct primary in the United States as the best example of voter control over the selection of party candidates, Katz notes that "the majority of democratic electoral systems . . . allow those voting for a party to determine, or . . . to influence, the identity of the particular candidates from among those the party has nominated." In a number of countries—Finland and Italy among them—*intrapartisan* defeat created more parliamentary turnover than did *interpartisan* defeat. Peter Mair's article on Ireland, where members of the Dáil are elected from multimember constituencies by the single transferable vote (STV), notes that candidates often seek election through constituent service to "particular bailiwicks" (pp. 293–94). Students of reapportionment in this country will be amused by R. J. Johnston's article on recent redistricting of the British Parliament. British courts in 1982–83 upheld the plan adopted by the Boundary Commission; in the process, the courts refused to establish a precise "definition of excessive discrepancy" (p. 282) in the population of legislative districts and, at the same time, recognized that (under statutory guidelines) "equality of representation for people is secondary to that of representation for places" (p. 284).

Among the articles focused primarily on the United States are selections on format of partisan ballots and candidate cross filing. Three deal primarily with the consequences of the Progressive movement of the early twentieth century. Leon Weaver traces the history of PR (usually STV) in the United States. Twenty-two city councils experimented with PR, mostly in the period between the two world wars, but by the late 1950s PR had nearly disappeared. In recent years PR has attracted renewed interest. Richard L. Engstrom and Michael D. MacDonald address the impact of at-large (as opposed to ward or mixed) elections on minority representation on city councils. The thesis that blacks are less likely to achieve "proportionate" success under at-large than under ward elections is, they conclude, "impressively documented" (p. 224). However, they find the evidence of the linkage between black representation and governmental responsiveness to minority interests to be ambiguous. Interestingly, in her review of the ef-

fects of nonpartisan elections (as compared to partisan elections), Carol A. Cassel suggests that a nonpartisan format "inhibits the election of black council members but promotes the election of black mayors" (p. 237).

Among the other articles on the United States, Gordon E. Baker evaluates the impact of the reapportionment and calls for "a long-overdue 'dialogue' about the fundamentals of representation" (p. 274). Arguing that two decades of court-supervised redistricting activity has brought "only a partial victory" (p. 266) in the battle for "fair representation," Baker criticizes the Supreme Court's preoccupation with strict mathematical equality in legislative districts to the neglect of broader questions of representation: "Finding ways to check partisan gerrymandering that has proliferated in the wake of one-person, one-vote rulings is probably the most pressing challenge" (p. 274).

Baker's essay raises questions about representational values that merit more attention than they receive in this volume. Litigation against at-large elections and partisan gerrymandering, for instance, has focused attention on political scientists' conflicting interpretations of the consequences of particular election rules. Yet taken as a whole, this collection gives too little attention to the substance—and drama—of the controversy surrounding "fair representation." In this respect, one wishes that the spirit of debate that marks the opening chapters might have been extended to other parts of the book. Then too, the editors' introductory essay might have more clearly identified the underlying issues that connect the wide-ranging subject matter of the various articles. Two or three of the articles are too brief, and thus sketchy; conversely, too much space (about one-fifth of the text) is devoted to the three chapters on Duverger.

Yet, on balance, this is a worthwhile collection. In the editors' words, this volume is supposed to serve as "an overview of recent research on electoral laws . . . by scholars who have helped shape the field" (p. 1). The volume surely achieves this modest objective. In doing this, the book establishes a broader international and interdisciplinary perspective on the methods of representation.

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Who Profits: Winners, Losers, and Government Regulation. By Robert A. Leone (New York: Basic Books, 1986. xiii, 248p. \$17.95).

The last decade has witnessed a revolution in the regulatory process that rivals the New Deal in scope and impact. For the first time in history, the executive branch has achieved an unprecedented degree of control over the vast body of regulatory activity in both the independent and executive agencies. Initiated by the Carter administration, and accelerated by President Ronald Reagan, the new regulatory process is as different in its treatment of administrative power as were President Roosevelt's initiatives in the 1930s. Now, however, the role of government has changed. In the New Deal period, the president viewed government as a growth industry, to be used as a vehicle for problem solving; while today, White House efforts focus on minimizing government, in order to allow market forces to work more effectively.

Who Profits is an example of the kind of thinking, much in vogue in the late 1970s, that led to this revolution in executive power. It is primarily a study of the negative impacts of regulation on the manufacturing process, using examples that draw heavily on case materials from the Harvard Business School. The author is an economist who understands the problems facing manufacturers suddenly confronted with regulatory decisions that can drive them out of business or cut substantially into their profit margins. He analyzes the impact of regulatory decisions on such factors as differential costs, tracing what happens to affect the final drafting of the bottom line. Especially interesting is his discussion of regulation as a disincentive to new investment; how the existence of a cumbersome regulatory process leads to a bias against building new manufacturing facilities, and affects U.S. competitiveness. Leone's extensive use of the vast literature on the costs of regulation brings this body of work up to date and casts a critical eye on some of its problems.

Actually, *Who Profits* could serve as a road map for what has already transpired in the federal government, although the author does not seem aware of these developments. In that sense, the book seems out of date. The author argues eloquently for the use of economic

analysis in regulatory decision making on the grounds that it does not exist in the federal arena. In fact, economic analysis has been mandated in all major regulatory decisions since 1981 and the promulgation of Executive Order 12291. The order is rigorously enforced by the Office of Management and Budget, and the agencies have heard the message loud and clear. Benefit-cost analysis now supersedes technical and scientific analysis, as well as health, safety, and environmental considerations.

The book's theoretical approach posits a view that looks very much like public-choice theory. Briefly, Leone argues that public-policy decisions have an enormous impact on industry, that there are heavy winners and losers, and that policymakers need to be made more aware of the economic consequences of their decisions. A strong faith in the value of marketplace competition is repeatedly expressed throughout the book. The idea is that the forces of the free market almost always lead to outcomes preferable to government intervention. There is virtually no recognition of the unanticipated negative consequences of untrammeled market forces or of what happens when an unregulated marketplace is overtaken by predators out to destroy competition. Scholars needn't go far to find examples of this outcome: ample evidence exists in the fields of antitrust, airline, trucking, and railroad deregulation.

Another important subtheme emphasizes that regulatory decision makers are unaware of the unanticipated consequences of their decisions. To buttress this argument, the book offers many examples, most of which fall into one category: the impact on a firm or an industry's profit margins. Some of these examples are rich in detail and offer dramatic proof of the costs of regulation. Other examples show inadequate documentation or inaccuracies. The author, in attacking liberals for ignoring the long-term effects of government intervention, writes, "When policy makers for the state of California look at the prospect for a unitary tax. . ." California has not looked at the "prospect" for a unitary tax for a long time; it has had a unitary tax since 1929, repealing it in 1986. When dealing with auto-safety regulation, the book concludes that there are more "cost effective ways of achieving levels of safety benefits than passive restraints." The documentation for this state-

ment is a General Motors report.

The book's strength is its appreciation for some of the economic problems incurred by industry as a result of its relationship to government. Its weakness lies in its treatment of the political and regulatory process, and its lack of understanding of those processes. The argument that public managers "tend to be held accountable almost exclusively to process and rarely to outcome" misses the importance of integrity to the regulatory process: if the process is fair, there is a greater chance that the outcomes will favor the public's interests. This is the basis for the Administrative Procedure Act and for appeals by citizens and corporations who feel that they have been disadvantaged by regulatory decisions.

The author's major concerns are competitive realities, and he deals with these very well. He skillfully disentangles the roles of equity and efficiency, emphasizes the importance of business managers' understanding the regulatory process, and gives solid advice to industry on how to manage the regulatory process in a more sophisticated way. "Take preemptive action," "Take individual action," and "Use information as a weapon," are several suggestions that are offered corporate public-affairs managers. Public managers are advised to manage their data better and to intrude on industry as little as possible.

But regulatory problems have a broader dimension, as practitioners and scholars are beginning to recognize. What happens in the absence of regulation, and what are the long-term costs on this "bottom line" approach? Who suffers? and Who pays in the long run? are questions notably missing from the Who-profits? approach. In fact, new technologies need more government regulation, not less, as their leaders are beginning to recognize: recent efforts by the biotechnology industry to seek additional government regulation bears witness to this change in thinking. That process needs to be fair, cost conscious, and adequate to the task, in order to help industry fulfill its potential and to protect the consumer. And, as government and enlightened business leaders agree, regulators need to be at least equal in strength to the industry they are regulating, not subordinate to the current vogue of market forces.

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Game Theory and Political Theory. By Peter C. Ordeshook (Cambridge: Cambridge University Press, 1986. xv, 511p. \$49.50, cloth; \$17.95, paper).

Many of us have been awaiting publication of Peter Ordeshook's *Game Theory and Political Theory* for quite a few years. Now it has arrived, and the wait has been worthwhile. As the subdiscipline generally known as positive political theory enters its third decade, Ordeshook has crafted what for the time being must be regarded as the definitive survey of the field. The treatment is comprehensive (within the generous limits of five hundred pages) with respect to the theoretical substructure of the field and with respect to applications of that theory to electoral and committee voting processes (though bureaucratic and nondemocratic processes are not covered); up-to-date; and—given the author's many contributions to the literature, both theoretical and experimental, that he is reviewing—distinctly authoritative.

Ordeshook's premise—and what his book demonstrates—is that, "formal models of politics" now form an integrated whole, much like "formal models of markets." It is game theory that unifies the heretofore disparate models into what Ordeshook boldly calls "political theory" (dropping the modifier *positive*). From this premise come both the title and the organization of Ordeshook's book.

Ordeshook's first chapter presents an essentially standard, but exceptionally clear and comprehensive, discussion of individual preference and choice. In the first part of chapter 2, he presents Arrow's impossibility theorem and a variety of other voting paradoxes, but to this point he assumes that electoral institutions mechanically aggregate individual preferences into social choices. In the second part of the chapter, Ordeshook establishes the possibility of strategic manipulation of institutions, first by considering the power of agendas in influencing voting outcomes and second by considering incentives for misrepresentation of preferences under voting procedures and social-choice functions of all types. This sets the stage for the systematic analysis—provided by game theory—of strategic interaction of agents within institutions. The remaining eight chapters then survey standard topics in game theory, but with interesting dif-

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ferences in organization and emphasis compared with the classic texts in the field.

Indeed, Ordehook's book invites comparison with R. Duncan Luce and Howard Raiffa's *Games and Decisions* (New York: Wiley, 1957), which for years has been considered the standard treatment of game theory for social scientists. It would be fair to say the two books are pitched at about the same level of abstraction and difficulty. And Ordehook's writing approaches the standard of clarity set by Luce and Raiffa (quite a compliment). To some degree, Ordehook shares the critical posture of Luce and Raiffa, at least to the extent of regularly pointing out that much conceptual development and research remains to be done in the construction of comprehensive political theory. At the same time, there are major differences between the two books, which derive from two obvious and related facts. The first is the Luce and Raiffa book is now 30 years old; while the main lines of game theory were clearly established when they wrote, there have been many very significant developments in game theory itself (especially new solution concepts) and in its application over the past decades. Second, Luce and Raiffa were not political scientists, and their survey did not focus particularly on applications of game theory to politics. Indeed, apart from a discussion of the Shapley-Shubik power index to weighted voting systems and a brief and unsystematic discussion of strategic aspects of majority rule, their survey includes no distinctively political topics. Of course, at the time they wrote there was little to survey in this respect, while many of the more recent applications of game theory are to political processes.

Ordehook's focus on political applications of game theory leads him to depart from the standard mode of presentation established by Luce and Raiffa. Conventionally, two-person games are considered before n -person games, and zero-sum games before variable-sum games, but Ordehook moves directly (in chap. 3) to consider n -person variable-sum noncooperative games (focusing on voting games in particular). And conventionally, cooperative games are considered under the assumption of transferable utility (generally relevant to economics but not politics), while Ordehook treats transferable utility only as a very special case. Perhaps most important,

Ordehook devotes a great deal of attention (beginning in chap. 1) to spatially defined preferences, on which most models of electoral competition and committee voting are now based. This orientation is completely missing from Luce and Raiffa and most other game-theory texts.

In addition, Ordehook's new book invites comparison with his previous text, coauthored with William Riker, *An Introduction to Positive Political Theory* (Englewood Cliffs, NJ: Prentice-Hall, 1973, now out of print). The new book is no mere updating, as it presents a far more coherent synthesis of the seemingly distinct topics that defined the chapters of the earlier book. And of course the new book reflects the progress of research over the past decade (most notably, perhaps, with respect to the theory of spatial voting games and agenda processes and with respect to experimental testing of solution concepts).

Game Theory and Political Theory will serve two audiences especially well. First, specialists in the area of positive political theory will surely want to read it to see how a leading scholar in the field organizes and presents familiar but difficult material; they will also find it handy reference work with respect to recent and perhaps unfamiliar developments. Second, present nonspecialists who are seriously interested in gaining an understanding of positive political theory will find this an excellent place to start. In particular, the book will serve as a fine text for graduate-level courses in the field. (I doubt it can be successfully used at the undergraduate level). The fact that a literature guide is attached to each chapter together with the fact that Cambridge is publishing the book in a reasonably priced paperback edition, will enhance the book's utility as a course text.

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**Coalitional Behaviour in Theory and Practice:
An Inductive Model for Western Europe.**
Edited by Geoffrey Pridham (Cambridge: Cambridge University Press, 1986. xix, 308p. \$44.50).

The recent revival of interest in government coalitions in parliamentary democracies is in-

disputable. Geoffrey Pridham's recent book underscores the rediscovery of this traditional topic among Western Europeanists. However, this is not another collection of theoretically unconnected, country-specific articles. Nor is it a volume of abstract game-theoretic exercises. On the contrary, the purpose of Pridham's book is to present a sustained critique of, and alternative to, the game-theoretic approach to parliamentary government. Pridham refers to this enterprise as a fresh look at coalition theory in the light of coalition experience (p. xvii) and calls his alternative an inductive theoretical framework within a multidimensional perspective.

This edited volume is the fruit of a workshop held during the sessions of the European Consortium for Political Research at Salzburg in April 1984. The volume is implicitly divided into three parts. The first two chapters (by Pridham and Michael Laver) outline the analytical framework and critique the existing literature. The bulk of the volume (the next nine chapters) consists of country studies more or less committed to the analytical tools Pridham provides in the first chapter. Finally, chapters 12 and 13 explore the applicability of the same tools to coalitions at subnational levels and introduce the reader to a couple of important ongoing research projects. Neither of the latter, however, bears much theoretical resemblance to Pridham's approach.

In the Preface, the editor informs us that the idea for the volume originated in his own work on Italian coalition politics. In fact, Pridham's chapter on Italian coalitions is a good place to begin familiarizing oneself with his approach to coalition formation and maintenance. Although the chapter is neither parsimonious nor particularly elegant, it does demonstrate the relevance of his seven dimensions to the study of coalition politics. More specifically, Pridham shows how the governmental game in Italy is constrained by historical events (such as established coalition formulae), institutional characteristics (e.g., the role of the president), differences in party motivation (patronage vs. policy), local and regional coalition patterns (often precursors of national coalitions), internal party factionalization (most notably among the Christian Democrats), socio-political factors (e.g., declining mass deference and party identification), domestic events (stagflation), and international commitments

(e.g., to NATO and the United States).

Pridham's attempt in the introductory chapter to deal with these constraints analytically is much less persuasive. The discussion is frequently rambling and repetitive. Pridham presents no sustained critique of the deductive theories he opposes, and his own model is not presented with great clarity. His invocation of the concept of dimensionality is confusing. Deductive coalition theories are labelled "one-dimensional," but this somewhat mystifying characterization is never explained to the reader. Nor is it clear what is dimensional about Pridham's own dimensions, which seem more aptly described as constraint categories. Moreover, one must question the editor's contention that his inductive approach "is imperative for handling the complexity of this subject in a viable and comprehensive way" (p. 14). Complex subjects may require complex models, but not necessarily inductive ones. These flaws notwithstanding, Pridham deserves credit for his ambitious attempt to bring a broad range of literature to bear on the subject of coalition formation and maintenance.

The reader is treated to another aperitif in the form of Michael Laver's analytical chapter on deductive models and cabinet coalitions in Europe. This concise, witty, and admirably nontechnical chapter is clearly the best in the book. It should be especially instructive for the intelligent novice to this field of study. Like Pridham, Laver criticizes formal coalition theories for a number of unrealistic assumptions. Foremost among these problems is the static nature of most theories. However, Laver's conclusions are more sanguine, in the sense that he sees more potential for progressive modifications of deductive coalition theory.

Laver is also partly responsible (with Michael D. Higgins) for a very readable and informative chapter on government coalitions in Ireland. The country chapters are on the whole surprisingly even in quality and structure. Certain heterogeneities and imbalances do, however, exist. Two chapters focus on the coalitional behavior of particular parties rather than national party systems. The chapter on France (by Eric C. Browne and Dennis W. Gleiber), on the other hand, scrutinizes a single government (Ramadier) and departs radically from Pridham's multidimensional approach.

Book Reviews: Political Economy

Geographical coverage is, as usual, not strictly determined by theoretical interest. Thus it is difficult to see why Germany, with its paucity of parties and coalitions, deserves two chapters, whereas the Scandinavian countries collectively get the short shrift of one. The latter chapter (by John Fitzmaurice) is in turn internally uneven. Denmark is covered in a much more satisfactory manner than Sweden or (particularly) Norway. On the latter country, Fitzmaurice commits a multitude of embarrassing factual errors, such as misspelling the names of all but 2 parties (out of 11) in the Appendix (pp. 276-77). A positive feature of the country coverage is the inclusion of Spain, despite the limited history of party coalitions in that new democracy.

In sum, Geoffrey Pridham and his collaborators have made a useful, but hardly indispensable, contribution to our understanding of coalition governments. The book may be read for its juxtaposition of Pridham's and Laver's critiques of deductive coalition theory. Some readers may also value the insight they gain concerning ongoing research projects. Yet it is fair to assume that most will find its main contribution in the rich and relatively systematic country-specific treatment of historical, cultural, and institutional constraints on coalition politics.

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Erratum

Francis A. Beer, Alice F. Healy, Grant P. Sinclair, & Lyle Bourne, Jr. "War Cues and Foreign Policy Acts" (September 1987, 701-55). On page 706, the figures should be titled as follows:

Figure 1. Mean Cooperation-Conflict Scale Score ("Conflict Score") as a Function of Dominance Group and Priming Condition

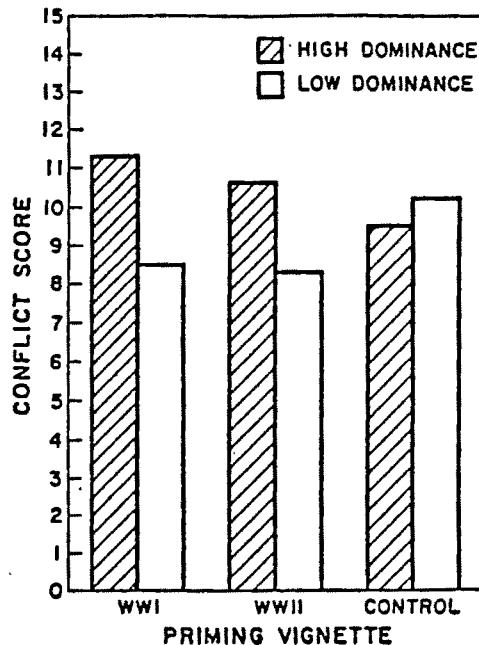
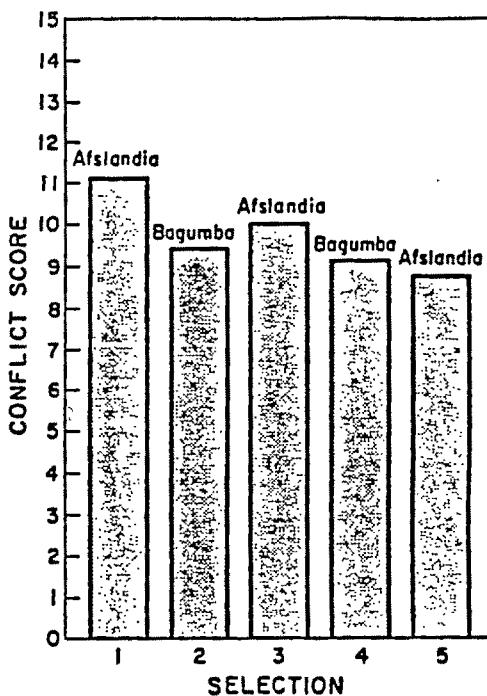


Figure 2. Mean Cooperation-Conflict Scale Score ("Conflict Score") as a Function of Successive Selection



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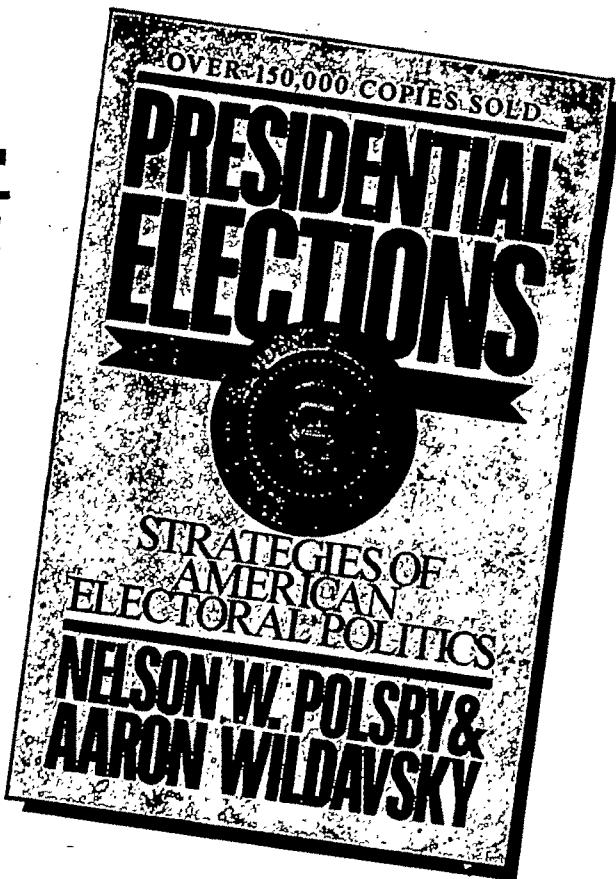
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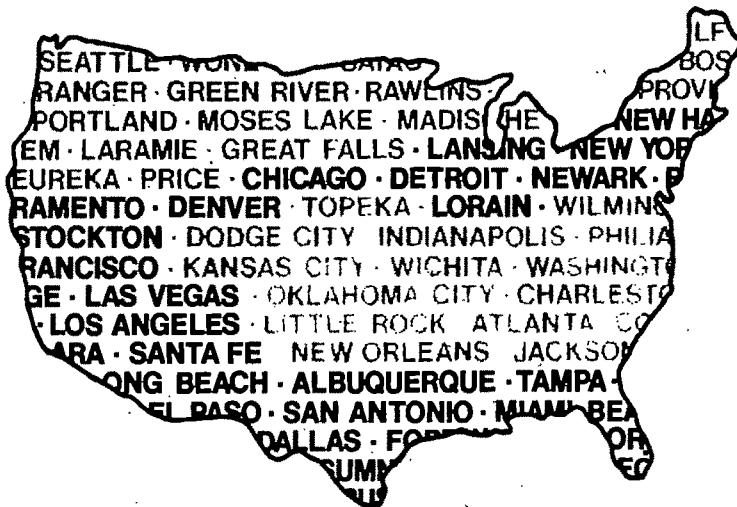
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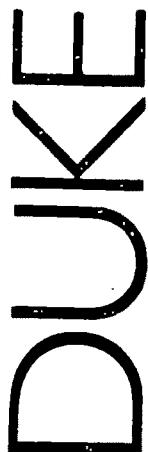
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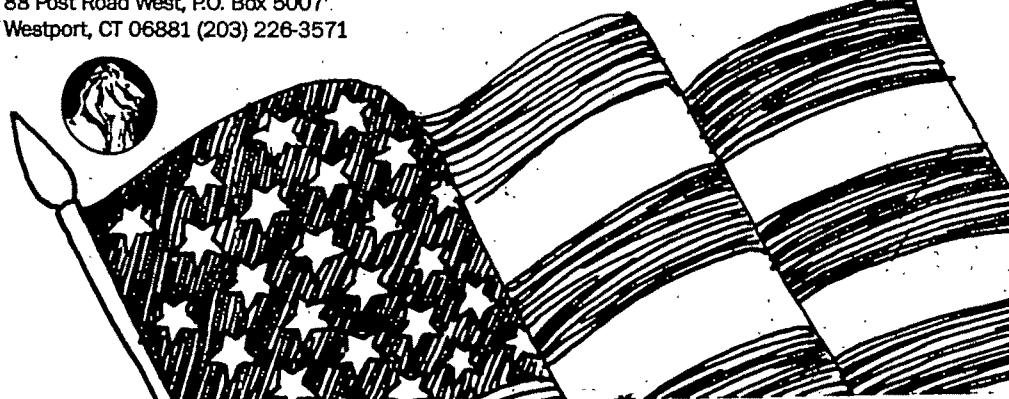
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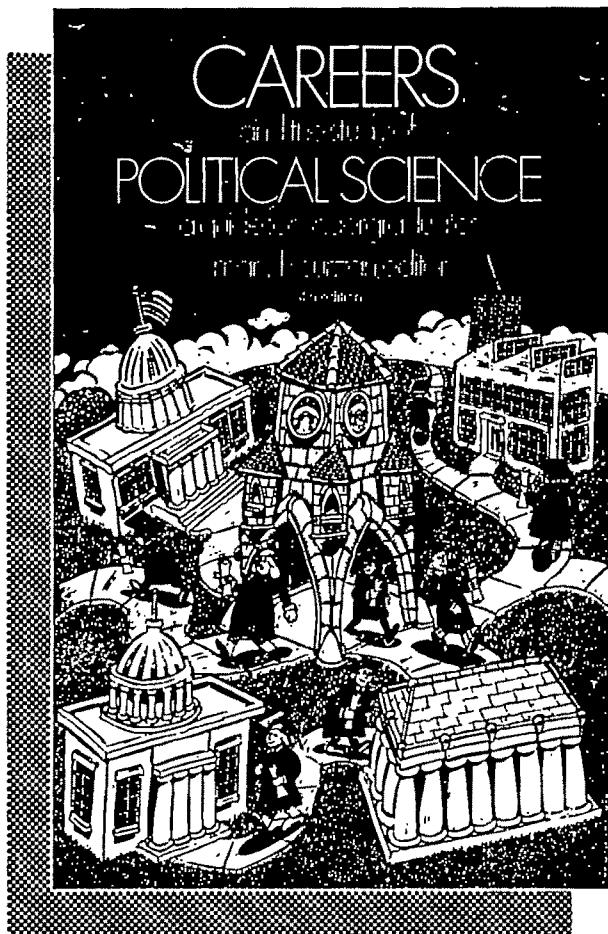
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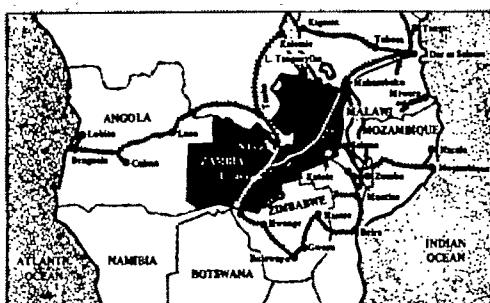
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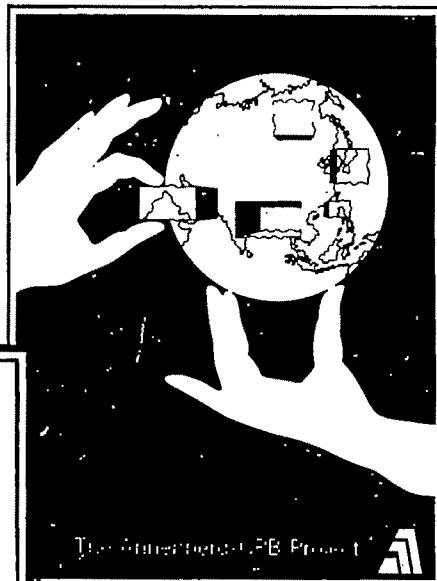
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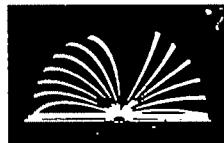
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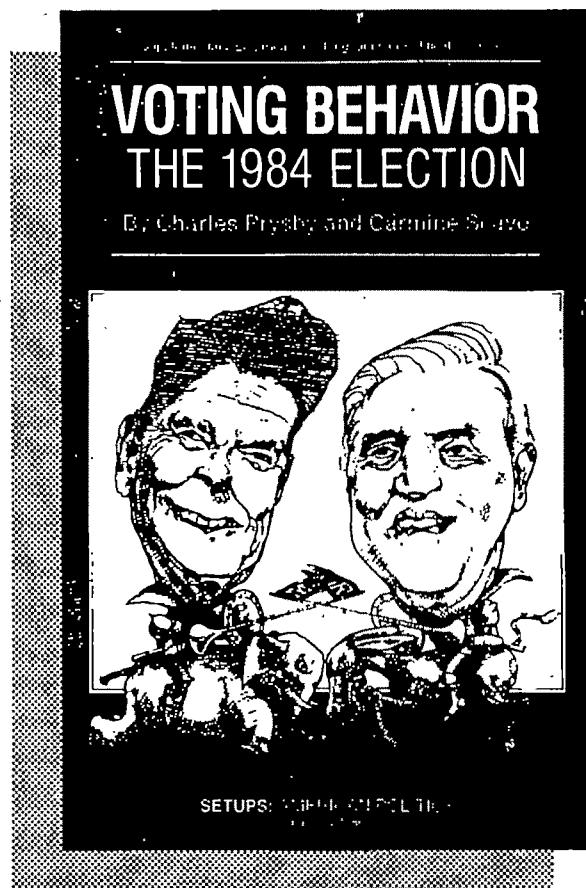
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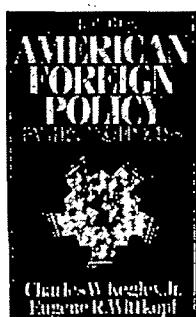
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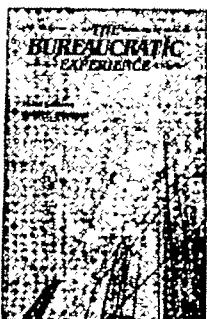
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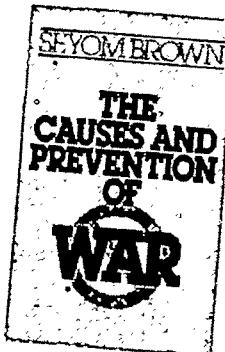
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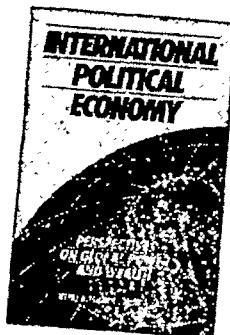
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